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OF

THOMAS CHROWDER CHAMBERLIN

1843-1928

BY

ROLLIN THOMAS CHAMBERLIN

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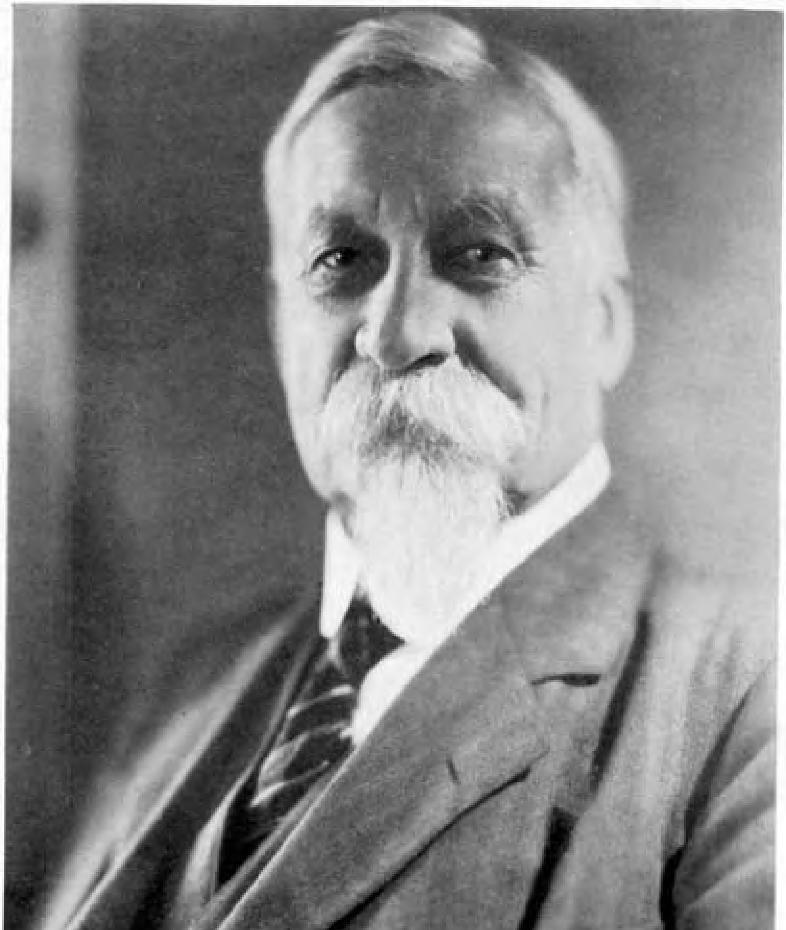

T. C. Chamberlin

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THE ANTECEDENTS

The Chamberlain family has been traced back to the ancient Norman counts of Sankville, knights from which stock took an active part in the conquest of England under William the Conqueror and were repaid for their services with large estates in the counties of Gloucester, Oxford, York, and Warwick. The founder of the family of Chamberlain in England, as an independent branch of the house of Sankville in Normandy, is said by some to have been Richard, "Grand Chamberlain" to King Stephen, who assumed his surname from his office.[1] According to another account, it was the Count of Sankville, chamberlain to King Henry the Second, who gave rise to the name in 1154.

In the early days of the American colonies several members of the Chamberlain family emigrated from England to the new country, among whom was Henry Chamberlain, who came to Hingham, Massachusetts, in 1638 from the parish of Hingham, County Norfolk, England, having crossed the Atlantic in the ship "Diligent," with his wife, his mother, and his two children. He was one of many who, under the leadership of the Rev. Robert Peck, fled from the religious persecution in England at that time. Later this branch of the family was engaged in ship building in New England. One member, Joseph, grandfather of Thomas Chrowder Chamberlin, went to North Carolina in the interest of the family's business, married Sarah Cartwright, and settled there. Ten of their children lived to maturity and evidently prospered, for the Chamberlains are said to have become the richest and most prominent family of Camden County, North Carolina. The second son, John, became convinced early that the system

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[1] During the succeeding centuries over twenty variations in the spelling of the name came to be recognized in England.

of slavery, which played an important part in the plantation life of the time, was an evil one, and because of his strong feeling on the subject he left the slave-holding region for the newer country farther west. He traveled through Tennessee to Palestine, a small settlement on the Wabash River in Illinois. It was here that he met and married Cecilia Gill, a girl of Scotch descent who was born at Lexington in the Blue Grass region of Kentucky. Shortly after their marriage they moved about sixty miles westward. There, on the crest of the Shelbyville terminal moraine, a few miles southwest of the site of the present city of Mattoon, were born the three oldest boys—William Fletcher, John Nelson, and Thomas Chrowder. In this geologic setting, prophetic of his later interests, Thomas appeared on September 25, 1843.

THE BEGINNINGS

When Thomas was about three years old, the family started on a long journey to the wheat country of Wisconsin, traveling, as was the custom of the time, in two "prairie schooners," and camping many nights on the lonely prairie. Family tradition has it that the boy learned his letters on the way. When they reached Beloit, a small settlement just over the Illinois-Wisconsin state line, they found land to their liking and located near a grove on a ridge about four miles northwest of the village. The ridge-top above the rolling prairie was a stimulating environment for the development of a naturalist, for myriads of the now extinct passenger pigeons and countless other birds, each year migrated northward and southward and were set forth sharply on the black stubble left by the fall and spring prairie fires. Often at night wolves came up to the picket fence that surrounded the log cabin and quarreled with the dog till a blast from the old flint-lock shotgun scared them away.

One of Thomas' earliest remembrances was that of his father telling of the "laying of the corner stone," a matter which impressed the boy deeply, though he had not the slightest notion of its meaning. It referred to the founding of Beloit College, in 1847, an institution that had a profound influence on the boy in later years. When he was a small boy of eight a rather prophetic incident occurred. One rainy day when Thomas and his two older brothers were playing together, one of them asked in fun what Thomas was going to do when he grew up. Thomas came back quickly with the reply: "I'm going to school till I can teach the best school in the state," at which the brothers laughed until Thomas became embarrassed. His father caught the answer and said, " Boy, don't you back out. Stick to it!" And Thomas stuck to it.

From the original log cabin the Chamberlin family progressed to a more modern farm house which they built about a mile away. When as a helper to the two older boys, Tommy quarried rock from an outcrop of Trenton limestone he debated with his brothers how the great "snakes" (portions of orthoceratites) could have gotten into the layers of rock and turned to stone. It was easy to see how they got down the crevices, but how could they get in between the layers where the boys found them? So, too, in digging sand for mortar, they found clay pebbles that were soft and could be broken with the fingers. They easily decided that these clay masses were young growing stones that had not yet hardened. This was the first school of geology the boy attended, and his only textbook was Nature's own, written a long time before.

In the course of time, two younger sons, Joseph Hanson and James Alexander, appeared in the Chamberlin family circle. The father, by preference a Methodist minister, by necessity a farmer as well, was known as a "circuit rider" because of his practice of riding around to several different meeting places each Sunday to preach. In time, this overtaxed his strength and eventually undermined his health. Though of limited academic education, he was an incisive thinker and reader in theological-philosophical lines, strict in his views, and greatly given to argument on religious subjects. He was regarded as abler than any of his boys and was long remembered locally as a man of exceptionally clear speech and systematic ideas. Thus the sons grew up in an atmosphere of serious and sharp debate in theology and philosophy.

In succession the five Chamberlin boys, Fletcher, Nelson, Thomas, Joseph, and James, entered Beloit College. The curriculum was wholly prescribed, modeled on the classical courses of the colleges of New England, and on that of Yale in particular. In large part the course was Latin, Greek, and mathematics, though containing some history, philosophy, and science. Thomas was most deeply interested in philosophy and mathematics. Geology he faced with the attitude of a skeptic, for it did not at all agree with his theological beliefs. He determined to investigate the subject in order to show that his theology was right and that the geologists were wrong. So thorough were his investigations that, instead of convincing the geologists of their errors, he convinced himself of the truth of their contentions. When, at the end of the term, he went to his professor to find out his standing in the class, the older man turned and, with a twinkle in his eye, said: "Well, Chamberlin, would you like to know who stood next?" With this encouragement, Thomas became even more interested than before in the subject of geology.

On graduating from Beloit College, in 1866, Thomas Chamberlin was engaged as principal of the Delavan (Wisconsin) High School in which he had been preceded by men of more than ordinary ability and success. The situation was, therefore, highly stimulating to his work. He soon came to feel that the course of study was too narrow and barren to give the young men and women what they ought to know as an equipment for life, particularly in respect to the earth, and the heavens, and to the great events of the past. So he introduced a series of lecturettes on elementary phases of geology, astronomy, and other natural sciences, and occasionally on fine afternoons he took his students on field trips to explore the wonders of the out of doors.

In 1867 Thomas married Alma Isabel Wilson of Beloit, whose gentle, cheerful nature was the basis for the happiest home life and who until her death in 1923 was a great inspiration to him. Many of his early notes and manuscripts are in her clear, beautiful penmanship.

Since his equipment for teaching the innovations which he was introducing was very meagre, Thomas felt keenly the need for a broader foundation in natural science. He also foresaw the coming wave of scientific development. In 1868, therefore, he and his young wife went to Ann Arbor for graduate study in science at the University of Michigan, which seemed to offer its learning for the least money. In 1869, after a year spent on a wide range of studies, including some work in geology under Alexander Winchell, young Chamberlin was called to the "settee" of the natural sciences in the State Normal School at Whitewater, Wisconsin. Though his field included all the usual sciences listed, by concentrating his study on one science after another during a period of two or three years, he became qualified to teach all of them to the extent required for his pupils.

It was here at Whitewater, while inspiring eager young minds with an uncommon enthusiasm for the phenomena of Nature, that he first found himself. In 1920, as a mature scientist, he wrote to his old pupil, Professor L. C. Wooster of the Kansas State Normal School, the following brief letter: "I still look back upon the beginning of my work at Whitewater as among the halcyon days of my life. To be sure, it was a stage of small attainments, but it was as full of endeavor to get on and get more that was worth while as any later days, and the fruits were just as sweet, for no fruits are perfect. They are merely pabulum on which to grow."

Of the results let one speak who came shortly after Professor Chamberlin left Whitewater.[2]

> "When I first entered the school I found that it had a decided bent toward natural science. All the other departments were overshadowed by the predominance of this. The opinion of the teacher in natural science, even on Latin roots, was more highly esteemed than that of any other teacher in the school. Because of this prevalence of the scientific spirit, many a student received such an impulse toward the natural sciences that it developed into a passion, and gave direction to his future career. There has been a larger number of the earlier graduates of this school who have become teachers of science, or who have given unusual attention to some branch of it, than from any of the

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[2] Prof. Lewis H. Clark, '79. Reminiscences of the Faculty. Historical Sketches of the First Quarter-Century of the State Normal School at Whitewater, Wisconsin, 1868-1893, p. 51. Madison, 1893.

other normal schools. I need but mention Professor King of Wisconsin University, Professor Salisbury of Chicago University, Professor Ewing of River Falls Normal School, Professors Culver and Buell of Beloit, Professor Wooster of Kansas, in evidence of this. I have thought that this was due to the fact that Professor T. C. Chamberlin was one of the earliest teachers of natural science in this school and made his department first in the esteem of the students."

THE GEOLOGICAL SURVEY OF WISCONSIN

In the spring of 1873, Governor C. C. Washburn signed a bill providing for a complete geological survey of Wisconsin and appointed T. A. Lapham chief geologist and Roland D. Irving, T. C. Chamberlin, and Moses Strong assistant geologists. To Chamberlin was assigned the southeast section of the state. The iron deposits lay in the central and northern sections, the lead and zinc deposits in the southwestern corner. The member of the legislature who, under Chamberlin's stimulus, had been most active in procuring the passage of the bill which established the Survey, on learning of the appointments of others to the mineral regions, said pointedly: "Mr. Chamberlin, you are shelved. What is there to be found in southeastern Wisconsin?" In reality, there seemed to be little more than the well-known early Paleozoic beds, covered up deeply with glacial drift. The apparently unpromising area practically forced Chamberlin to become a student of glaciation, and it was his studies in this field that eventually led him into the fundamental problems of geology and cosmogony.

From 1876 to the completion of the Survey in 1882, Chamberlin was chief geologist, instructed by the legislature to perform a task the comprehensiveness and difficulty of which may be gathered from the words of the enactment (Chapter 121 of the Laws of 1876):

"The people of the State of Wisconsin, represented in Senate and Assembly, do enact as follows: Section 1. That in the preparation of his final report, the chief geologist be, and he is hereby, authorized to collate the general geology and the leading facts and principles relating to the material resources of the

State, together with practical suggestions as to the methods of detecting and utilizing the same, so as to constitute the material for a volume suited to the wants of explorers, miners, land owners and manufacturers, who use crude native products, and to the needs of the schools of the State, and the masses of intelligent people who are not familiar with the principles of geology; said volume to be written in clear, plain language, with explanations of technical terms, and to be properly illustrated with maps and diagrams, and to be so arranged as to constitute a key to the more perfect understanding of the whole report."

The Survey, and particularly the chief geologist, had a hard task to perform. At that time the physical difficulties alone of so comprehensive a survey were enormous; the time allotted short; the legislature had to be cultivated for appropriations, and a capable group of collaborators had to be assembled and directed. Then, as always throughout his career, Chamberlin gathered around him able associates, among whom were Irving, Strong, Wright, Pumpelly, Sweet, Brooks, Whitefield, Wight, Wooster, King, Van Hise, and Salisbury. It was a time of great activity for the State Geologist, yet in the midst of these labors he served also part of the time as professor in Beloit College, and gave short courses of lectures on the relations of science and religion to audiences which filled the Second Congregational Church of Beloit.

That the Survey was strikingly successful is evidenced by the four grand volumes, "Geology of Wisconsin," in the last of which to appear (Volume I, 1883) the State Geologist carried out in remarkable fashion the difficult requirements imposed by the legislature. These volumes even today are highly prized by the people of Wisconsin and, as models of pioneer geologic research and of clear unfolding of the geology of a state, still stand without a peer.[3]

Chamberlin's scientific contributions ranged widely through many fields, but none was more characteristic of the man than the unraveling of the origin of the glacial deposits, the lead and zinc ores, and the Silurian coral reefs. Though "the survey in

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[3] C. K. Leith, Chamberlin's Work in Wisconsin. Jour. of Geol., Vol. XXXVII (1929), p. 291.

the seventies swept over the Pleistocene formations and those that underlie them at the rate of about 4,000 square miles a year,"[4] Chamberlin's early studies of the Wisconsin Kettle Moraine were so outstanding as to win speedy recognition by geologists. In 1878, he went to Europe to present his results at the International Geological Congress in Paris and to study the workings of existing glaciers in the Alps. Upon returning to Wisconsin, he soon came to recognize that the prominent kettle moraine crossing the state in louped festoons divided an area of later glaciation from that of an earlier one. He thus recognized that the problem was more complex than he had supposed and that it reached beyond the scope of a single state survey.

Soon Chamberlin's Wisconsin glacial studies merged into the broader investigations which he started when he was appointed Chief of the new Glacial Division of the U. S. Geological Survey. Glacial studies at that time were somewhat further advanced in Europe than on this side of the Atlantic, but the penetrating examination of the unsurpassed glacial records left in the Upper Mississippi Valley soon placed American glacial investigations on strong, independent foundations and led to the important later developments.

Who can read the 200 pages in Volume IV, Geology of Wisconsin, upon the lead and zinc ores of the state, without realizing that here is a remarkable piece of work? The technical descriptions are rich in their fullness of significant details, and clear, lucid, and literary in diction, but the competent reader is most deeply impressed by the fertility of Chamberlin in framing and evaluating the possible hypotheses for the origin of the deposits. The "method of the multiple working hypotheses" is already being used. Hypothesis after hypothesis is tested with thoroughness and rejected because of its inconsistency with important facts. The favored view is discussed fully, carrying along several alternative sub-hypotheses, some one of which, or

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[4] T. C. C.: Preface to the "Quaternary Geology of Southeastern Wisconsin," by W. C. Alden, U. S. Geological Survey Professional Paper 106 (1918), p. 13.

some combination of which, seems likely to loom large in the ultimate explanation when that stage shall be reached. The conclusions have been called daring in conception, and so they may seem, but they were the result of a far-seeing vision guided by rigorous logic and a mastery of the facts and principles involved. They have seemed daring largely because transcending the ordinary.

Chamberlin believed that the lead and zinc salts were carried to their present position in solution in descending meteoric waters which had obtained them from a more disseminated distribution in the Ordovician strata of the region. A zone of oxidation lay near the land surface. Below was a zone of "sulphuration" in close relation to shales so rich in bituminous matter that a fragment can be ignited with a match. Here occurred precipitation of the sulphides. Referring to the important percolating waters, he wrote: "That from the zone of oxidation carries in the metallic salts in solution while, below, there enter sulphureted waters and the two, mingling, deposit the ore."[5]

That some experts in ore deposits, coming later, attributed these ores to ascending hot solutions, worried him little, for he had great confidence in the essential correctness of his early work and he explained in a decisive way why the later interpretations seemed to him untenable. In fact, he himself had already discussed these alternative possibilities with great pains and rejected them in his original report.

In Volume II the peculiar dome-like structures in the Niagara limestone at Racine, Wauwatosa, Waukesha, and several other places in southeastern Wisconsin were minutely described and boldly interpreted as ancient coral reefs.[6] Fifty years later, Cummings and Shrock paid the following tribute to this truly surprising work: "Wisconsin has the honor of being the State in which the pre-eminent Chamberlin first accurately described ancient reef structures in America. His diagrams and descriptions (1877) leave little to be desired, and he correctly distin-

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[5] Geology of Wisconsin, Vol. IV, p. 550.

[6] Geology of Wisconsin, Vol. II (1877), pp. 351-371.

guished both the reef and inter-reef lithologic facies and distinct faunas."[7] This remarkable diagnosis was several decades ahead of its time. Many other geologists throughout the whole span of years since 1877 have seen much of these prominent structures in Wisconsin, Illinois, Iowa, and Indiana; but lacking the daring imagination necessary for correctly explaining the various phenomena on exhibition, they have remained either baffled or incredulous of the reef interpretation.

Chamberlin made many more important contributions than that of describing and diagnosing the Niagaran coral reefs, but no single achievement seems more amazing than this early advance into the unknown. The training of the young State Geologist had been limited; he had traveled but little and at that time (1877) had never seen the ocean. Moreover, these quaquaversal structures were to him but minor features in a rapid survey of a large state. Only the true instinct of superior genius could have enabled him in those days to interpret correctly phenomena which have continued to puzzle geologists almost to the present time. Prophetic vision was the most outstanding characteristic of his mind, as was to be shown over and over again in later years.

ON THE UNITED STATES GEOLOGICAL SURVEY

In 1879, the United States Geological Survey was established with Clarence King as director, but the following year the directorship passed to Major J. W. Powell who reorganized the Survey into divisions. The success of the Wisconsin Survey, and particularly the character of its reports, had so strongly impressed Powell that he appointed Chamberlin to take charge of the glacial division of the Federal Survey. In transmitting this appointment on June 17, 1881, Powell gave the following instructions:

"The terminal moraine that enters the United States on the north border of the Territory of Dakota and stretches thence

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[7] E. R. Cummings and R. R. Shrock: Niagaran Coral Reefs of Indiana and Adjacent States and Their Stratigraphic Relations. Bull. Geol. Soc. Amer., Vol. 39 (1928), pp. 579-620.

southward and eastward in sinuous course probably to the Atlantic will be the subject of your investigations for the coming year. You will also study such collateral deposits as may be intimately associated with its history. Your investigation should be devoted principally to that portion lying in the Territory of Dakota and to such other portions as have not hitherto been traced; giving to still other portions the examination that may be necessary to determine the relations of the several parts and the proper delineation of the whole on the map."

Thus commenced Chamberlin's work as geologist in charge of the glacial division of the United States Geological Survey, which continued until 1904 concurrently with many other duties in the later years. Carrying out the original instructions from Major Powell, Chamberlin worked from Montana through to the Atlantic coast. That there was an old glacial drift and a younger glacial drift, which in general did not reach so far south as the deposits of the earlier glaciation, he was quick to recognize, and he made it his task to trace the border of the younger drift across this great stretch of country. Others became associated in this work: J. E. Todd, in Dakota and Iowa; Warren Upham, in Minnesota; L. C. Wooster, in Michigan; and R. D. Salisbury, in New York. Earlier work in Pennsylvania by H. C. Lewis and G. F. Wright; in New Jersey, by G. H. Cook and J. C. Smock; and on Long Island, by W. W. Mather also aided much in the project. But the geologist in charge personally worked out long stretches of the chain, went over other links with his associates, and coordinated the whole into a comprehensive, well-considered report entitled "Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch," which appeared in 1883 in the Third Annual Report of the United States Geological Survey.

Though ever scrupulously careful to give full credit to the work of others, it is apparent that the published report is very largely based on the observations and interpretations of the geologist in charge. Yet so rapidly were these studies completed that the finished report of 107 quarto pages was submitted by the director of the Survey to the Secretary of the Interior as a part of the Third Annual Report on July 1, 1882, scacely a year after the assignment of Chamberlin to the task. The several maps cover the entire glaciated portion of the United States east of Montana, delineating the outer limit of the old drift, the limiting terminal moraine of the young (Wisconsin) ice advance, together with the directions of ice movement within the area of the young drift. Many refinements have since been made in the mapping and we now know that there were four main drift sheets instead of two, but "the importance of this paper was very great at the time of its publication."[8]

The Fifth Annual Report (1883-84) contained one of the classics of geological literature, "The Requisite and Qualifying Conditions of Artesian Wells," by T. C. Chamberlin. Artesian wells had been explained by French and English geologists many years earlier, but there had been no comprehensive analysis of the principles involved to compare with this for practical value. It was one of those publications of broad, fundamental principles and wide application for which the earlier years of the United States Geological Survey were notable and which built up the prestige that the Survey came to enjoy.

The Sixth Annual Report (1884-85) includes a joint paper by T. C. Chamberlin and R. D. Salisbury entitled "The Driftless Area of the Upper Mississippi." Within its 118 pages the reader finds a clear delineation of the characteristic features of the driftless area, compared and contrasted with the dominant features and peculiarities of the adjacent glaciated tracts. There is also an incisive treatment of the residual products of rock weathering characteristic of this area free from glacial scouring. A masterful discussion of the loess and its problems will also impress the reader; but what will strike him most is, no doubt, the thorough-going philosophical handling of the detailed problems, large and small, which combine to make the full story of the driftless area. Many of the conclusions were frankly stated to be tentative, some questions were left open for further development of knowledge, and for some of them the authors later

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[8] W. C. Alden: Thomas Chrowder Chamberlin's Contributions to Glacial Geology. Jour. of Geol., Vol. XXXVII (1929) p. 299.

modified their views; but as a study in the methods of scientific investigation where the problems are complex, this report will make stimulating reading for a long time to come.

At that time Baron von Richthofen's eolian hypothesis of the origin of the loess was contending with the more generally accepted theory of aqueous origin. In the Driftless Area report each hypothesis was confronted with the facts presented by the loess of the Mississippi Valley, and the strong points and weaknesses of each were critically discussed. The aqueous hypothesis met a great difficulty in accounting for the widespread waters required by the distribution of the loess. In their thorough canvass of the possibilities, the authors considered ice dams which might have ponded waters over the loess region; and they enlisted the services of R. S. Woodward to consider mathematically the possibility that the attraction of the ice sheets by tilting the water level changed the drainage so as to furnish the requisite conditions for loess deposition in spite of the lowering of sea level by abstraction of water to furnish the ice of the continental glaciers. They considered also the possibility of crustal deformation due to weighting by the ice burden, and other causes. In the end they came very near to, but did not quite reach, the complete interpretation of the loess of this region as glaciofluvial silts swept up by winds from floodplains of streams from the melting ice and deposited widely. This theory, as set forth by the senior author in 1900, has hardly been improved upon by thirty years of subsequent study.

The assigned reasons for the driftless area in this location stand today just as they were outlined in the "Sixth Annual Report." To one familiar with the peculiar situation of the driftless area between tongues from the Labradorean and Keewatin ice sheets and with the well-known "Jardin," in the Talèfre basin of the French Alps near Chamonix, the poetical finishing sentence of the report is singularly full of meaning. "Diverted by highlands, led away by valleys, consumed by wastage where weak, self-perpetuated where strong, the fingers of the mer de glace closed around the ancient Jardin of the Upper Mississippi Valley, but failed to close upon it."

'The Rock-Scorings of the Great Ice Invasions" was a notable contribution which came out in the "Seventh Annual Report" (1888) after its author had become President of the University of Wisconsin. In an exhaustive study of the phenomena of striation, plucking, grooving, fluting, chatter marks, crescentic gouges and polishing, the records left on rock surfaces by glaciers were described and interpreted, and the inferences deducible from them as to the nature of glacier movement and its effects on underlying surfaces were developed in detail. Today, perhaps, we may be inclined to regard these features as of minor importance; certainly they were very simple compared with the profound philosophical studies into which the author was led in his later years. Yet his powers of observation and analysis and his capacity for taking pains are here revealed just as truly in this handling of small problems as in anything which he ever did. The accompanying glacial map of the United States, portraying the area of the earlier drift and that of the later drift, with the directions of striae for the eastern half of the country, records faithfully the state of knowledge in 1886.

The period of Chamberlin's greatest activity on the Survey closed in 1887 when he went to Madison to take up executive work as President of the University of Wisconsin. The heavy administrative burdens at Madison, and later the increasing diversity of interests at Chicago, left much less time for active field work, but by means of frequent conferences he continued to direct the work of the glacial division. He had trained several of his colleagues in these investigations, giving them the working criteria which they carried out with fidelity and painstaking care under his leadership. Thus it is, perhaps, only fair to state that much of the constructive synthetic thought and general interpretations which appeared in the glacial publications of the United States Geological Survey up to his retirement from general charge of the division in 1904, and for several years thereafter, were due directly or indirectly to T. C. Chamberlin.

PRESIDENT OF THE UNIVERSITY OF WISCONSIN

In the spring of 1885 Chamberlin had been offered tentatively the presidency of the University of Wisconsin, but he preferred to continue in his chosen field of geology. A second approach met with no more favorable response. The Board of Regents of the University, however, continued to press him, urging that the State of Wisconsin had given him his start and that his duty to the state lay in accepting the presidency. This to him was indeed an appeal hard to resist, and he was formally elected to the office by the Board of Regents on June 23, 1886. A full year, however, was granted for completing as much as possible of the Survey work.

When Chamberlin became President of the University of Wisconsin, in the early fall of 1887, it was still essentially a college. President Bascom had been a remarkable teacher and had brought from Williams College much of the spirit and tradition of a New England college. But a new type of educational institution was then coming into prominence with Johns Hopkins and Cornell as conspicuous leaders. The new president was young, vigorous, full of ideas, to a certain extent self-made and unhampered by tradition, and quick to see the best in new developments. The situation at Wisconsin was then favorable for educational advancement. New buildings to take care of the more pressing needs had recently been provided. Various other desirable but less urgent improvements could wait. Promptly and boldly he seized the opportunity to initiate a radical reorganization of the university.

So well has the nature of this reorganization been presented by Pyre in his excellent book on the University of Wisconsin that one can hardly do better than to quote extensively from it:

" 'The University of Wisconsin is in that transitional period in which it is easy to go either backward or forward,' wrote John Bascom, in his farewell *Report* to the Board of Regents. There was no doubt of the direction which President Chamberlin chose to pursue. With clearly defined purpose, he set about advancing the institution to a university basis. More distinctly than any predecessor he understood the university in three dimensions. He saw at once the opportunity to extend it both in area and in depth. More distinctly than any predecessor, also, he grasped at once the triple function of the university, that of undergraduate and professional instruction, that of original investigation combined with graduate teaching, and that of "university extension," or popular dissemination of the essentials of advanced knowledge. Of these, the distinctively university function, he saw, was that of individual research and leadership in original study, and his dominant interest was in the development of the institution in this plane. But before this could be accomplished in any notable degree, the spirit of investigation must be conveyed into the undergraduate courses so that they might lead up naturally, not in information only, but in method, to work of the distinctive university type. In lateral extension of the curriculum, therefore, favor was shown to subjects of study which lent themselves to modern methods of investigation and teaching; or which, as in the technical departments, were most forward in the application of science to industry. Of several departments established at this time, it could be said, and was, that their counterparts had not previously existed in any university. In order to facilitate all of these activities it was desirable that the university should be organized on a more logical and workable plan."[9]

At that time the Agricultural College was an experimental station with an occasional student. The Engineering School had departments, and the last years of the engineering work were separated from the general scientific work of the college. On the other hand, the College of Letters and the College of Arts overlapped. The nominal divisions of the University did not coincide with the actual alignment of interests. Quoting again from Pyre:

"Accordingly an act of legislature was procured in 1889, whereby the old organization was abolished and the University was reconstituted into the four colleges of Letters and Science, of Agriculture, of Engineering, and of Law. All the departments of pure knowledge and investigation, including the physical sciences, were associated together in a single college, with complete autonomy as to its internal affairs; each of the more powerful divisions of professional or applied knowledge was erected into an independent organization with which the central

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[9] J. F. A. Pyre: Wisconsin (1920) pp. 258-259.

college might have such relations as should seem, at any time, reciprocally advantageous."[10]

Thus commenced the system of organization now in operation at the State University.

"The development of the special colleges constituted one phase, and an important one, of the transition from college to university. Equally fundamental were the changes wrought in the central College of Letters and Science. Here again the University is indebted to the initiative of President Chamberlin. Had he, in his sympathy with natural science, elected to give the institution a powerful impetus in this direction, it could not have been considered remarkable. Nearly half a million dollars had just been expended by the State for the new group of science buildings and the nucleus of a strong scientific faculty had already been collected. The opportunity for a special development was obvious. Nothing in Chamberlin's administration, therefore, is more noteworthy than the largeness of spirit in which he set about to strengthen the humanities. But he saw with great clearness that, if the humanities were to be maintained in competition with science, they must be reanimated by an infusion of as much of the modern spirit as was compatible with their nature. 'The remarkable advance which the natural sciences have made in recent years as educational factors,' he wrote in his earliest *Report,* 'has been dependent very largely upon the laboratory and field methods which have given them vitality and effectiveness. Parallel methods in other departments of study undoubtedly mark a coming era of vigorous growth and commanding influence.' "[11]

"For the purpose of permitting greater concentration, continuity, and thoroughness in the leading lines of study" [12] the Group System was installed, whereby the first two years of the undergraduate work were devoted to the acquisition of a general education through a basal group of required subjects, to be followed in the junior and senior years by a "major" line of study. The latter was introduced not as a requirement, but as optional. The student was free to follow what was called the Course System, which was substantially what the University had, or he could follow the Group System in which he would con-

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[10] Pyre: op. cit. p. 259.

[11] Pyre: op. cit. pp. 280-281.

[12] Pyre: op. cit. p. 282.

centrate his work more and more in the last two years of the course. The announced purpose of this system was "to introduce university methods, in the modern sense of the term, more largely in the undergraduate college course, and so prepare the way for the better development of graduate work." [13]

"Then he hoped to concentrate the informational aspects of education, the part of education which consists in accumulating a reasonable amount of handy information that belongs to a college man, by a system of short lecture courses known as Synoptic Lectures. It was a fine idea, and about the only phase of his work that has never been adequately developed. If he had stayed here, he would probably have developed that, but it took a genius and a great deal of energy to develop an idea so novel. The modern form of it appears in numerous orientation and adjustment courses that we see in colleges, none of which looks as attractive in many ways as Chamberlin's original ideas." [14]

"The introduction of the Group System necessarily had two effects. One was that it brought about the introduction of a considerable number of elective studies for the advanced undergraduate students, and the other that it led inevitably to the continuing of this advanced work as graduate study; so that the next thing which Chamberlin did was to begin the formal development of graduate study. He did that in three ways; in the first place, by encouraging the offering of courses; in the second place he established graduate fellowships for the first time in the history of the University; and in the last year of his administration he organized the School of Economics, Political Science, and History, and brought Dr. Richard T. Ely from Johns Hopkins here to head it; and that was very definitely to have graduate study as the primary object. Of course this movement toward graduate work antedated the founding of the University of Chicago, and I do not believe that any university in the Middle West was moving specifically toward graduate study in the way that we were during the early part of Chamberlin's administration." [15]

He found a strong faculty when he came, and he added greatly to it. Very early he called Joseph Jastrow from Johns Hopkins to undertake the establishment of a laboratory of psychology. At that time the only well-established psychological laboratory

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[13] Pyre: op. cit. p. 283.

[14] Oral statement of C. S. Slichter to J. V. Nash, Dec. 18, 1928.

[15] Oral statement by E. A. Birge to J. V. Nash, Dec. 18, 1928.

was at Johns Hopkins, though the year before Cattell had started his work at the University of Pennsylvania. This innovation at Wisconsin was not only the first of its kind in the Middle West, but also the first connected with a state university. Other advances soon followed: "Wisconsin was the first state in America to give a short course in Agriculture in her university. It had for its purpose the calling of young men from the farms to Madison during the winter months and training them so they could pursue a line of farming which would be more remunerative and on a much higher plane than that which then existed in the state." [16] It was largely through President Chamberlin that this course became a reality.

One of Chamberlin's early students at Whitewater Normal School had been Franklin King, who would have been dropped from the school for unsatisfactory work but for the insistence of his natural science teacher, who saw promise in him. Later at Madison he appointed King to a professorship in agricultural physics—the first chair of that kind in existence. Even to this day (long after his death) the work of King is recognized the world over. "When Sir John Russell, of the Rothamstead Agricultural Station in England, visited Wisconsin he put first the work in soil physics started by King." [17]

Building up the School of Engineering was a project into which Chamberlin threw himself with much enthusiasm. For this he gathered together a very eminent staff, only to lose several of them to Leland Stanford University just before he himself went to Chicago. President Jordan was said often to have remarked that he waited to see who Chamberlin called to his faculty and then took them away with higher salaries. The disappointment over losing the pick of his engineering staff was a factor in Chamberlin's accepting the Chicago post.

In his last year at Madison, Chamberlin launched another bold project. Recognizing that no university can be great in all lines of thought but should concentrate upon certain selected lines, he decided to emphasize economics, political science, and

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[16] Ransome A. Moore: Hoards Dairyman. October 10, 1928, p. 876.

[17] C. S. Slichter: Statement to J. V. Nash. December 18, 1928.

history. Plans for a School of Economics, Political Science, and History were developed and approved by the Board of Regents, thus making it possible to give greater opportunities for graduate work and research within that unit than in the University as a whole. Because of the unusual opportunities offered for a powerful development of graduate work, he was able to secure as director of the new school, Dr. Richard T. Ely, head of the Department of Economics at Johns Hopkins University. Ely came to Wisconsin just as Chamberlin left for Chicago, but the new movement was already well under way and was destined before long to make Wisconsin great in economics, political science, and history.

> "It was, however, characteristic of Chamberlin that he never 'played his work up' so as to make his plans seem greater than they really were. He did not minimize their importance but he refrained even from indicating the results which the future might bring out of them. In this case he did not talk of founding a graduate school; he knew that neither the State nor the University was ready for such a movement. But just as he began his work at Wisconsin by encouraging graduate study through fellowships and by the offering of graduate courses in several departments, so the significant step which came at the close of his administration was the founding of this School which should be more specifically devoted to graduate study.
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> "This developmental method marked all of his administration. Chamberlin very definitely united his forward movements to the past history and the present state of the University. He did not desire revolution either in fact or in form. He advanced the University along the lines of development which, as he saw, were those that belong to a midwestern state university and were equally those along which the University of Wisconsin was ready to move." [18]

At Chamberlin's final commencement in 1892, the University for the first time granted the degree of Doctor of Philosophy for graduate work pursued at the University, although the degree had been conferred, *honoris causa,* before. It is interesting to note that the first man to receive this degree was Charles R. Van Hise, later (1903-1918) President of the University.

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[18] E. A. Birge: Statement to J. V. Nash.

In leading the institution along the path of progress from what was essentially a college into a university both in organization and in spirit, President Chamberlin had moved steadily forward in a series of successful achievements. His experience as State Geologist had given him a wide knowledge and understanding of legislatures. He had come to know that in dealing with legislatures everything is likely to go well at first, but that later the ship seems to scrape on the bottom. When that happened Chamberlin knew exactly what to do, rather by figuring it out than by instinct, for he did not have the instinct of a politician. Understanding the way in which things were likely to go, he was never surprised and did not have to improvise, which made him probably the University's most successful president in getting appropriation bills through the legislature. He knew likewise how to move in order to get measures through the faculty as well as through the Board of Regents.

For all of the larger sides of administration, according to Dr. Birge, Chamberlin was a very exceptional man. But the very qualities which insured success in these larger affairs were a hindrance to him in many small matters. He was likely to ponder over petty matters as though they were important scientific questions, whereas when a student wants to know whether he can be excused from class on Friday, so as to go home, he wants an answer, "yes" or "no," right away. Minor details piled up on Chamberlin and worried him and made the administrative work more of a burden to him than it should have been.

In 1891, when President William R. Harper was gathering together outstanding men for the faculty of the new University of Chicago, he chanced to learn that President Chamberlin might welcome a return to his scientific work, and he promptly offered Chamberlin the headship of the department of geology to be organized at Chicago. The opportunity seemed golden and Chamberlin felt that he had earned the right to accept it. In five years as president he had fulfilled his obligations and duty to the State of Wisconsin by reorganizing the University and preparing it for an even more rapid advance to follow. With a deep feeling of relief at being free from the burdens of the University presidency, he turned hopefully toward Chicago. He had been "a pioneer in the educational field, who saw the possibilities which seemed visionary at the time, but which he fortunately lived long enough to see realized and justified. The remarkable development of the University of Wisconsin which he lived to see in other hands justified his early anticipations; and it was a particular source of gratification to him that a part of this program was realized through the activities of Van Hise, whose career in geology, as well as his administrative policies, was so largely influenced by the example of his master, Chamberlin." [19] The work had been well done. "No quinquennial in the history of the University has yielded results greater, more permanent, or more beneficent," wrote President Emeritus Birge.[20]

AT THE NEW UNIVERSITY

President Harper gathered his faculty for the new University of Chicago from the four quarters of the earth. Rarely, if ever, at the inauguration of an institution of learning, had there been brought together men of such varied academic experience and such diverse points of view. Chamberlin came because he welcomed the relief from the burdens of administrative work, but despite the fact that he had resigned the presidency of the University of Wisconsin, when it was in a flourishing state and in the face of a unanimous petition of the faculty and students that he should remain, in order to return to scientific research, he was unable to keep free from participation in the organization of the new University during its earlier years. He felt forced to accept for a time the deanship of a scientific section, the Directorship of the University Museums, and other duties in addition to the development of the Department of Geology. He became the acknowledged leader of those who urged greater freedom for the sciences and less requirements for the classics, though he favored the full development and free election of the latter.

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[19] Joseph Jastrow: Letter to J. V. Nash in 1928.

[20] Historical Sketches of Beloit College. Edward Dwight Eaton (1928), p. 253.

In those days Latin and Greek were the backbone of a general college course, and on them the student was required to focus much of his attention. President Harper's own scholarly achievements (phenomenally great for so young a man) had been in the field of the classics, and he had had little contact with the growing field of science. The University of Chicago started with what seemed to Chamberlin an overemphasis on the classics. He himself in preparatory school and college had studied Latin for eight years and Greek for five. Fully appreciating the classics, he was in no way opposed to their full development and the free election of these studies by those so inclined, but he did feel most strongly that, with the rapid extension of the domain of science, students entering the scientific fields were greatly hampered by excessive requirements of Latin and Greek. Thus started the memorable succession of debates in faculty meetings principally with Professors William Gardner Hale, of the Latin Department, and Paul Shorey, of the Greek Department, on "the Latin question." The faculty naturally was divided in sympathy, and the contest was long-continued and severe. Science gradually emerged victor in this struggle, and the vigorous efforts of Chamberlin paved the way for the great expansion of the University in scientific lines.

The University inaugurated the four-quarter system, instead of the usual two semesters, and at the close of the Winter Quarter in 1893 Chamberlin gave the University's second Convocation address. To add momentum to the movement which he was championing, he chose as his subject: "The Mission of the Scientific Spirit." Two paragraphs of this address which introduce the scientific spirit may be quoted:

"In the architecture of science, every beam is to be tested, every joint is to be put to trial. Conjectures, assertions, opinions, current impressions, preconceived notions, accepted doctrines, all alike are pushed aside to give free scope to untrammelled induction from carefully sifted evidence. The supreme endeavor is to present a disposition of fairness and openness to all evidence and all inductions. Whatever evidence demands, that it accepts. Whichever way the balance of evidence inclines, to that it leans. There is no resistance to the leadings of evidence, there is no pressing of evidence to give it greater or less than its intrinsic weight. All lines of inquiry are pursued with equal zest. All phenomena are welcomed with equal cordiality. The mind opens itself on all sides to every avenue of truth with equal impartiality.

"When demonstrative realities are brought forth they are embraced to the exclusion of all else. They displace all preconceptions, all deductions from general postulates, all favorite theories. The dearest doctrines, the most fascinating hypotheses, the most cherished creations of the imagination or of the reason are cast aside that the new light may freely enter and illuminate the mind. Previous intellectual affections are crushed without hesitation and without remorse. Demonstrative facts are placed before reasonings and before ideals, even though the reasonings and the ideals seem, from previous bias, to be more beautiful, to be more lofty, yea, even though they should seem for the time, until the clearer vision comes, to be truer. That which at the first seems absurd, that which for the time seems impossible, still ofttimes proves to be true in the light of a rectified vision of real relations. And so, the scientific spirit prompts to the acceptance of duly determined facts however they may accord with preconceived standards."

Farther on we read:

"The most significant and prophetic fact in the history of material studies is the tendency to the dematerialization of matter. Starting with the common notion of the fixity, inertness, and passivity of matter, it was discovered, step by step, that activity was a more and more pervading and potential characteristic of all forms of matter from the atom to the falsely-fixed star. It was early discovered that some of the very elements of fixity were but expressions of motion. The gyroscope was a key that unlocked a wondrous treasure-house of hidden truth. As research advanced the functions attributable to fixed solid matter diminished, until the greatest living English-speaking physicist felt warranted in advancing a kinetic theory of matter; a theory which almost completely eliminates matter as ordinarily conceived, leaving only an ulterior, fluidal, ethereal entity—for I scarcely dare say matter—which has become a basal concept in modern ultra-physics. Vortices of intense, incessant, irrotational motion are conceived to be the real units of physical phenomena. Towards this view evidence and opinion seem tending. It is not demonstrative, but it seems a prophetic vision in the line of coming truth. And so it appears but a prolongation of intellectual perspective to anticipate the entire elimination of the current view of matter

> as a true concept and the substitution therefor of inconceivably intense activity in a basal entity whose characters are just beginning to disclose themselves. Certain at least it seems that the future concept of matter must have as its central factor inexpressible intensity and refinement of activity. Certain it seems a picturing of the real constitution of matter must be antipodal to what we have inherited, must awaken higher sentiments and call forth the utmost resources of the true imagination."

This was in 1893: quite different was the view frequently expressed in the years immediately following, that "future advances in physics will be largely in the fifth decimal place."

In turn there were treated biologic investigations, historical inquiries, science and volition, the new humanities, philological research, literary development and religious phenomena, to bring home to the university community a realization that the scientific spirit is ideally and truly a vital factor in all advanced human thought.

> "The curtains of the heavens have been folded up and laid away as the garments of our children; as things loved but outgrown. Olympus is gone. Milton's Cosmos equally with his chaos is only a picture of the past. The richest imagery of all past literature has lost its power save as a glory of the past. And this simply because it was not true. The heavens are not as they were imagined. The beauty of thought does not make it true. The loveliness of thought does not make it immortal. Only the true is enduring. We shall still love these literary myths as marvelous products of days and conditions that are gone. They rightly teach us as all past life productions teach the appreciative soul. Rightly viewed their value is even heightened by the very fact that their day is gone to return no more. The bone that lies in the gutter is a matter for the scavenger. The bone embedded in the Cambrian shales is beyond price. And so it is with the literature that marks the evolution of the thoughts and feelings of the ages. As products of the past their value is beyond estimate. As factors of present and future creations they have lost their potency."

With the beginning of 1893, three months after the University opened its doors to students, appeared the first number of the *Journal of Geology*. Its announced purpose was to serve as a medium of publication for the results of researches connected with the department, to bring to the University and its constituency some of the products of leading geologists in this and other countries, and to be a means of intercommunication between investigators throughout the world. The breadth of plan at the outset may be gathered from the imposing list of associate editors from outside the University—Archibald Geikie, Hans Reusch, Charles Barrois, Albrecht Penck, Gerard DeGeer, G. M. Dawson, O. A. Derby, Joseph LeConte, G. K. Gilbert, H. S. Williams, J. C. Branner, G. H. Williams, and I. C. Russell. The opening article of Volume I was "On the Pre-Cambrian Rocks of the British Isles," by Sir Archibald Geikie. Followed by other noteworthy contributions, reviews and editorials, the initial number of the *Journal,* in spite of the scant time available amid the chaos of the University's first quarter, compares favorably with any later issue.

To keep the *Journal* at the high level of its beginning imposed a severe task on the editor in chief, particularly in the earlier years. Contributions of desirable manuscript sometimes fell short of filling the numbers and more material was needed. A series of essays under the heading of "Studies for Students" was accordingly inaugurated to fill gaps when required. To this series both Chamberlin and Salisbury contributed from time to time descriptive and expository studies which presented in model form the latest developments of knowledge and the best thought of the times along certain lines which they selected. Though modestly addressed to students, some of these "studies" proved to be of lasting value. In this unpretentious series appeared "The Method of Multiple Working Hypotheses." So steady has been the stream of requests for copies of this little essay during the thirty-four years since it first did its duty, that it has recently (1931) been reprinted in the *Journal of Geology* in its original form.

Later, with the growth of the science, there was no shortage of manuscript and Chamberlin's own productions were oft held back to give precedence to the work of others.

PEARY AUXILIARY EXPEDITION TO GREENLAND

Before Lieutenant Robert E. Peary left the United States in 1893 on his third trip to Greenland in quest of the North Pole, he made arrangements for an auxiliary expedition to join him the following summer and furnish his party with additional supplies should it remain for another year's work, or offer it a means of return should its work be finished. On July 7, 1894, the Peary Auxiliary Expedition, comprising Henry G. Bryant, commander; William Libbey, geographer; T. C. Chamberlin, geologist; Axel Ohlin, of Sweden, zoologist; Emil Diebitsch, civil engineer; H. L. Bridgman, historian; and H. E. Wetherill, surgeon; sailed from St. Johns, Newfoundland, in the *Falcon,* a small but staunch sealing steamer specially fitted for ice work.

Early on the morning of the 12th of July, Cape Desolation was sighted. The first glimpse of the topography of Greenland was full of suggestions. The coastal mountains were rugged in outline and rough and angular in detail. Flowing contours scarcely entered into the general view. The dominant lines of sculpture ran in vertical rather than horizontal courses, and led to the inference that they owe their fashioning to the familiar meteoric agencies that work vertically rather than to the horizontal rasping of ice flowing out from the Greenland ice cap. The bearing of this angular topography upon the problem of the former extension of the ice, and especially upon the broad question whether the ice sheet of Greenland once crossed the Baffin basin and became the source of the North American glaciation, as held by some geologists, at once presented itself and stimulated a careful scrutiny of the topography throughout the whole length of coast northward to Peary's headquarters on Inglefield Gulf.

As the *Falcon* steamed slowly northward it was found, however, that angularity was not a universal feature, but that it alternated with very considerable tracts of flowing outlines of glaciated aspect. In fact, about one-half of the western coast of Greenland between Cape Desolation and Inglefield Gulf, a stretch of some seventeen degrees of latitude, presented angular and apparently unglaciated contours, whereas the other half presented subdued outlines indicating former glaciation. North of Cape York, Chamberlin found two islands of such significantly different configuration, though composed of almost identical gneissic rock, that they have since become the familiar textbook illustrations of a rugged conical island before and after an ice sheet has passed over it. Dalrymple Rock, near the Greenland coast about 77° north, is exceptionally jagged and rugose, with no signs of glacial abrasion either in its general configuration or in its surface details. Quite in contrast are the Carey Islands out in the middle of North Baffin Bay, thirty miles to the west northwest, which have been worn smooth in outline. Striae on their scoured surfaces show glacier movement from the north coming from Smith Sound rather than from the Greenland coast. On Southeastern Carey Island were also found erratics of limestone, sandstone, shale, and quartzite wholly unlike anything seen in the islands themselves or known to occur on the mainland of Greenland to the eastward, but such as might apparently have come from the sedimentary series of Grinnell Land and the extreme northwestern coast of Greenland. The natural pathway of the ice bearing these boulders would be through Kennedy Channel, Kane Basin, and Smith Sound. But that ice did not extend as far as Dalrymple Island near the Greenland shore.

Later a small driftless area was found near Peary's camp on Bowdoin Bay, indicating that the former glaciation in this region did not extend greatly beyond the limits of the present glaciers. From these and other lines of evidence Chamberlin came to the conclusion that, although the ice of Greenland formerly extended somewhat beyond its present limits, it did not have any great amplification during the glacial epoch, at least in a westerly direction. Greenland was not the source of the vast ice sheets which spread over much of northern North America during Pleistocene times, as many had previously thought.

After many struggles with the Baffin Bay ice pack, at times following narrow lanes of open water amid the floe ice, at other times crashing full speed into the ice to split it asunder, the *Falcon,* on July 25, reached Murchison Sound, the northern entrance to Inglefield Gulf. Peary's headquarters were at the head of Bowdoin Bay some thirty-five miles up the Gulf, but the ice pack was late in breaking up that summer and a week elapsed before the camp was finally reached.

In their attempt to cross the great ice cap to the northern end of Greenland, Lieutenant Peary's party had encountered a succession of terrific storms, which, together with the death of many of the dogs drawing the sleds, had forced a return to the base camp and the abandonment of the project for the year. Although most of his party were greatly discouraged, Peary himself was undaunted and decided to pass another winter on Bowdoin Bay with a few of his stouter-hearted companions and to try again to cross the ice in the following spring. After unloading its supplies the *Falcon* left for three weeks of exploration along the coast of Ellesmere Land, before starting back south. Chamberlin thus had three weeks for a study of the glaciers of the neighborhood under a never-setting sun.

The results of this strenuous work have been presented as "Glacial Studies in Greenland," in the *Journal of Geology;* in his presidential address before the Geological Society of America in 1895, and later in more scattered form in textbooks. Glaciers in temperate latitudes, as is well known, end in slopes of moderate declivity, thinning and disappearing at their margins as a consequence chiefly of progressive surface melting. Chamberlin found that this habit prevails with the Greenland glaciers as far north as Disco Island and even somewhat beyond. But in Inglefield Gulf, between the latitudes of 77 and 78 degrees, the glaciers end in abrupt terminal walls rising to heights of 50 to 150 feet. This peculiarity he interpreted as being due to the fact that in this high latitude the sun's rays are at a low angle and are directed against the ice from all points of the compass during the continuous daylight of the summer.

Chamberlin described the pronounced stratification of the ice as the most impressive feature displayed in the vertical sides of these high-latitude glaciers. As contrasted with the glaciers of the Alps, the remarkable extent, the definiteness, and the peculiar characteristics of the stratification displayed in the vertical walls of these northern glaciers were a veritable revelation to him. On the vertical ice faces there were characteristically two notable zones, an upper inconspicuously laminated one of nearly pure white ice, and a lower one whose laminated structure was made very conspicuous by the presence, along very numerous parting surfaces, of rocky débris ranging from fine silt to boulders. This débris was in very definite and limited horizons, leaving the ice clean and pure above and below. In general, the abundant débris was confined to the lower fifty or seventy-five feet of the glacier.

The question of foremost importance was the origin of the stratification, or foliation, and the method of introduction of the débris. "The original stratification could not have been very pronounced. Perhaps it was intensified somewhat during subsequent consolidation, but some new agency was necessary to produce the more definite partings and to introduce the layers of débris. This agency appears to have been a shearing movement between the layers."[21]

On almost every one of the vertical faces certain layers were found to jut out sharply over those beneath. The phenomenon suggested differential movement between layers, but the possibility that it had resulted instead from differences in melting raised doubts as to the truth of the explanation. More direct proof was sought in grooves or flutings which cobblestones might be expected to make if movement occurred between the layers. Such were found. But the best evidence of the verity of shearing between layers was the intrusion of the earthy material itself along planes of slippage. This process Chamberlin was fortunate enough to observe in actual operation in places where a glacier was overriding embossments of rock. That internal shearing plays an important part in the movement of glaciers seemed thus to be demonstrated.

The many other features of the well-rounded glacial studies consummated in these three weeks must be passed over, but when, on August 26, the *Falcon* turned her prow southward and Lieutenant Peary left the ship to spend another dreary winter

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[21] Recent Glacial Studies in Greenland. Bull. Geol. Soc. Amer., Vol. 6 (1895), pp. 206-208.

in the Arctic, it seems to have been generally remarked that the only member of the party who had accomplished what he set out to do was Professor Chamberlin.

MULTIPLE GLACIATION IN NORTH AMERICA

The first formal attempt to define and name the multiple glaciations of North America was that of Chamberlin which appeared in 1894 in the third edition of "The Great Ice Age" by James Geikie. His classification was as follows:

Glacial Period

1. Concealed under series (theoretical)—Unknown.
2. Kansan stage of glaciation—First (represented) glacial epoch.
3. First interval of deglaciation—First interglacial epoch.
4. East Iowan stage of glaciation—Second glacial epoch.
5. Second interval of deglaciation—Second interglacial epoch.
6. East Wisconsin stage of glaciation—Third glacial epoch.
7. Later oscillations of undetermined importance—Final glacial epoch embracing possibly a fourth glacial epoch.

W J McGee had distinguished two glacial drift sheets in Iowa, an "upper till" and a "lower till," separated by a bed of interglacial accumulations not everywhere a "forest bed" but conveniently so styled. McGee had worked chiefly in northeastern Iowa, but had reconnoitered the area south and west of the Des Moines lobe of Wisconsin age and had there identified the same group of sheets which he had long studied in northeastern Iowa. Chamberlin, however, in a reconnaissance along the route of the Burlington railway in southern Iowa, had seen the extensive excavations recently made to get ballast for the road, and particularly the deep cuts made between Afton and Thayer in improving the grade across the Grand River Valley. McGee had not visited this most promising locality. Since they could not understand one another's descriptions, they held a field conference in 1893, divided between the areas of their more special studies. McGee's interpretation thus centered on northeastern Iowa, while Chamberlin's centered on the Grand River localities *in the south central summit region of Iowa.* These seemed to be in general harmony, though today we know that a portion of northeastern Iowa is surfaced by a younger drift not occurring in central and southern Iowa.

Shortly after this field conference, Chamberlin was asked by Geikie to prepare the chapters on the glaciation in North America for his well-known work, "The Great Ice Age." This invitation was the immediate occasion for framing and publishing the mode of interpretation, correlation, and nomenclature given above.

> "At the outset this scheme recognized only three ice invasions of the major type. The bed of till lying on Paleozoic rock at the bend of the Grand River, about a mile southeast of Afton Junction, was taken as the type locality of the glacial sheet left by the first ice invasion. It was named Kansan from the belief that it belonged to the same horizon as the old patchy drift on the rise toward the Ozarks south of the Kansas River, and that it represented the farthest extension to windward of the North American ice invasions. The Kansan formation at this typical locality was also made to include the kame-like deposits shown in the four trenches nearest Afton Junction. The original description in *The Great Ice Age* (page 757) was placed under the sidehead, 'Kansan formulation.' It includes this statement, 'there occur buried lenticular masses of gravel that appear to be kames formed on the surface of this deposit (the original Kansan) and subsequently buried by till belonging to the next higher formation' (the original Iowan).
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> "In the original text the Aftonian (interglacial deposit between first and second drifts) was not characterized by gravels but by the direct evidence of an interglacial interval, such as soils, muck, peat, twigs, limbs, trunks, fossils, etc. (*The Great Ice Age,"* pp. 757-759). No stress has ever been laid on gravels by the proponent of the scheme of 1894. In so far as gravels were associated with these direct evidences, they were usually gravels of the horizontal stream type as already stated.
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> "The very exceptional exposures of the upper till in the Thayer-Afton Junction region were regarded as the product of the second ice invasion. They embrace the greatest summit formations in Iowa and constitute the most representative glacial formation of that state. Even in 1894, it was practically known

to occupy a wide belt at the surface embracing and running under the Des Moines lobe of the third (Wisconsin) ice invasion."[22]

"In the two years following the publication of the correlation of 1894, it was shown by reconnaissances that the upper till sheet, the original Iowan, extended farther south than it was represented on the map in *The Great Ice Age*. Bain held, on the basis of a reconnaissance, that it extended to the state of Kansas. This indicated that the second ice invasion was more nearly coextensive with the first than had been assumed with unnecessary conservatism. This increased the importance of the Iowan and added weight to the priority that had been given it. It was preeminently worthy to bear the state name as it was the most outstanding glacial formation in Iowa.

"Up to this time, it had been natural, as well as conservative, to assume, in the absence of evidence to the contrary, that the old glacial sheets in Illinois were contemporaneous with the old sheets in the trans-Mississippian region. But almost simultaneously with the issue of a three-fold division of the system of 1894, Leverett found that one of the older ice sheets of Illinois had crossed the Mississippi and overlapped the upper sheet of the west side. He also found evidence of a notable interval between the overlapping sheet and the formation of the preceding surface. This was fully confirmed later, and in 1896 the Illinoian invasion was added to those previously named, making four in all.

"As these four glacial invasions are all that now (1928) seem worthy of recognition, it will help to give definiteness to our sketch of the history of the shifting of names that followed, to give the four here in tabular form.

4. The fourth glacial invasion, Wisconsin.
3. The third glacial invasion, Illinoian.
2. The second glacial invasion, Iowan.
1. The first glacial invasion, Kansan."[23]

Most unfortunately the two years following the publication of the system of 1894 in "The Great Ice Age" were marked by so many shifts of terms and correlations that it is difficult for the ordinary reader or the busy geologist to follow the history

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[22] T. C. Chamberlin: The Development of the Theory of Multiple Glaciation in North America. (Unpublished manuscript, March, 1928, pp. 16-18.)

[23] Ibid., pp. 19-20.

with accuracy. The shifts followed one another in almost kaleidoscopic frequency and complication.

The attempted emendations began in 1895 with the recognition by Calvin that the surface drift in the northern portion of Johnson County, in east central Iowa, was much younger in age than the drift surfacing the southern portion of the county, and that there were thus three instead of two pre-Wisconsin tills in McGee's area. In the northern part was a thin, patchy drift of young aspect, covering the much thicker drift (the middle sheet of the three-fold division) which was the surface formation to the south.

McGee had used the terms "lower till," "forest bed," "upper till." Chamberlin's scheme used Kansan, Aftonian, Iowan, terms not used previously. Chamberlin always believed firmly that McGee's "upper till" was, in the main, the great surface sheet of southern Iowa (the second drift or upper till in Chamberlin's type section near Afton), but included also in northeastern Iowa the thin local cover sheet which Calvin, in 1895, differentiated as the third drift. His own system of 1894 took no cognizance of this cover sheet, which came to be appreciated later and which is confined to northeastern Iowa, but only recognized the main mass of McGee's upper till, keeping to the fore the splendid exposures in the Afton region, far beyond the southern limits of the cover sheet. He had temporarily attached the prefix *East* to the state names, Wisconsin and Iowan, as a prudential measure to guard against a possible misuse of the unqualified state names, for he felt that only the best exhibit of a given state should, as a rule, bear the unqualified state name, and that too little was then known of the glacial formations of northwest Wisconsin and western Iowa to warrant the unqualified use of the names Wisconsin and Iowan for formations which had only been intensively studied in the eastern halves of these states. Later he regretted this excessive scrupulousness, as the prefixes were only so much lumber and even led frequently to misinterpretation. They were soon discarded when no longer needed.

The Iowa Geological Survey began in 1895 to give particular attention to the local cover sheet and adopted the view that McGee's upper till was merely this cover sheet of northeastern Iowa rather than the great sheet on which it rests and which is the surface drift over a large part of the state beyond the confines of the cover. The Survey then went further and proceeded to use Chamberlin's term Iowan for the cover sheet and his term Kansan for the greater sheet below and beyond, which Chamberlin had called Iowan. This use of Chamberlin's names was then given much currency in the Iowa county reports which were coming out in rapid succession. Bain later went to the Afton region and found that these names, as the Iowa geologists were then using them in northeastern Iowa, did not fit Chamberlin's type sections. He might now have recognized the Afton sections as the basis of Chamberlin's published nomenclature, and in accordance with the claims of priority have brought the newer developments in northeastern Iowa into harmony with it. Instead he shifted Chamberlin's names to fit his own ideas of northeastern Iowa, transferring the name Kansan, which Chamberlin had used for the first drift, to the second drift which Chamberlin had named Iowan, thus leaving the first drift without a name and divorcing the term Iowan from its type sections near Afton and Thayer. The Iowa county reports then came to use the succession, sub-Aftonian or pre-Kansan, Kansan, Illinoian, Iowan, and Wisconsin.

Perhaps nothing showed more plainly the restraint, patience, and generously friendly character of Chamberlin than his reception of these developments. He revisited Thayer and Afton with Bain, who later was to take graduate work and the doctor's degree under him at Chicago, and also went over other parts of Iowa as guest of the State Survey. The problems were complicated and confusing, and he was able to appreciate alternative points of view. In a very generous spirit he acquiesced, though inwardly with considerable misgiving. In later years, when he saw the fuller relationships of the glacial succession and was convinced that a very unfortunate mistake had been made, he deeply regretted having allowed his kindly impulses to overcome his judgment.

The difficulty had arisen from a difference of opinion as to the proper interpretation of McGee's lower and upper till with separating forest bed, which even today is a debatable question. More particularly it arose from making an interpretation so open to doubt the basis of a correlation and nomenclature to which the whole American glacial succession was to be adjusted. The uncertainty of the premises may be illustrated by quoting from a paper by Calvin in 1898:

> "McGee looked upon the forest bed as the plane of division between his lower and upper till, but later investigators following the lines which he pointed out have reached the conclusion that his lower till embraces two distinct drift sheets, and that it is between these two that the forest bed invariably lies. Thus there are three drift sheets in northeastern Iowa, and in the recent literature referring to Pleistocene geology they are known respectively as sub-Aftonian or Albertan, Kansan and Iowan. No forest material has been observed between the Kansan and the Iowan, but in this situation there occur extensive beds of stratified sands and gravels."[24]
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> "Calvin believed that in northeastern Iowa the forest bed was 'invariably' between the sub-Aftonian and Kansan tills."[25]

Though the names have been shifted, this recognition that the forest bed, as then known, was between the first and second drifts and not between the second drift and the cover sheet is in perfect accord with Chamberlin's classification. The cover sheet, not recognized by Chamberlin but subsequently differentiated, accordingly becomes a separate unit to be named and fitted into its proper place in the sequence. More recently patches of forest bed have also been found between the second drift and the cover sheet.

In the cut of the Chicago Great Western Railroad near Oelwein, in northeastern Iowa, were found three drifts with a dis-

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[24] Samuel Calvin: The Interglacial Deposits of Northeastern Iowa. Iowa Acad. Sci., Vol. V (1898) pp. 64-70.

[25] George F. Kay: History of the Investigation and Classification of the Pleistocene Deposits of Iowa. Iowa Geol. Surv., 1928, p. 20.

tinct forest bed between the lowest drift and the intermediate drift. Bain wrote: "A review of sections published by McGee makes it more than doubtful whether the forest bed which he has so clearly shown to be present does not mark the Aftonian rather than the Buchanan horizon between the two upper tills."[26]

Kay, however, thinks that there is good argument in defense of the policy followed by the Iowa Geological Survey, if proper emphasis is given to McGee's description of his two tills, their mapping, and his forest bed or gumbo, rather than too great stress given to his forest bed alone. He continues: "At the time the Oelwein cut was described gravels and sand only had been found by Calvin and his assistants in sections separating the Iowan from the Kansan, whereas McGee emphasized the number of well records in which a forest bed separated his Lower Till from his Upper Till. Since, as stated above, it is now known that in McGee's area there is in places a forest bed at the same stratigraphic horizon as the Buchanan sands and gravels and a forest bed at the stratigraphic horizon of the Aftonian, quite naturally McGee's forest bed in some of his sections is the forest bed between the Kansan and the Iowan, as these names are now applied, and in others the forest bed lies between the Kansan and the pre-Kansan."[27]

Here would seem to be the probable truth of the matter. In some places McGee's upper till was the second drift; in other places it was the second drift plus the cover sheet where that was also present; in still other places it was probably merely the cover sheet. Stratigraphically, McGee was lumping together in many places two distinct drifts, a thick and a thin one; topographically, he noted more particularly the characteristics of the thin local cover in northeastern Iowa. In this pioneer grouping together of different glacial units lay the germ of the unfortunate proceedings which followed. Nothing composite, however, characterized Chamberlin's Grand River section near

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[26] H. F. Bain: Relations of the Wisconsin and Kansan Drift Sheets in Central Iowa, and Related Phenomena. Iowa Geol. Surv., Vol. VI, 1896, pp. 466-467.

[27] George F. Kay: Op. cit. p. 21.

Afton Junction in south central Iowa, which showed the first drift, which he called Kansan, and the second drift, which he called Iowan and without a later drift to cause confusion. But still the Iowa Survey shifted the very appropriate nomenclature of the splendid Grand River type section to fit the debatable interpretations farther north.

In the years which have followed, the comparatively insignificant cover sheet, carrying the name Iowan, has been an embarrassing member of the North American glacial succession and has occupied an uncertain position. The U. S. Geological Survey party engaged in correlating the older drifts around the driftless area in 1906-07 even tested out carefully the hypothesis that there was *no such distinct drift sheet,* but in the end became convinced of its existence. Later it was regarded by some as possibly the Keewatin equivalent of the Illinoian. But the very recent studies of Kay and Leighton seem now to demonstrate that it represents an early advance of the last great ice invasion, which we call the Wisconsin.

Thus, by a poetic irony of fate, the second drift which is *the* great drift sheet of the state of Iowa is now known as the Kansan, and the so-called Iowan glaciation of the Iowa geologists is apparently now to be taken out of the category of major ice invasions and reduced to a sub-stage of the Wisconsin. With four instead of the anomalous five major ice invasions, the American glacial succession comes into harmony with that of Europe.

Chamberlin did not live to see this last development, but in his later years he cherished the hope that respect for the claims of priority, coupled with the greater appropriateness of his terms, would lead to an ultimate return to the sequence, 1. Kansan, 2. Iowan, 3. Illinoian, and 4. Wisconsin, leaving the status of the cover sheet to be adjusted later. Iowa would then be restored to the prominence and prestige in Pleistocene nomenclature to which the state is so justly entitled. In his last year he started steps to bring this about, and had he lived longer his leadership might, perhaps, have accomplished it.

GEOLOGIC CLIMATES

Glacial geology had been Chamberlin's chosen field up to the middle nineties of the last century. By this time field studies had given a fair understanding of the main features of the Pleistocene glaciation of the country, and competent observers were actively engaged in advancing this knowledge through systematic areal investigations. Chamberlin's interest now spread to the larger scientific implications of glaciation. He turned in particular toward the causes of glacial climates which naturally led to a broader consideration of the changing climates of the geologic past. These, in their turn, were to prove but the first steps of that long flight of stairs which he was to mount steadily for thirty years till he reached the heights in cosmogony and fundamental geologic philosophy. The ascent throughout was laborious and difficult, but each step led directly to the next. The whole was a sustained effort remarkable alike for steady systematic progress into the unknown and for the far-reaching results finally achieved.

He began by considering the earth's atmosphere. "All our attempts at the solution of climatic problems proceed on some conscious or *unconscious* assumption concerning the extent and nature of the atmosphere at the stage involved."[28] Accumulating geologic evidence seemed to him to indicate no radical differences between the atmosphere in Paleozoic time and that of the more recent ages. Very opportunely signs of glaciation far back in the Paleozoic were then beginning to be found, and Chamberlin was quick to grasp their great significance. Belief was then nearly universal in an earth gradually cooling off from an original fiery state. The Laplacian nebular hypothesis had led directly to the view that the earth's early atmosphere was an enormous mass of hot vapors and gases surrounding a molten globe. Fifty times our total present atmosphere was the estimated amount of atmospheric carbon dioxide later to become locked up in our extensive carbonate rocks and coal deposits. The great hot primordial atmosphere was thought to have di-

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[28] "A Group of Hypotheses Bearing on Climatic Changes." Jour. of Geol., Vol. V (1897), p. 653.

minished to its present volume with the passage of time and earth climates to have become progressively cooler. But glaciation early in the earth's history and the vast original atmosphere, rich in carbon dioxide, postulated in accordance with the Laplacian hypothesis, were apparently incompatible. Chamberlin then examined the postulate of a once-molten globe with heavy uncondensed atmosphere and then, in turn, the basis for it in the nebular hypothesis at that time practically universally accepted. But the careful scrutiny which he gave the nebular hypothesis developed unsuspected weaknesses of a serious nature. As the foundations of the hypothetical structure began to crumble before his analysis, Chamberlin boldly ventured to question not only the verity of the supposed original molten state of the earth but the correctness of the time-honored Laplacian hypothesis itself.

But he was not one to tear down without trying to build something better in its place. Even in his 1897 contribution on climatic changes there was already entertained the alternative hypothesis that the earth may have grown as a solid body by slow accretion of meteoroidal material. A source of the earth's heat was found in self-compression through rearrangement of this heterogeneous material under the force of gravity. From within such an earth the atmosphere could be resupplied by volcanic and other exhalations in proportion as its gases were lost. Through losses and resupply, an atmosphere fluctuating only moderately about an equilibrium value seemed to be possible, and a vast, hot atmospheric envelope, with all its attendant geologic consequences, was therefore no longer a necessary deduction.

Current conceptions, both of the early atmosphere of the globe and of the origin of the earth itself, were plainly subject to the gravest doubt; alternative hypotheses to replace them were already in view. The way was thus opened for a study of geologic climates on a new basis, and a better theory for the origin of the solar system became one of the greatest needs of geology. To both of these great problems Chamberlin now directed his attention. The climatic studies matured first.

Some fifty years earlier Tyndall had suggested that glaciation might be dependent upon depletion of the atmospheric carbon dioxide whose peculiar competence to retain solar heat he had demonstrated. This suggestion, however, had failed to find much acceptance, partly from doubt as to its adequacy and partly from lack of any definitely assignable cause for the requisite intermittent depletion. Arrhenius, in 1896, endeavored to ascertain what degree of depletion of the carbon dioxide of the present atmosphere would bring on the conditions of Pleistocene glaciation and what degree of enrichment would produce the warm climate of the Tertiary. He reached the conclusion that the removal of 38 to 45 per cent of the present carbon dioxide would bring on glaciation, whereas an increase of 2.5 or 3 times its value would produce the mild temperatures of Tertiary times.

Chamberlin investigated the geologic setting and supplied the possible mechanism. Following the greater diastrophic revolutions the lands are relatively high and the land surfaces of the globe reach their greatest areal extent. At such times of unusually extensive exposure of the rocks, carbon dioxide from the atmosphere enters into chemical combination with the rocks, in the process of carbonation of the silicates, to an exceptional degree. This reduction in the amount of carbon dioxide in the atmosphere lowers its temperature by permitting more rapid radiation of the sun's heat from the earth. The oceans in turn become cooler and consequently absorb more carbon dioxide from the atmosphere. This further removal of carbon dioxide from the atmosphere results in a still further lowering of its temperature. Water vapor in the atmosphere also plays its appropriate part, and other processes are involved. Cold climates result.

On the other hand, during times of baseleveling, when shallow seas have spread widely upon the continents protecting great areas of rock from carbonation, carbon dioxide is freed in large quantities as the calcium bicarbonate in the sea water becomes changed to the monocarbonate in the development of the extensive limestone deposits so characteristic of such times.[29] With enrichment of the atmospheric carbon dioxide the earth is favored with warm, equable climates, such as have prevailed during much of geologic time. During such epochs the geologic record shows that temperate and tropical faunas and floras have spread strongly poleward.

The fluctuating climates of the earth manifestly have resulted from the interplay of a large number of variable factors. The extreme complexity of the processes in operation and the delicate adjustments that maintain a general equilibrium and have permitted fluctuation only between comparatively narrow limits of temperature made this a problem requiring for its solution the fullest understanding of what was involved and the keenest analysis. In 1900, Chamberlin presented his study of the intricacies of this problem and his working hypothesis of the important part played by fluctuating atmospheric carbon dioxide.[30]

An auxiliary agency in the control of secular climates appeared in possible reversals of the direction of water movement in the deeper portion of the oceans.

"In an endeavor to find some measure of the rate of the abysmal circulation, it became clear that the agencies which influenced the deep-sea movements in opposite phases [poleward or equatorward] were very nearly balanced. From this sprang the suggestion that if the relative values were changed to the extent implied by geological evidence there might be a reversal in the direction of the deep-sea circulation and that this might throw light on some of the strange climatic phenomena of the past and give us a new means of forecast of climatic states in the future."[31]

The most important opposing agencies are differences in temperature, including freezing and thawing, and differences in

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[29] "The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmosphere." Jour. of Geol., Vol. VI (1898), pp. 609-21.

[30] "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis." Jour. of Geol., Vol. VII (1899), pp. 545-85.

[31] "On a Possible Reversal of Deep-Sea Circulation and Its Influence on Geologic Climates." Proc. Amer. Phil. Soc., Vol. XLV (1906), pp. 33-43.

salinity of the ocean waters. In so far as evaporation exceeds precipitation in the low latitudes, it results in an increased salinity of the superficial waters and a tendency of these to sink and flow poleward. On the other hand, the lower temperatures of the high latitudes, and the exclusion of salts from freezing sea-water, increase the density of the water and tend to cause it to sink and flow equatorward. At the present time the polar agencies control the deep-sea circulation; the deep waters, even beneath the tropics, are cold, and the climate is rigorous and diversified. Although today we enjoy considerable amelioration of the more severe glacial climates of not long ago, the deep-sea circulation still remains equatorward as presumably it was also throughout the Pleistocene glacial epoch.

Chamberlin's survey of the existing temperatures and salinities of the ocean made it clear that the battle between temperature and salinity is a close one and that no profound change is necessary to turn the balance. To him it seemed that the lowering of lands by the baseleveling processes with resulting spread of the seas on the continents and accompanying warming of the atmosphere, partly through increased carbon dioxide and partly due to other causes, would be likely to render evaporation and salinity dominant and reverse the deep-sea circulation. Warm waters descending in the equatorial regions, moving poleward and rising to the surface in the high latitudes would constitute a veritable hot-water heating system delivering heat in the polar regions and giving the whole globe the mild, equable climates which the geologic evidence indicates have prevailed, interspersed with occasional epochs of glaciation, through most of the geologic past.

The correlation of climates with the pulsations of geologic history, specifically the relatively short diastrophic upheavals and the longer intervals of baseleveling and sea-transgression is beautifully shown by the geologic record. The transcendent importance of these relations in any adequate concept of the typical geologic period, either from the point of view of the physical history or that of the biologic response to the physical conditions, should have been apparent long ago. Yet very few geologists seem to have realized this. Few, indeed, have any convictions, pro or con, on the solidarity of the hypothesis of the reversal of the deep-sea circulation, presented a quarter of a century ago. If true, it was a contribution of far-reaching significance and should play a vital part in fundamental geologic philosophy; if faulty, there is still a great gap entirely unfilled.

THE REPLY TO LORD KELVIN

In 1897 Lord Kelvin delivered his famous address before the Victoria Institute on "The Age of the Earth as an Abode Fitted for Life." In that address he took up the question of loss of heat from a cooling molten earth and the longevity of the sun as a highly heated body, and from his analytical studies asserted with a finality unusual in scientific discussions that the age of the earth was more than twenty and less than forty million years, and probably much nearer twenty than forty million years. Geologists and biologists had been requiring much greater lengths of time for the recorded history of the earth and the evolution of its inhabitants. To be denied this seemingly necessary span of time was disconcerting, and yet so great was the prestige of Lord Kelvin in the field of mathematical physics that the age limits which he placed on the earth and its evolution had scarcely less force than a supreme court decision.

Geologists, according to Kelvin's conclusions, had been very extravagant in their time postulates. Possibly the scientific public carried the implications even further, for the address had been "permeated with an air of retrospective triumph and a tone of prophetic assurance." At best the geologists were in a position of uncertainty between the dictum of exact science on one hand and faith in their own methods and criteria on the other.

For some reason Kelvin's address was reprinted in *Science* in May of 1899. Chamberlin now promptly and boldly challenged Lord Kelvin along the whole front of his argumentative position.[32] Not only did he show the weaknesses of his geologic

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[32] Science. Vol. IX (1899), pp. 889-901 and Vol. X (1899), pp. 11-18.

assumptions, but he took him to task more particularly on the basis of physical laws and physical antecedents. The stroke was as unexpected at that time as today it seems remarkable in the light of subsequent advances in physics. Not a few British scientists expressed surprise that anybody should have the temerity even to reply to Lord Kelvin, let alone to attempt to unhorse him in his own field. Yet the latter is precisely what was done, in ways that can be appreciated much better now than at that time.

Let us follow merely Kelvin's third line of argument which was drawn from the theoretical age of the sun. Accepting the Helmholtzian theory that the sun's heat is solely the result of its contraction, Kelvin calculated that the sun could not have supplied sufficient heat for life on the earth for more than twenty to thirty million years. Let us now read Chamberlin's reply, keeping in mind as we do so that it was given before any of the remarkable developments in physics during the last few years, particularly the discovery of electrons, the complex structure of atoms, and the prodigious amounts of energy now known to be locked up in atomic structure. With our present concepts of atomic physics in mind, we may appreciate the extraordinarily prophetic vision developed in the following paragraph which Chamberlin wrote in 1899.[33]

"Here is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its correctness, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a complete theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would

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[33] Science. Vol. X (1899), p. 12.

affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. The Helmholtzian theory takes no cognizance of latent and occluded energies of an atomic or ultra-atomic nature. A ton of ice and a ton of water at a like distance from the center of the system are accounted equivalents, though they differ notably in the total sum of their energies. The familiar latent and chemical energies are, to be sure, negligible quantities compared with the enormous resources that reside in gravitation. But is it quite safe to assume that this is true of the unknown energies wrapped up in the internal constitution of the atoms? Are we quite sure we have yet probed the bottom of the sources of energy and are able to measure even roughly its sum-total?"

This sounds like inspired prophecy! Substitute our present word "electrons" for Chamberlin's "atomecules" and the vision of 1899 is almost uncanny in its prophetic correctness. That Chamberlin should thus have anticipated, by almost a quarter of a century, in form as well as in substance, some of the most basal concepts of modern physics was, as Professor Moulton has recently said,[34] in some sense accidental, but that he saw that there were these energies, that there *must be* some such energies, instead of being accidental, was the result of mature reflection upon a whole range of phenomena whose import he saw with remarkable clearness.

Even in 1899, when the full significance of this reply could not be appreciated, its effect on geologic thought was prompt and decisive; Lord Kelvin's time restrictions never bothered geologists thereafter. Significant also was the reply in showing how far its author had already advanced in those fundamental researches which later grew into an almost complete geologic philosophy.

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[34] F. R. Moulton: The Planetesimal Hypothesis. Science, Vol. LXVIII (1928), pp. 10-12.

THE PLANETESIMAL HYPOTHESIS

The study of ancient geologic climates disclosed facts which seemed to Chamberlin inconsistent with the common belief growing out of the Laplacian hypothesis that a hot, vaporous atmosphere of enormous extent surrounded the earth during its early stages. Tested by means of the molecular velocities, after the method of Johnstone Stoney, the supposed vast atmosphere was found to be an impossibility. This conclusion naturally led back one step further to the question of whether there was a basis for the view in the Laplacian nebular hypothesis, and eventually raised question as to the correctness of that time-honored theory of the beginning of the earth.

The earth-moon ring postulated by Laplace was put to the test and it was found that this supposed ring could not remain in a true gaseous state under the conditions required by the hypothesis. Chamberlin, therefore, began to consider the alternative possibility of molecules or particles revolving in independent orbits in planetoidal fashion. Condensation from such a state had been generally held, though without sufficient reason, to give rise to retrograde rotations, whereas most of the rotations of the solar system are forward. But Chamberlin, by a further analysis of the problem found grounds for concluding that, on account of the ellipticity of the orbits, forward rotations will in general result from the gathering together of individual particles. The importance of this conclusion was very great for it indicated the tenability of an alternative method of earth genesis. Instead of a single mode of genesis, it now became a question of which of two alternatives was better in accord with molecular behavior and recognized mechanical principles.

The next step was to apply critical tests to the Laplacian hypothesis along as many different lines as possible. Professor F. R. Moulton, who had been collaborating with Chamberlin from an early stage in these cosmogonic studies, brought to bear his mastery of celestial mechanics and his mathematical insight and contributed brilliantly to the success of their joint investigations. The two men were very different in their training, points of view, and methods of analysis. One was naturalistic, the other mathematical; together they made an ideal team, each supplementing the other. They worked so closely and informally together that the precise parts which each played in disproving the Laplacian hypothesis and in developing the planetesimal hypothesis to replace it are not readily separated and will not be differentiated here. They have been unscrambled to some extent by the authors themselves in a joint statement called forth by certain not over-careful criticisms and captures by other workers in related fields.[35] The credit belongs to both.

The searching tests by Chamberlin and Moulton proved conclusively the untenability of the Laplacian hypothesis. This famous theory failed to meet the requirements along a dozen different lines, and they formally and finally abandoned it in 1900. But so firmly entrenched in the scientific thought of the time was this beautifully harmonious hypothesis, and so great is the inertia of inherited ideas, that Chamberlin frequently remarked that he expected a quarter of a century would elapse before the geological and astronomical professions as a whole would finally give it up for something better. It is interesting to note that the date given in Moulton's new Astronomy (1931) for the general abandonment of the Laplacian hypothesis is 1925.

On the constructive side considerable futile work was done, largely by Chamberlin, in trying out the possibilities of collision between nebulous bodies, "but no escape was found from the high probability, amounting almost to certainty, that the resulting orbits would be too eccentric to fit the case of the solar system in any instance that was likely to occur."[36] He proceeded to a study of the effects of the differential attractions exerted by bodies on one another when they make close approaches, as developed by the researches of Roche, Maxwell, and others, and

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[35] "The Development of the Planetesimal Hypothesis." Science, Vol. XXX (1909), pp. 642-45. This statement has been reprinted together with the seven attacks and counter attacks which preceded it (Science, April 23, 1909, to November 5, 1909) under the title: "Our Friends, the Enemy, a Discussion bearing on Scientific Ethics, with Concrete Illustrations." Idem. (1909), pp. 21.

[36] Science, Vol. XXX (1909), p. 644.

found this a promising field. Not only was the chance of close approach vastly greater than that of actual collision, but in addition to the direct tidal effect he recognized also "the projective effect developed in a body of enormous elasticity already under high pressure and affected by violent local explosions which were subject to intensification by the changes of gravity brought to bear on them by a passing body.[37] The disruption of solid bodies was seen to afford a basis for explaining the clustered fragments of comets and the angularity of the meteorites into which they are supposed to be finally dispersed. The explosive projections from suns under the influence of a passing body were thought to explain the two-armed spiral nebulæ which were then coming into prominence as the dominant type of nebula in the heavens.[38]

Following these preliminary steps, which cleared the way by narrowing the limitations of tenable hypotheses and which defined and developed the governing principles, the planetesimal hypothesis was gradually molded into the form in which it appeared in 1904 in Year Book No. 3 of the Carnegie Institution. When it appeared somewhat further elaborated for textbook use in Chamberlin and Salisbury's "Geology," Volume II, at the beginning of 1906 and in Moulton's "Introduction to Astronomy" in the same year, the planetesimal hypothesis for the origin of the solar system was fully launched on its career.

At that time spiral nebulæ played an important part in the planetesimal hypothesis. Known then to be the dominant nebulæ of the heavens, it was at first thought that they might represent huge solar systems in the making. Their characteristic large central mass, from which two arms of scattered haze, studded with large and small knots of matter, curved spirally outward on opposite sides, seemed just what an embryo solar system ought to be according to the planetesimal hypothesis. The central mass seemingly would become the

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[37] Idem.

[38] "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulæ." Astrophys. Jour., Vol. XIV (1901), pp. 17-40 and Jour. of Geol., Vol. IX (1901), 369-92.

sun; the larger knots of matter in the arms would become planets by gathering in the innumerable planetesimals constituting the general haze, while the smaller knots would give rise to satellites and planetoids. Diagrams showed how a star passing close to our sun would cause the development of a nebula of this spiral form, but it was explicitly stated that the planetesimal hypothesis was in no way dependent on the spiral form or upon the nature of the great spiral nebulæ revealed by the telescopes. The only essential feature was that the passing star pulls less than one per cent of the matter of the sun into elliptical orbits around it.

This was the stem hypothesis; from it the peculiar conditions of any individual case might cause a solar system to evolve in accordance with any one of a considerable number of sub-hypotheses, all of them species of the same genus. Much time, in fact, was given to testing out various alternative sub-hypotheses. When, some years later, further researches with larger telescopes showed the spiral nebulæ of the heavens to be vast independent galaxies of stars beyond the limits of our own galaxy, the planetesimal hypothesis was in no way adversely affected by this advance of knowledge. In fact, the supreme advantage of the planetesimal hypothesis lies in having opened an entirely new field, wide and fertile in possibilities, and from which in all probabilty the true explanation of the solar system either has sprung or will spring. It may be that some other of the sub-hypotheses will be found to work out better, as knowledge develops, than the particular one to which the authors have given preference. But as yet their selection seems best.

CHAMBERLIN AND SALISBURY'S "GEOLOGY"

Chamberlin's chosen lines of research had led him into steadily widening fields and these he was now plowing deeply. The planetesimal hypothesis provided for an earth with inherited characters quite unlike those previously postulated. Not only did the new view assign to it double parentage, instead of only one parent, but gave it also very different environmental conditions during the formative stages of its youth when its characteristics were being established. Naturally the better-known eras of its later life were but the outgrowth of the juvenile earth and were to be interpreted in accordance with its antecedents. A new geologic philosophy therefore sprang from the planetesimal hypothesis, and its far-reaching ramifications were to occupy Chamberlin throughout the rest of his life.

The development of these new lines was an important feature of the Chamberlin and Salisbury "Geology" whose three volumes came out between 1904 and 1906. The publishing house of Henry Holt and Company had sought this textbook for their American Science Series some time before the planetesimal hypothesis appeared, and the text was already under way in 1900. But as the new cosmogony unfolded and many geologic innovations were found to spring from it, the authors set themselves the task of bringing out a comprehensive textbook of geology developed along the new lines.

Volume I treated geologic processes to which Salisbury contributed most largely on erosion and deposition, and Chamberlin most largely on diastrophism, vulcanism, and the origin and descent of rocks.

Volumes II and III gave the history of the earth. Chamberlin portrayed in the first two chapters the origin of the solar system and the early stages of the earth, sketching in parallel fashion the development of the earth to maturity, first as had been taught in accordance with the Laplacian hypothesis and then as logically developed from the planetesimal hypothesis. It was characteristic of the spirit and method of the two authors that they made every effort to amend the molten-globe story so as to accord better with recognized geologic facts, but seemingly with little success. The planetesimal evolution was then traced through the initial volcanic, atmospheric, hydrospheric, and life stages to the recognized Archeozoic era. These stages involved new conceptions of the cause of vulcanism, of the origin of the atmosphere, of the origin of continents and ocean basins, of megadiastrophism, and of the inception of life on the globe. The departure from earlier views was revolutionary in the highest degree, but so defective did the time-honored interpretations seem to the author that the new basis was formally adopted and the Laplacian hypothesis and its consequences were given no further consideration in the unfolding of the earth's later history.

From the Cambrian period onward Salisbury was primarily responsible for the physical evolution and Chamberlin for the biological evolution. Paleontology is remote from cosmogony, but for the treatment of the faunas and floras recorded in the geologic strata Chamberlin was quite well prepared. The Wisconsin Survey while he was state geologist had done much paleontological work, and his early diagnosis of the Niagaran coral reefs even to this day excites the admiration of those who give attention to these peculiar structures. He had also published, in 1898, "A Systematic Source of Evolution of Provincial Faunas," and in 1900, "On the Habitat of the Early Vertebrates."

In the former publication he pointed out the fact that earth readjustments which cause the emergence of continents and the withdrawal of marine waters into the ocean basins are periodic (or episodic) in their occurrence, and that these periods of activity are separated by long intervening stages of relative quiescence during which lands are lowered by erosion and shallow seas are gradually spread over the continental platforms. During such a time of extended "epicontinental seas," as he termed them, the conditions for what he designated the "expansional evolution" of shallow-water marine life are signally favorable. Later, after long-continued baseleveling and expansional evolution of marine life, diastrophism causes the withdrawal of the marine waters from the continental platforms, leaving around each only a narrow, rapidly shelving shore-tract between the land and the abysmal slopes. The shallow-water fauna is compelled to follow the retiring sea and to crowd itself into this restricted zone.

"The new conditions will be in many respects uncongenial, for the streams will be rejuvenated and the amount of land wash will be greatly increased. Those species whose existence is dependent upon clear seas will be in imminent danger of extinction. Certain species to which these conditions are congenial may on the other hand be favored, but the grand result must necessarily be the destruction of the larger part of the previous expansional fauna and the forced adaptation of the remainder to new, and on the whole sterile and hostile conditions. A stage of general repressional evolution is thereby inaugurated and, in a comparatively short period, it is safe to assume, all or nearly all preceding species will have passed out of existence and new species, in a much more limited number but better adapted to the new conditions, will have been introduced." [39]

As a result of warping, the continental platform is likely to be bordered by various embayments favorable to life, separated from one another by narrow shore tracts which largely prevent free migration of shallow-water life. Each embayment will therefore develop its fauna in measurable independence and each will become the generating area of a provincial fauna. If a time of quiescence and baseleveling now ensues, these embayments will grow into extensive gulfs and, as they join in a general epicontinental sea, the commingling of their expanding faunas may result in a new cosmopolitan assemblage. These principles entered notably into Chamberlin's discussion of the Silurian and Devonian faunas in Volume II of the textbook.

The textbook also echoed and built upon Chamberlin's classic "Habitat of the Early Vertebrates." In a few pages of effective flowing language he had portrayed the appearance of the fishes, the first known vertebrates, as one of the most abrupt and dramatic in the life history of the earth, and had proceeded to show in simple fashion, as if the whole story had been revealed to him, how perfectly the known facts fitted together to indicate that the earth's vertebrate animals had their beginning not in the waters of the ocean, as generally had been supposed, but in running waters on land.[40] The geological facts bearing

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[39] Journal of Geology. Vol. VI (1898), pp. 604-05.

[40] "On the Habitat of the Early Vertebrates." Jour. of Geol., Vol. VIII (1900), pp. 400-412.

on the problem were not so much marshalled in argumentative support of a thesis to be established as they were unfolded in proper succession, along with zoological observations, to give a clear picture which invited acceptance through its own inherent probability and remarkable harmony throughout.

In flowing water, an animal must maintain its position against the current either by a contract of some resisting kind with the bottom of the stream, or by an effective mode of propulsion competent to meet the constant force of the current without undue draft on its vital resources. Otherwise it would be swept out to sea and its race would be ended as a stream-dweller. It is different with ocean currents, for they return upon themselves and an animal may yield to them without losing its marine habitat; moreover, they are usually much feebler than river currents.

"A compact form of body presents obvious advantages, except as environment or food or locomotion requires some departure from it, and the vast majority of animals are more or less rotund, and their locomotive devices are adjusted to this form. But the rotund form offers much resistance to rapid currents and unfits the animal for effective stream life unless it persistently hugs the bottom. Neither the rotund floaters and swimmers like the ancient cephalopods, nor the cilliated sprawn of the sessile forms are well adapted to resist the unceasing pressure of a rapid stream, and these are practically absent from river faunas.

"There is only one conspicuous type that is facilely suited to free life, independent of the bottom, in swift streams, and that is the fish-form. The form and the motion of the typical fish are a close imitation of the form and motion of wisps of water-grass passively shaped and gracefully waved by the pulsations of the current. The rhythmical undulations of the lamprey which perhaps best illustrates the primitive vertebrate form, and is itself archaic in structure, are an almost perfect embodiment in the active voice of the passive undulations of ropes of river confervæ. The movement of the fish is produced by alternate rhythmical contractions of the side muscles, by which the pressure of the fish's body is brought to bear in successive waves against the water of the incurved sections. In the movement of a rope of vegetation in a pulsating current, it is the pressure of the pulses of water against the sides of the rope that give the incurvations. The two phenomena are natural reciprocals in the active and passive voices.

"The development in the fish of a rhythmical system of motion responsive to the rhythm impressed upon it by its persistent environment and duly adjusted to it in pulse and force, is a natural mode of neutralizing the current force and securing stability of position or motion against the current, as desired. Beyond question the form and the movement of the typical fish are admirably adapted to motion in static water and that has been thought a sufficient reason for the evolution of the form, and so possibly it may be, but fishes in static water have not as uniformly retained the attenuated spindle-like form and the extreme lateral flexibility as have those of running water. Among these latter it is rare that any great departure from the typical 'lines' and from ample flexibility has taken place, while it is not uncommon in sea fishes. Among the latter not a few have lost both the typical form and the flexibility. The porcupine-fish, the sea-horse, the flounders, and many others are examples of such retrogressive evolution, which is doubtless advantageous to them within their special spheres in quiet waters, but would quite unfit them for life in a swift stream. And if the view be extended to include the low degenerate forms, like the Ascidians, that are by some authors classed as chordates, the statement finds further emphasis.

"It is not difficult for the imagination to picture a lowly aggregate of animal cells, still plastic and indeterminate in organization, brought under the influence of a persistent current and caused to develop into determinate organization under its control, and hence to acquire, as its essential features, a spindle-like form, a lateral flexibility, and a set of longitudinal side-muscles adapted to rhythmical contraction, since these are but expressions of conformity and responsiveness to the shape and movement normally impressed by the controlling environment upon plastic bodies immersed in it. The necessity for a stiffened axial tract to resist the longitudinal contractions of the side muscles and thus to prevent shortening without seriously interfering with lateral flexibility, is obvious and is supplied by a notochord. Thus, by hypothesis, the primitive chordate form may be regarded as specific response to the special environment that dominated the evolution of a previously indeterminate ancestral form.

"In this larger application of the interpretation herein suggested, the chordate phylum is made to be essentially from first to last a terrestrial race, whose main habitat was the land waters and the land itself, though still a race that sent its offshoots down to sea from time to time from the mid-Paleozoic onwards."

These five paragraphs constitute the most hypothetical portion of the article, and that the hypothetical element was large was duly recognized by Chamberlin who was careful to state that the problem at that time admitted of no other than hypothetical treatment. Thirty years have since lapsed and the problem is essentially where Chamberlin left it. But the original article and the textbook exemplification of it have been notable contributions to one of the big problems of the evolution of living forms.

The textbook was the fruition of many years of research and teaching experience. Both authors were teachers of exceptional power and effectiveness. Chamberlin used the Socratic method in earlier years and with the occasional undergraduate class which he continued to teach nearly to the appearance of the textbooks; his later classes were exclusively graduate and to these he lectured, with an impressive command of the subject matter, upon the principles and theories of geology. Salisbury followed the Socratic method almost exclusively, and was recognized as one of the most outstanding teachers in the University. He was nearly at his best when the textbooks appeared. So the three-volume "Geology" served to extend widely the classroom spirit and intellectual methods of its authors, and through these, as well as by the great original contributions within its pages, it helped notably to mould American geologic thought.

THE MISSION TO CHINA

With that far-reaching vision which characterized so many of his benevolences, Mr. John D. Rockefeller conceived the idea of doing something worthwhile for the Chinese people. A sum, adequate for a commission to visit China, reconnoiter the ground, and prepare recommendations, was entrusted to the University of Chicago. The University's choice of commissioners to conduct this survey of China and its needs fell upon

Ernest D. Burton and T. C. Chamberlin—two men selected from widely different spheres of thought to combine their humanistic and scientific experience in a balanced team. Horace G. Reed and Rollin T. Chamberlin were appointed assistants. Professor Burton and his assistant encircled the globe traveling eastward and studying conditions in India en route, while the Chamberlins, to bring a different point of view, went westward, the whole party assembling in Shanghai at the beginning of February, 1909. For nearly five months thereafter the commission traversed China by rail, river boats, sedan chairs, and Peking carts, studying fifteen of the eighteen provinces of China proper and making short excursions into Mongolia and Manchuria.

Chamberlin was profoundly impressed by the industry, thrift, and practical good sense of the Chinese people, who, in spite of strange superstitions and weak civic and nationalistic spirit, have steadily maintained their civilization for forty centuries during which many an oriental and western civilization has budded, flowered, and gone into decay. Entering into the project in hand with great enthusiasm, many a day on the march and after dark before the flickering flame of a tiny nut-oil lamp in some rustic Chinese inn, he unfolded to his companions the dreams and plans which came to his fertile mind. He drew lines for railroads, visioned in detail more scientific utilization of natural resources, mineral and agricultural, grappled with the application of western medical practice and research to the conservation of human life, and thrashed over the many possible ways of blending eastern and western educational methods and institutions so as to yield the best prospective results in the Chinese environment. His idea throughout was to graft, as far as possible, improvements and innovations on what already existed and to plant anew as little as possible, thus avoiding the semblance of revolutionary changes.

The commission visited the various viceroys and other high government officials to discover their general attitude toward foreign cooperation, sought their opinions and suggestions, and established good working relations with Chinese officialdom. Government schools were carefully investigated. The foreign mission schools and missionaries contributed, on their side, the practical experience and conclusions growing out of many years of work and struggle with Chinese problems. Chinese and foreign views were quite diverse, but together they disclosed fairly the needs and opportunities, the strong and the weak sides of the Chinese people and institutions, and the desirable and undesirable forces and tendencies then in operation in China.

But Chamberlin was too highly original a man to rely principally on the opinions of others when given carte blanche to make recommendations for something new of large proportions whose potential possibilities depended in considerable part on the judgment and constructive ideas of the commission. The commissioners must see China with their own eyes and in the final analysis build chiefly on their own observations.

Their climacteric excursion was up the Yangtze Kiang into far western China to the borders of the great mountains which buttress Tibet on the east. For thirty-nine nights, going and coming, Chamberlin slept afloat this great river and its tributary, the Min, mostly in native house-boats. The stupendous gorges above Ichang, where the river has cut through a folded mountain range, claimed a full week on the up journey in spite of favorable winds and a bonus to the boatmen for their best speed. Exposure of the whole Paleozoic stratigraphic section of central China in the canyon walls, with glacial tillite at the base of the Cambrian system, however, compensated the geologist for this enforced vacation from educational investigation.

Subsequently, four hundred miles in fourteen days across the Red Basin of Szechuan in sedan chairs took the commission through one of the loveliest garden spots on the face of the globe. Though sixty-five years of age, Chamberlin did much of this on foot for more intimate contact with the country and the people. The marvelous terracing of the hillsides, the intensive cultivation of the soil with utilization of plant reciprocities to secure the simultaneous cooperation of plants well fitted to one another so as to yield the farmer three to five crops a year, the simple but clever inventions of the Chinese, and the never-failing picturesqueness of the daily scenes, were a constant source of delight to him. Here was an old civilization unaffected by influences from without. Its inherent strength was plainly manifest, yet disease was rampant and dread epidemics swept away droves of people from time to time. In sanitation and in medical practice these people were still in the Dark Ages. The prompt cures by even such simple antiseptics as the members of the commission applied to the festering wounds and sores of several of their chair-bearers and boatmen astonished the natives. The white man's magic was plainly needed.

The commission eventually submitted a report of 977 pages covering the general outlook, the educational needs and the opportunities for large philanthropic work in China. The outcome of the report was a decision to concentrate effort in the field of medical science. The splendid Peking Union Medical College of the Rockefeller Foundation is now the tangible expression of this decision.

Their mission in China accomplished, the Chamberlins entrained at Mukden for Moscow in June, 1909. Ten days on the trans-Siberian train gave a much-needed rest, but the months of primitive travel and poor food had taken their toll. The first symptoms of serious stomach trouble, which was to afflict Chamberlin from time to time through the remainder of his life, now appeared. Slowly forming cataracts had already commenced to dim and blur his vision. A projected trip into Turkestan had to be abandoned, but father and son then zigzagged over much of Europe from the North Cape of Norway to Italy, Hungary and Roumania on the south, and from the Urals to the Atlantic, crossing as many of the mountain ranges as they could for a general picture of the orography and tectonics of Europe.

The expedition to China marked the final stage of Chamberlin's active field work. Well over six feet in height, of large frame which carried more than two hundred pounds, he was powerful physically, and the vigor which he displayed in tramping the paved paths of China seemed to show that he had lost little of his physical prowess with the advance in years. He felt fit and thought little of fatigue. But the break in health which began unexpectedly on the return across Siberia with stomach ulceration and severe attacks of sciatica, gradually grew worse and at several times during the next three or four years his condition occasioned alarm. Eventually skillful treatment prevailed and during the last fifteen years of his life his health slowly improved except for steadily failing eyesight.

PHILOSOPHY OF CORRELATION

Our knowledge of the history of the earth has grown gradually by fitting together the local histories of small areas studied in detail, but as yet the general story is very incomplete. Most crucial and also most difficult has been the correlation of events in widely separated regions. An early advance in working method was the division of time into geologic periods based on major events of the earth's history. As the underlying reasons and basis for the division into geologic periods came to be better understood, the classification and correlation of events took on a new meaning. In 1874, J. S. Newberry had recognized the fact that the transgressions of oceanic waters on the continents and the resulting marine sedimentary accumulations have been cyclical in nature. In Volume I of the Geology of Wisconsin (1883) Chamberlin defined the typical geologic period to provide background for his treatment of the Paleozoic systems of Wisconsin, and presented a clear picture of the sequence of events which normally occurs during a period. "Ideally there might be said to be three main epochs in each period: (1) that of advancing waters and coarse detrital deposits; (2) that of deep waters and limestone deposits; and (3) that of retiring waters and mixed shaly deposits; but practically such ideal symmetry is usually broken by minor oscillations and irregularities of movement." [41]

Fifteen years later he took up the philosophy of correlation in the first of a long series of fundamental studies.

"The most vital problem before the general geologist today is the question whether the earth's history is naturally divided

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[41] Geology of Wisconsin. Vol. I (1883), p. 210.

into periodic phases of world-wide prevalence, or whether it is but an aggregation of local events dependent upon local conditions uncontrolled by overmastering agencies of universal dominance. That there were no universal breaks in sedimentation or in the fundamental continuity of life is not only admitted but affirmed without hesitation . . . If, therefore, we seek for absolute divisions we doubtless seek in vain. But this does not dismiss the question whether the continuity of physical and vital action proceeded by heterogeneous impulses or by correlated pulsations. If the latter, then the history of the earth, when deciphered, will assume a rhythmical periodicity susceptible of natural classification and of significant and rational nomenclature; if the former, the contradictory phases of local actions will inhibit all but the most general unity and render classification and nomenclature either arbitrary or provincial." [42]

Chamberlin believed that the evidence strongly favored periodicity of the larger movements. The major movements of the earth's surface seemed to him to have been the sinking of the ocean bottoms and the withdrawal of the shallow epicontinental seas into the deepened basins. A case for the sinking was found in the force of gravity operating on the material of the earth's interior to produce rearrangement in more compact form. Quite different this was from the prevalent idea of contraction of the earth due to cooling from a molten condition. In the general process of rearrangement of matter in favor of greater density, the oceanic segments, being larger and of heavier material, sank most and the smaller and lighter continental segments sank less, thus increasing the topographic relief between the ocean basins and the continental protuberances. In the downcrowding process, the weaker, lighter continental segments were squeezed between the stronger, heavier oceanic segments and their borders in particular were wrinkled into mountain ranges.

Emergence and deformation of the continents in this manner take place with relative rapidity apparently when gradually accumulating stresses finally reach the yielding point of the materials. Then accompanying withdrawal of the shallow seas

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[42] The Ulterior Basis of Time Divisions and the Classification of Geologic History. Jour. of Geol., Vol. VI (1898), pp. 449-50.

interrupts the marine stratigraphic record. Land areas become extensive and locally the topographic relief is high. Climates become diversified and plants and animals are subject to the stress of changed environmental conditions. The whole combination of interrelated events is a diastrophic revolution involving and affecting geologic and biologic processes to an exceptional degree. An old period has been brought to an end and a new one begins.

Erosive processes now attack the rejuvenated lands with maximum vigor and in time reduce them to lowlands and extensive sea-level plains. Sands and muds carried to the ocean displace their volume of sea water. Sea level rises and shallow waters advance widely upon the bevelled land surfaces. Extensive sedimentary deposits accumulate on the continents constituting a geologic rock system. Climates are mild and life in general thrives. Such conditions develop slowly and continue for a long time. Eventually, growing stresses within the earth bring on another diastrophic revolution; the geologic period is ended, and another cycle is inaugurated.

So basic are these principles in fundamental geologic philosophy, that Chamberlin's own words may be repeated for further exemplification.[43]

> "I trust that many of you will agree that, in general, the relatively upward movements of diastrophism have been located continuously in the continents, and the broad downward movements continuously in the ocean basins, and that, setting aside incidental features, the dominant effect of the successive diastrophic movements has been to restore the capacity of the ocean basins and to rejuvenate the continents. This conclusion seems to me to be strongly supported by the general course of geologic history, wherein sea-transgressions and sea-withdrawals have constituted master features. Perhaps our firmest ground for this conviction is found in the present relations of the continents and the sea basins. If heterogeneity had dominated continental action in the great Tertiary diastrophisms, the results should stand clearly forth today. Some continents should show recent general emergence, while others should show simultaneous general

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[43] Diastrophism as the Ultimate Basis of Correlation. Jour. of Geol., Vol. XVII (1909), pp. 688-92.

submergence. The dominant processes today should be those of depressional progress, on the one hand, and those of ascensional progress, on the other. As a matter of fact, all the continents are strikingly alike in their general physiographic attitude toward the sea. They are all surrounded by a border-belt, overflowed by the sea to the nearly uniform depth of 100 fathoms. These submerged tracts are all crossed by channels, implying a recent emergent state. None of the continents is covered widely by recent marine deposits, and yet all show some measure of these. Wide recent transgressions in one part do not stand in contrast with great elevations in another. Even beyond what theory might lead us to expect, when we duly recognize the warping incidental to all adjustments, the recent relations of the continents to the seas conform to one type. The 10,000,000 square miles of continental margin, now submerged, is distributed around the borders of all the continents with a fair degree of equability. May we not, therefore, agree that in the world-wide phases of diastrophic movements, the basins have been additionally depressed and the continents repeatedly rejuvenated.

"It is important that we should agree, or agree to disagree, on one further point. Have diastrophic movements been in progress constantly, or at intervals only, with quiescent periods between? Are they perpetual or periodic? The latter view prevails, I think, among American geologists. This view has acquired special claims since base-leveling has come to play so large a part in our science, for it is clear that the doctrine of base-leveling is specifically inconsistent with the doctrine of perpetual deformation, for the very conditions prerequisite to the accomplishment of base-leveling involve a high degree of stability through a long period. The great base-levelings, and the great sea-transgressions, which I think are little more than alternative expressions for the same thing, have, as their fundamental assumption, a sufficient stability of the surface to permit base-leveling to accomplish its ends. Shall we not therefore agree that there has been periodicity in the world-warping deformations? Let this not be held with such exclusiveness that we fail to recognize duly the effects of the adjustment of minor stresses, at other times. These may be preliminary of after-effects of the larger movements, or they may be due to local stresses more or less independent of the general body-stresses. These quite certainly have been present, and have produced intercurrent departures from the strict tenor of the great systematic movements.

"Correlation by general diastrophic movements takes cognizance of four stages: (1) the stages of climacteric base-leveling and sea-transgression, (2) the stages of retreat which are the first stages of diastrophic movement after the quiescent period, (3) the stages of climacteric diastrophism and of greatest sea-retreat, and (4) the stages of early quiescence, progressive degradation, and sea-advance.

"(1) The characteristics of the climacteric stage of base-leveling and sea-transgression need little further characterization here, for the function of base-levels is known to all American geologists and the function of great sea-transgression to every stratigrapher and paleontologist. We have in base-leveling conjoined with sea-transgression, just that combination of agencies which is competent to develop the broad epicontinental seas of nearly uniform depth requisite for an expansional evolution of shallow-water life. At the same time, it furnishes for wide migrations and the comminglings that lead to cosmopolitan faunas of the shallow-water type.

"(2) The stages of initial diastrophism and sea-retreat find their criteria in the deposits that spring from an increased erosion of the deep soil-mantles accumulated in the base-level period, in the effects of increasing turbidity, in the lessening areas suitable for the shallow-water life, and in the limitation of migration.

"(3) The climacteric stages of diastrophism are marked by the stress of restrictional evolution among the shallow-water species; by increased clastic deposition in land basins, on low slopes, and on sea borders, by great land extension, but often, perhaps dominantly, by diversity of land surface and by liability to climatic severities and diversification. Arealy, land life is favored, but it is hampered by the climatic and topographic diversities, and these may prove graver obstacles to migration and intermingling than even the tongues of sea that previously traversed the land surface. Correlation by glaciation in these stages is likely to prove a valuable adjunct, but we must first test our criterion, for we are not as yet quite sure that contemporaneity of glaciation is inferred on reliable grounds. The shallow-water life of the diastrophic stages is driven into narrow border tracts and into local embayments, and is thus forced into special adaptations and into narrowly provincial aspects.

"(4) The early stages of quiescence and of base-leveling, with advancing seas, are peculiarly fruitful in biological criteria, for they are marked by re-expansions of the narrowly provincial shallow-water faunas of the previous stages. The progressive development of these provincial faunas and their successive unions with the faunas of neighboring provinces, as these come to coalesce by means of the progressive sea-advances, form one of the most fascinating chapters in life evolution, and give some of the most delicate of criteria for correlation.

"We are accustomed to look to the life record as our chief means of correlation. Its very high utility is quite beyond discussion. Thoughtful students, however, recognize that the paleontological record is based, in an essential way, on stratigraphy and that it is corrected and authenticated by the precise place the life is found to occupy in the stratigraphical succession. Stratigraphy and paleontology thus go hand in hand, each sanctioning the other. *Diastrophism lies back of both and furnishes the conditions on which they depend.*"

Hence Chamberlin's dictum: Diastrophism is the ultimate basis of classification and correlation.

THE ORIGIN OF LIFE

A radically new story of the parentage and birth of the earth having appeared in the planetesimal hypothesis, a new interpretation of its juvenile and mature activities was required. To this and to the working out of a comprehensive geologic philosophy on the new lines, Chamberlin devoted his later years. The newly opened leads naturally carried him into many different fields, one of which was the beginning of life on the earth.

An earth built up by accretion of planetesimals naturally presented very different surface conditions from one solidifying from a molten state. Instead of being entirely of crystalline igneous rock, the surface material of the youthful earth on the new basis was in part igneous, in part sedimentary and in part a heterogeneous assemblage of planetesimal matter of composition not radically different from that of the meteorites falling in today. This was the starting point. Rather opportunely an investigation of the gases obtained from heating meteorites in vacuo made during a larger study of the gases in rocks,[44]

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[44] Rollin T. Chamberlin, The Gases in Rocks. Publication No. 106, Carnegie Institution of Washington (1908), p. 80.

brought forth some facts seemingly of much significance. Meteorites were known to contain carbides, nitrides, sulphides, phosphides, and hydrocarbons, and it was noted both that these peculiar compounds are unstable on the surface of the earth, and that they include the particular elements which comprise living tissue and are essential to life. Furthermore, these unstable compounds of the elements essential for life were, according to the planetesimal hypothesis, commonly intergrown, or otherwise closely associated with one another, so that the products of any reactions that might grow out of their instability would be favorably located for reaction upon one another. Particularly significant, therefore, was the close intergrowth of these compounds of the organic elements in states of combination which, while stable in the cosmic regions surrounding the earth from which the planetesimal material was gathered, were pronouncedly unstable in the presence of air and water, even at the ordinary temperatures at which organic synthesis takes place. On the earth these compounds would inevitably have participated in chemical reactions giving rise to other combinations of the elements critical to biologic activity.[45]

With his characteristic thoroughness of treatment of problems, Chamberlin proceeded to analyze the environmental conditions that would have favored organic synthesis on the youthful earth. The open ocean seemed to him unfavorable for the start of life, for there diffusion and dilution of the products of reactions would prevail. But on the other hand, the porous mantle of the land areas, supplied by planetesimal infall with unstable carbides, nitrides, phosphides, and sulphides steadily undergoing transformation into more stable compounds and generating during this process hydrocarbons, ammonia, hydrogen phosphide and hydrogen sulphide gases mingled with the ordinary gases carried by the planetesimals, furnished exceptional opportunities for interactions and combinations.

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[45] T. C. Chamberlin and R. T. Chamberlin, Early Terrestrial Conditions that may have favored Organic Synthesis. Science, Vol. XXVIII (1908), pp. 897-911.

For specific illustration, Chamberlin selected a porous soil mantle situated alongside of a permanent body of water of appropriate salinity. In such an environment he envisioned many natural processes likely to cooperate in synthetic activity. Among these were percolation of water conditioned by a fluctuating water table due to accessions and evaporation, capillary action, osmosis, catalytic action, polymerization and other possible activities. Possible rôles which might be played by these were portrayed tentatively to deploy the problems in detail, but with constant reminders that nothing more than hypothetical suggestion was in mind.

What was attempted, however, was to indicate that in the early stages of the earth's development, according to the planetesimal hypothesis, the appropriate chemical elements, predisposed to enter into new combinations, were brought together in environments and under conditions singularly favorable for organic synthesis in the direction of primitive life. It was shown also that later, when planetesimal infall slackened, the opportunities for inception of life in this manner were greatly reduced. Chamberlin believed that the geologist's proper contribution to the problem of life beginnings on the earth was to handle the available stage settings for the start of the life story. This seemed to be accomplished by presenting a combination of inorganic processes operating on the necessary materials under conditions of such exceptional nature that they might have led into the realm of the organic. But to supply the vital spark of life which actually started the drama of life and kept it going falls naturally into the province of the biologist or the bio-chemist.

THE ORIGIN OF THE EARTH

When the planetesimal hypothesis was about fifteen years old and had become widely known from the textbooks and technical articles by its two authors, Chamberlin yielded to the request for a less technical presentation of the hypothesis in book form. The outcome was his little book, "The Origin of the Earth," which was published in 1916. To tell the story simply but well was his endeavor. For a better understanding and a more intimate contact, he took his readers into his workshop to show them the tools with which he had worked, and how, step by step, the investigation had been carried on. By relating how some efforts had proved futile and how various paths had led to no useful results he revealed the fullness of his explorations, unproductive as well as productive, and the persistency of his search for the truth.

"The Origin of the Earth" typifies Chamberlin's manner of unfolding a theme to the serious lay reader. The treatment throughout is direct and forceful, but the wealth of ideas and the rich literary style make the book far from light reading for either the scientific or the linguistic student. The reader must take it at its own level, for the author never allowed himself to lower his standards, though he simplified as much as possible by avoiding technical terminology.

The evolution of the planetary system and the juvenile shaping of the earth and its reorganization may be considered the key topics of the volume. The first is the planetesimal hypothesis, already considered; the others are the earth's growth and youthful development, worked out in accordance with that hypothesis and the state of geologic knowledge at the time. The infall of planetesimals, the holding of an atmosphere close about the young earth, and the gathering of its primitive waters into its surface hollows were the three concurrent activities which gave direction to the planet's growth and thus dominated its later career. The cooperation of this triumvirate—the lithosphere, hydrosphere and atmosphere—in the progressive shaping of the earth's surface is an intricate story partly told in this book but more fully developed in later publications.[46]

Changes in the rate of rotation, as the planet grew by adding to itself the planetesimals, set up stresses which analysis seemed

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[46] Study of Fundamental Problems of Geology: The Growth of the Earth. Carnegie Institution of Washington, Yearbook No. 25 (1926), pp. 372-87.

to show might lead to a primary segmentation of the earth body. Modifying atmospheric and hydrospheric influences also came into play. The finer and lighter planetesimal materials floated longest in the atmosphere and were brought down by rain and snow especially in the regions of greatest precipitation. These being dominantly the land areas, judged by analogy with present precipitation, such areas gradually came to be built up of materials slightly less in specific gravity than those falling on the water areas. Slight though these differences in specific gravity were, they were perpetuated and increased by selective action of the hydrosphere. The lands suffered weathering and wash, and the dissolved portions were added to the oceans, either to remain in solution in their waters, or to be deposited on their bottoms. The residual material left on land was richer in silica and aluminum and consequently of lower specific gravity than the more soluble compounds of calcium, magnesium, sodium, and potassium which found their way to the sea. Small as were the differences in density thus brought about, the separating processes were long in operation during the later growing stages of the earth, and tended to develop sub-oceanic segments of greater density than continental segments. Reorganization of the material, local liquefaction, and magmatic segregation proceeded in their appropriate ways in the natural course of events. In subsequent deformations of the globe the heavier oceanic segments were especially prone to sinking, thus accentuating the ocean basins and leaving the lighter continental segments standing progressively higher and more sharply differentiated. In this way the foundations were prepared for the ensuing geologic story.

DIASTROPHISM AND THE FORMATIVE PROCESSES

The autumnal years of Chamberlin's life were given in considerable part to gathering in some of the slowly ripening fruits of the planetesimal hypothesis. The most systematic harvesting consisted of a series of fifteen articles under the heading, "Diastrophism and the Formative Processes," which appeared in the *Journal of Geology* between 1913 and 1921. The planetesimal cosmogony had opened the way for the evolution of an elastico-solid earth, though it did not exclude the possibility of a molten earth or even the probability that molten and gaseous states may dominate planets much more massive than the earth. "The long, slow growth of the main mass of the planet offers rather strong presumption of a relatively cool, solid accretion attended by heterogeneities of composition and differentiations of accession and crystalline organization that were never smoothed out by liquefaction, but have remained of the same type as those now presented by the earth." The concept of an elastico-solid earth implied an organization and subsequent development very different from that of the liquid globe based on the Laplacian hypothesis. To Chamberlin this concept seemed to avoid many embarrassing difficulties with which he had struggled for years and, as he tested the different possibilities, it was gratifying to find that the new lines grew increasingly fruitful in leading to further discoveries.

Shrinkage of a cooling earth had been found to be inadequate to produce the surface corrugations observed in the many belts of mountain folding. And yet, following the molten globe hypothesis, about all the shrinkage left to be registered in the rocks would have been the meager amount that might have resulted from cooling for, with the passage of the earth substance from the assigned gaseous to the liquid state and thence at length to the solidifying state, a large part of all possible adaptation to the demands of pressure should have taken place before the record of disastrophism began. Furthermore, cooling would have been opposed by an embarrassing supply of heat from radioactive processes.

On the other hand, an earth built up slowly by the accession of solid particles would have retained very nearly its maximum resources of combination, adjustment, and compression. Such an earth was potentially capable of great compacting and consequent deformation. Chamberlin sought the probable order of magnitude of the shrinkage of the earth by comparing our planet with neighboring members of the solar system. The moon, whose mean radius is 1080 miles, has an average specific gravity of 3.34; Mars, whose radius is 2170 miles, has a specific gravity of 3.58; Venus, whose radius is 3851 miles, a specific gravtiy of about 4.85; and the earth, whose radius is 3959 miles, a specific gravity of 5.53. If these four bodies were built up by the ingathering of planetesimal material of essentially the same density, and if the progressive increase of density with increasing size of the body be the result of greater compaction under greater pressures, the shrinkage in volume may readily be computed. Chamberlin found that an earth built up of moon material of density 3.34, if changed to the earth's density of 5.53 by rearrangement of matter under pressure, would have undergone a circumferential reduction of 4555 miles, corresponding to a radial shortening of 725 miles. Perhaps more surprising was the fact that in changing from the size and density of Venus, a stage in which 80 per cent of the growth of the earth had already been attained, to that of the present earth a circumferential shrinkage of 1112 miles would have resulted. Apparently it might reasonably be concluded that the rate of shrinkage per unit increase of mass was greatest in the final stages of the earth's growth.

Although some of the greater density of the earth may have been due to heavier planetesimal material than that which went into the moon and the other planets,[47] the amount of shrinkage deduced was so great as to appear ample for all the crustal shortening implied in the contorted rock formations. The planetesimal earth thus possessed adequate potentialities for crustal deformation. A basis for diastrophism was thereby provided though Chamberlin left the specific activities and modes of concentration for more complete analysis in the concluding article of the series: "The Self-Compression of the Earth as a Problem of Energy."

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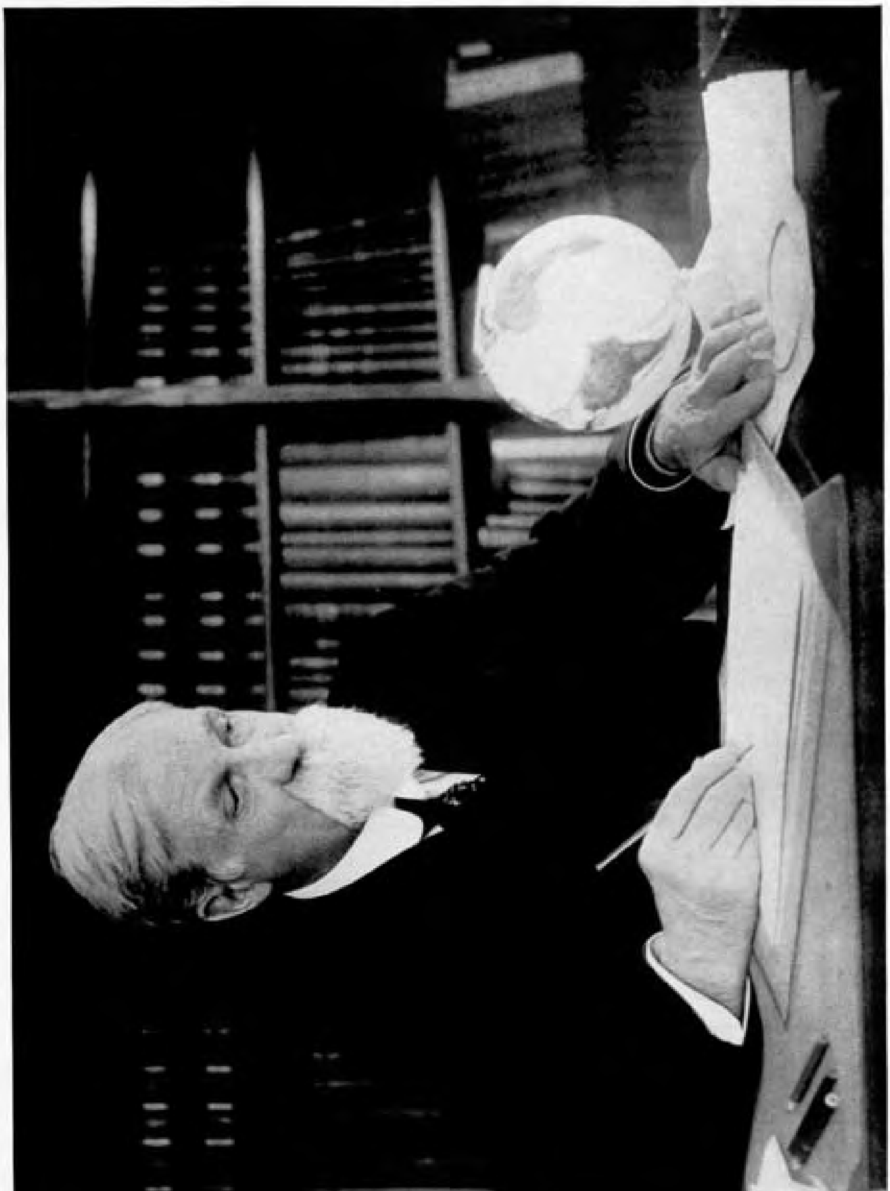
[47] Reasons, however, were advanced for thinking "that if the moon, Mars, Venus and the earth were built up normally by the planetesimal method, they should contain proportions of inherently heavy material in the inverse order of mass."

The series "Diastrophism and the Formative Processes" did not attempt a consecutive presentation of its author's philosophy of diastrophism. Many phases of that philosophy had been adequately presented before and some of them have already been considered briefly in this memoir. Rather was it a succession of critical essays on selected phases which Chamberlin wished to develop and extend beyond previous treatments. In particular, he gave much attention to the geologically significant interplay between diastrophism and the activities of the sea. The rejuvenation of the continents, their essential permanence as major relief features of the globe, the lateral stresses within the continental proturberances and their relations to continental creep and sea-transgression, the growth of the continental shelves, and the significance of the shelf-seas covering them, stood forth as subjects of prime consideration.

The geologic record shows that long periods of relative quiescence, during which the lands have been reduced to peneplains of wide expanse, have occurred from time to time. It is equally apparent that the base-leveling process cannot normally reach the stage of an advanced peneplain if continuous warping of the crust is in progress. On the contrary, the crust must hold an approximately static attitude long enough for the gradational processes to accomplish the results observed. The conclusion is clear that continental uplift has not been a continuous, steadily progressive process, but that it has recurred at such wide intervals as to permit in the meantime planation nearly to base-level with consequent evolution of shelf-seas and extensive inundations of the continental areas. All this seemed to Chamberlin to imply an elastic, rigid earth of sufficient strength to allow an accumulation of stress over a long period without notable yielding until, finally, easement of the stresses took place in a diastrophic revolution. The major diastrophic movements, according to the record, have been episodic in nature recurring between long periods of relative stability.

During times of stability, deposition of sediment on the outer slopes of the continental platforms built up continental shelves and extended them into the ocean basins. Chamberlin regarded



shelf-sea work as pre-eminently a process of terracing and the continents as pre-eminently the great terraces of the globe. Between the episodes of diastrophism the shelf-seas spread widely over the continents, extending the marine sedimentary deposits and affording opportunity for the evolution of shallow-water marine faunas. "The picture of the continents as essentially terraces wrought upon diastrophic embossments is no doubt the truest that can be found, and the contest between the diastrophic forces that emboss by protrusion and the gradational forces that terrace by planation and shelf-building, the chief physical battle of geologic history."

From the planetesimal hypothesis, followed logically through its natural chain of consequences under strict guidance from the testimony of the earth's past and present behavior, arose the foundations for a consistent, systematic interpretation of geologic history. Most fundamental was diastrophism for which the hypothesis afforded a ready cause and *modus operandi,* arising from the very nature of the postulated mode of the earth's growth. Chamberlin fitted in harmoniously the processes of vulcanism and made new contributions in that field. Cooperating with diastrophism in some ways and operating directly against it in others, the gradational processes played their appropriate part, as he brought out in masterly fashion. These in turn influenced earth climates in varying degree, and finally the whole combination, directly or indirectly, exerted a powerful control on the evolution of life.

The keynote of this series of articles was diastrophism in the large, for which Chamberlin coined the word, "megadiastrophism," to set it over the subsidiary manifestations of lesser magnitude ordinarily connoted by the term, "diastrophism." Few subjects have greater complexity or more ramifications, the mere enumeration of which reaches formidable proportions. To bring together some of the more essential developments and conclusions was Chamberlin's objective in the next to the last number of the sequence, which he called: "Groundwork for the Study of Megadiastrophism. Part I. Summary Statement of the Groundwork Already Laid." This was a succession of seventy-seven separately numbered propositions or paragraphs, each one of which reads like the abstract of an independent paper. A vast field was tilled in these seventy-seven furrows, which the plowman felt he had merely seeded for valuable crops to come in the future.

THE TWO SOLAR FAMILIES

Chamberlin's last publication was his booklet "The Two Solar Families." This was started as a revision of "The Origin of the Earth" at the request of the University of Chicago Press. But he soon found that his ideas had developed so greatly since the appearance of the earlier volume that two books would be required to cover the ground at all adequately. In the first he proposed to delineate the origin of the solar system, and in the second to present the growth of the earth. The first he completed; the second remains unfinished.

"The Two Solar Families" is the story of the sun's children. In an introduction, scarcely surpassed for beauty in the whole realm of scientific writing, we are told that the progeny of the sun belong to two families—the Planetary Family, embracing the planets, planetoids, and satellites, and the Cometary Family, comprising the comets, chondrulites, and meteorites. One parent was common to both families; the other was the chief differentiating factor. A passing star, the planetary family's stellar father, called forth larger masses of sun substance than the sun was accustomed to give forth at other times and gave the planetary offspring relatively robust constitutions and orderly habits of behavior. The cometary family came from feebler cooperation on the part of the starry host outside the solar system and fell heir to too much of the turbulence and the fiery antagonisms of the sun, and to too little of the steadying orbital energies of the starry group outside. "The planets, planetoids and satellites are plain plodding children compared with the comets, the meteors, and the meteorites. But the lives of the latter seem to be as notably short as they are spectacular and reckless in the wastage of their energies, while the lives of the former seem likely to be as long as they are modest, steady, and exemplary in the husbanding of their energies."

Among the changes in point of view from the earlier volume it is to be noted that the planetesimal hypothesis has become entirely divorced from the spiral nebulae which at one time had seemed to show family relationship. Much more important have become the spheres of activity, or spheres of control, of the sun and the individual planets, embracing both spheres of gravitative control and spheres of effective light pressure. The relative strengths of the pull of gravity and the push of light and other repellent forces of the sun distinguish the dynamic zones of the solar system. For small, light particles near the sun the repulsive forces overmatch the sun's gravitation and drive such particles away from the sun. Chamberlin saw in this a possible explanation for the striking fact that the four inner planets have grown less and are of higher specific gravity than the outer planets. He differentiated four dynamic zones concentric around the sun. The inner two he described as follows:

> "I. The first constitutes the inner environment of the sun. It embraces the orbits of the small, naked or slightly clothed planets, Mercury, Venus, Earth, Mars, and the planetoids. The repellent agencies effectively drive out of this zone free molecules and fine dust, except as they are held by the superior attractions of Venus, Earth, and Mars. Comet's heads suffer wastage and depletion while traversing this zone. Tail-stuff is developed from comet heads and driven out of this zone with great velocity. At the same time it seems to have been possible for small planets, planetoids, and satellites to assemble, to survive their 'perils of infancy,' to grow to their present states, and to give promise of living on to unknown lengths in spite of all solar repellency. These planetary bodies even seem to have thrived under conditions that were hostile to the comets.
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> "II. Outside this inner environment of the sun lies the zone of the great gaseous planets, Jupiter, Saturn, Uranus, and Neptune. In this zone the selective solar repellency seems to be much abated. There seems to be greater tolerance of gases, and there certainly was greater gaseous growth."

To accord with the observed natural division of the planets into two groups, an outer one of four large low-density planets and an inner one of four small high-density planets, Chamberlin suggested that the large outer planets arose from projectiles shot toward the passing star and that the smaller inner planets arose from projectiles shot in the opposite direction from the remote side of the sun. These two groups of four bolts each he supposed to represent the systematic response of the sun to the differential attraction of the star as it passed obliquely over the sun's two specially eruptive belts on either side of its equator. "It is assumed that there were two shots toward and two shots from the star in crossing each belt, making, in all, four great missiles sent far out past the star in its rear and four smaller missiles shot shorter distances from the opposite belts on the far side of the sun. We thus postulate only four main double shots. The planetoids are supposed to have arisen from small irregular projections stimulated on the far side of the sun by the passing star before it reached the specially eruptive equatorial belts." Chamberlin believed that there should have been counterpart eruptions of this irregular type from the side of the sun toward the star, and these he thought might possibly be represented by undiscovered bodies beyond Neptune. In fact, he introduced a diagram[48] which includes several lines of projection (with a question mark) for planetoids beyond the orbit of Neptune. As is well known, the planetoid Pluto, revolving far outside the orbit of Neptune, was discovered two years after his death.

The later pages of "The Two Solar Families" are filled with such a wealth of new contributions that nothing more than mere citations of a few lines of advance can be attempted here. A very careful analysis of the presumptive activities of the planetesimals brought out more concretely and completely than ever done before their probable behavior and showed how very naturally they may have occasioned the peculiarities observed in the different members of the solar system. Particularly suggestive is the chapter on the growth-creep of the axes of the planets in

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[48] The Two Solar Families (1928), Fig. 28, p. 156.

which it is shown that the impact of the planetesimals would shift the axis of rotation of a growing planet from the plane of projection of the solar outburst progressively toward a position at right angles to the plane of the solar system. The hitherto baffling inclination of the earth's axis, responsible so happily for our yearly seasons, thus apparently finds a very simple and plausible explanation. The searching analysis developed also possible explanations of the outstanding peculiarities of Mercury and Venus, the reason why the moon constantly presents the same face to the earth, and the reason for the opposing land and water hemispheres of the earth.

Although the cometary family is characterized by loose organization, yet Chamberlin brought the chondrulites, comets, and meteorites together as related products of gaseous outbursts projected to the farthest reaches of the sun's sphere of control where inequalities in the attractions of the stars outside gave side-pulls sufficient to cause the assemblage of now solid particles to miss the sun on the return journey and to swing around it in greatly elongated ellipses. Condensations of the gaseous masses on their outward journey produced the tiny chondrulites characterized by radical and concentric structures like the hailstones in a storm cloud. The whole swarm of chondrulites constituted a comet; when the comet came back into the heated environs of the sun at perihelion occasional collisions between a few of the chondrulites resulted in welding a very small proportion of them together into chondrule aggregates, which are the meteorites. So inherently simple and natural is the whole picture that it has much to commend it though it is radically different from its author's earlier conception of the formation of comets and meteorites.

"The Two Solar Families" came from the press on Chamberlin's eighty-fifth birthday, September 25, 1928, and the first copy to be finished was hurried to him for a modest celebration in the afternoon. He planned to rest for a month and then to take up again the manuscript for the second volume, "The Growth of the Earth," on which considerable progress had already been made. But a month later he was ill. The splendid mechanism which had worked so long and so well was worn out. He was taken to the University hospital, but nothing in the power of his medical colleagues could restore his weakening heart, and he slipped away peacefully on a still further journey into the unknown, November the fifteenth.

THE MAN HIMSELF

On the human side, Thomas Chrowder Chamberlin stood out nearly as distinctively as his works did in the field of science. Six feet one-and-a-half inches in height, of large, athletic build, he was a rugged and vigorous figure throughout most of his life. On the farm he had had a neighborhood reputation for pitching hay and for other occupations combining strength and skill; in college he excelled in sports, particularly football of the kind then being played. As a young geologist and administrator, his obvious physical strength and mental capacity, his open, friendly, sympathetic nature, and his marked abhorrence of all that was false and mean, led to his appointment to positions of greater and greater responsibility. As the years went by, his characteristic seriousness of mien, softened by a naturally kindly and serene facial expression came to give him an appearance of exceptional dignity. Later his dignity was enhanced by white hair and furrows of thought which gradually roughened his brow.

Most characteristic of the workings of Chamberlin's mind were his independence of thought and his intellectual integrity. In the bitter partisanship of the Civil War, he put in a good word, now and then, for the South. At the University of Wisconsin, whose previous presidents had been clergymen, he answered the sneering question: "Who will now preach the baccalaureate sermon?" by giving his own baccalaureate addresses, and so effectively as to create a profound impression by his originality of thought and his inspiring delivery. When satisfied that his policies were along the right lines, he was fearless in putting them into effect, though safely guided by a shrewd sense of what could be accomplished satisfactorily and what was inadvisable to attempt.

Intellectual honesty he placed highest, as was apparent in most of what he did. In all probability his consistent success in securing appropriations from the Wisconsin legislatures was due, in no small degree, to the confidence which he invariably inspired and to the transparent reasonableness of what he sought. Political sense he had, but he made little use of political ties and gave still less thought to personal popularity. In fact, as university president, he could scarcely be called popular in the ordinary meaning of the word, for he subordinated the personal and social aspects of his position to the carrying out of his ideas and the maintenance of his ideals in the administration and development of the University.

At the University of Chicago he was back again in the scientific work and environment most congenial to him. He brought together for the Department of Geology the strongest group which he could procure: Rollin D. Salisbury, in geographic geology; Joseph P. Iddings, in petrology; Richard A. F. Penrose, Jr., in economic geology; Charles R. Van Hise, as part-time professor of pre-Cambrian geology; Stuart Weller, in invertebrate paleontology; and, later, Samuel W. Williston, in vertebrate paleontology. In addition there were others on special appointments of shorter duration. This was indeed a remarkable assemblage of men who represented not only the best in their respective fields, but who were conspicuous also for exceptional personal qualities. Whenever the department came together in public it attracted attention; people commented on such a group of uniformly tall, handsome, dignified men. They reflected in a way the ideals of their leader, who was as thoughtful of superior manhood as of superior scientific ability and attainments. Likewise the associate editors of the *Journal of Geology,* selected from leading institutions in this country and abroad, were an imposing array of outstanding geologists. In the midst of such inspiring colleagues and associates, Chamberlin was in a position to do the best work of which he was capable.

During the first decade of the growth of the new University, Chamberlin taught some elementary courses as well as special graduate courses in glacial geology and the more advanced phases of general geology. His commanding personality, mastery of precise diction, and versatile fertility of mind, gave high cultural value to his undergraduate instruction. Though his contacts with the undergraduates were somewhat limited, whenever he did come in touch with them, they felt strongly his wholesome and stimulating influence.

But it was the graduate students who profited most from his lectures and the contact with him. For many years he gave a series of courses under the elastic title of "Principles and Theories of Geology." These ran through the whole gamut from cosmogony to the geology of the present and its implications for the future, and in the aggregate covered his entire geologic philosophy. Each year, completely ignoring the outline of the previous year, he presented his material afresh in accordance with his latest views and the newest developments. Thus it became quite customary for students to repeat the courses, for not only was a new slant commonly given to old problems, but many new problems were constantly coming to the fore. Cosmic geology, at the outset, and Pleistocene glacial geology, at the close, were naturally favorite fields for particularly thorough treatment; but outside of the more technical phases of paleontology and petrology, the whole geologic province came under his general survey. He elaborated the planetesimal hypothesis and traced with great care its bearing upon continental evolution, diastrophism, vulcanism, climatology, and the resulting biologic responses. All in all there arose a comprehensive geologic philosophy. But the prime object of these courses was not so much to present a consistent geologic philosophy as to develop the critical powers of the students in attacking new problems and thus to prepare them for their own researches. The chief emphasis was placed on sound, judicial thinking.

Setting an example was ever a vital part of Chamberlin's advanced teaching. He unfolded the steps of progress in many past researches and discussed critically his own and other current studies to bring out the objectives, difficulties in the way, and the means of overcoming them. Various students were utilized as helpers in the details of the larger studies, and thus they obtained first-hand acquaintance with his methods. In fact, the extraordinarily thorough drill in precise geologic thinking by Salisbury coupled with the inspiration for research from Chamberlin was in considerable measure responsible for the conspicuous success afterwards achieved by the impressive list of young geologists trained at Chicago when these two teachers were in their prime.

The quarter century 1892-1918 while Chamberlin was head of the Department of Geology at Chicago included his most productive years. In spite of multitudinous demands on his time, he was then engrossed primarily in his researches. Here lay his greatest interest; he took no vacations; research was play and any spare time was an opportunity not to be lost. But rarely did he drive himself; when in the mood he worked enthusiastically and effectively; when not in the mood for a particular problem, he read or attempted something else. While engaged on one of his books he once wrote fifty pages in one day, loafed the next, browsing in the library, and then wrote sixty-six pages on the following day. Though his large script filled pages quickly, the actual output of material was surprising in amount.

He wrote spontaneously and rapidly when handling new material, but such writings, fluent though they were, never reached the printer. They were rewritten and revised over and over again. It was amazing how much interlineation could be put on a page of seemingly satisfactory manuscript. Since his handwriting was peculiar and none too legible, the stenographer must have groaned under the constant slaughter of her copy. But he invariably kept at it until the final product satisfied his critical taste.

Somewhat similar was his habit of solving problems. With great fertility of imagination, he dreamed rapidly ahead along the various alternative lines of solution following the possible ramifications whithersoever they might lead. Later he cast aside the unpromising leads. Rejecting on the one hand what he termed the "method of colorless observation" as sterile, and on the other hand avoiding the "method of the ruling theory" as dangerous and likely to block the road to further advances, he believed that the true scientific method was to encompass the problem in all its aspects and to treat impartially the various factors and alternative possibilities. This he called the "method of multiple working hypotheses." [49]

> "Conscientiously followed, the method of the working hypothesis is an incalculable advance upon the method of the ruling theory, but it has some serious defects. One of these takes concrete form, as just noted, in the ease with which the hypothesis becomes a controlling idea. To avoid this grave danger, the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. It is thus in some measure protected against the radical defect of the two other methods. In developing the multiple hypotheses, the effort is to bring up into view every rational explanation of the phenomenon in hand and to develop every tenable hypothesis relative to its nature, cause, or origin, and to give to all of these as impartially as possible a working form and a due place in the investigation. The investigator thus becomes the parent of a family of hypotheses; and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted. Where some of the hypotheses have been already proposed and used, while others are the investigators' own creation, a natural difficulty arises; but the right use of the method requires the impartial adoption of all alike into the working family. The investigator thus at the outset puts himself in cordial sympathy and in parental relations (of adoption, if not of authorship) with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized, so far as may be, the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of mental attitude to the inquiry, knowing well that some of his intellectual children (by birth or adoption) must needs perish before maturity, but yet with the hope that several of them may survive the ordeal of crucial research, since it often proves in the end that several agencies were conjoined in the production of the phenomena. Honors must often be divided between hypotheses. One of the superiorities of the multiple hypotheses as a working mode lies just here. In following a single hypothesis, the mind is biased

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[49] Jour. of Geol., Vol. V (1897), pp. 843-45.

by the presumptions of its method toward a single explanatory conception. But an adequate explanation often involves the co-ordination of several causes. This is especially true when the research deals with a class of complicated phenomena naturally associated but not necessarily of the same origin and nature, as, for example, the Basement complex or the Pleistocene drift. Several agencies may participate not only but their proportions and importance may vary from instance to instance in the same field [sic]. The true explanation is therefore necessarily complex, and the elements of the complex are constantly varying. Such distributive explanations of phenomena are especially contemplated and encouraged by the method of multiple hypotheses and constitute one of its chief merits. For many reasons we are prone to refer phenomena to a single cause. It naturally follows that when we find an effective agency present, we are predisposed to be satisfied therewith. We are thus easily led to stop short of full results, sometimes short of the chief factors. The factor we find may not even be the dominant one, much less the full complement of agencies engaged in the accomplishment of the total phenomena under inquiry. The mooted question of the origin of the Great Lake basins may serve as an illustration. Several hypotheses have been urged by as many different students of the problem as the cause of these great excavations. All of these have been pressed with great force and with an admirable array of facts. Up to a certain point we are compelled to go with each advocate. It is practically demonstrable that these basins were river valleys antecedent to the glacial incursion. It is equally demonstrable that there was a blocking-up of outlets. We must conclude then that the present basins owe their origin in part to the pre-existence of river valleys and to the blocking-up of their outlets by drift. That there is a temptation to rest here, the history of the question shows. But on the other hand, it is demonstrable that these basins were occupied by great lobes of ice and were important channels of glacial movement. The leeward drift shows much material derived from their bottoms. We cannot therefore refuse assent to the doctrine that the basins owe something to glacial excavation. Still again it has been urged that the earth's crust beneath these basins was flexed downward by the weight of the ice load and contracted by its low temperature and that the basins owe something to crustal deformation. This third cause tallies with certain features not readily explained by the others. And still it is doubtful whether all these combined constitute an adequate explanation of the phenomena. Certain it is, at least, that the measure of participation of each must be determined before a satisfactory elucidation can be reached. The full solution therefore involves not only the recognition of multiple participation but an estimate of the measure and mode of each participation. For this the simultaneous use of a full staff of working hypotheses is demanded. The method of the single working hypothesis or the predominant working hypothesis is incompetent."

These were the basic principles of his scientific life and characterized his serious thought in other fields as well as science. They were already strongly in evidence in his youthful work on the lead and zinc problem in Wisconsin and continued to dominate his most mature cosmogonic researches. "Probably at no time in his career as an investigator did Chamberlin more courageously apply the method of multiple hypotheses than when, as a veteran of eighty odd years, he undertook to review and revise his work on the origin of the earth."[50] "No one else knows so well as I do that his every instinct and the habits of a lifetime kept him from ever making the planetesimal hypothesis a 'ruling theory.' To his last days he never ceased to regard it as a hypothesis to be put to the test in every possible way, rather than as an expression of final truth. . . . How seldom have these precepts been followed in science or philosophy! How different in many cases the results would have been had they been followed! What need there is for them also in social and political sciences, in theology, and in all the varied interests of the human mind!"[51]

The serenity of mind requisite for the long-sustained and successful journeys into the unknown was made possible by a happy home life which fortified him for his manifold tasks. What greater blessing can a man have than a sympathetic and understanding wife? What is more helpful than to find home ever a haven of rest from the cares and disappointments of the day's struggles? The difficulties melt away in the peaceful quiet of the happy home and the morrow starts fresh and full of

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[50] Bailey Willis, Dynamics is the Soul of the Problem. Jour. of Geol., Vol. XXXVII (1929), p. 362.

[51] F. R. Moulton, Thomas Chrowder Chamberlin as a Philosopher. Jour. of Geol., Vol. XXXVII (1929), pp. 372-73.

promise. In this all-important part of his life, Thomas Chamberlin was particularly blessed. Mrs. Chamberlin was indeed his ideal counterpart. She was home-loving, quiet and somewhat retiring by nature, but practical and capable and strong in the optimistic faith brought over from the previous pioneer generation. Though naturally proud of the achievements of her husband, successes or failures were not allowed to alter the cheerful, even tone of the family fireside. For well over half a century their life together was singularly harmonious and happy. When, at length in the summer of 1923, she was the first to answer the final call, it seemed as if he too, so dependent on her in many ways, might not long survive the parting. But her spirit continued with him in effect and he carried on for five years more.

It was always a source of satisfaction to the father that his son, the writer of this memorial, chose to follow in the same line of work. They had many things in common and their companionship was most congenial. From early childhood the father talked to the son as to a grown person and so spontaneously there developed a complete understanding between them. In later years of more philosophical trend, he outlined elaborately his inmost thoughts in many of the different realms into which the human mind penetrates. Screen after screen seemed to be lifted before long vistas into the hidden beyond. The inspiration was tremendous, and the poetic beauty of the vistas beyond description. But only the magic lamp of a master can illumine such portals; when the light is removed, one is left groping, though groping with a little more confidence for the visions which have been revealed.

At the close of the academic year in 1918, when approaching his seventy-fifth birthday, Chamberlin retired from the University. He continued, however, as senior editor of the *Journal of Geology,* but gradually lessened the load of outside demands on his strength. His time he now considered his own, to be utilized to the best advantage. Researches, as ambitious as any of former years, continued to fill his mind and to keep him conscious of how much he still had to do in the next few years.

He was working more slowly now. Cataracts had been dimming his vision gradually for a decade and all reading now had to be done through a large hand glass, but he toiled on without complaint. An operation to remove the opaque crystalline lenses had seemed the ultimate solution of the difficulty, but conditions did not reach the point where an operation was advisable and it was never attempted. Fortunately the eyes served for ten years more. His general health, which already had occasioned some alarm, now improved perceptibly. Needing more time for his work so badly, in his characteristic way he turned his power of analysis and experimentation upon his stomach problem and eventually developed a regimen of treatment and control which accomplished the desired results and carried him along.

Ten years for the continuation and rounding up of his researches were eventually allowed him, and approximately one-fifth of all his published papers appeared in this decade. A perusal of the bibliography will show their diverse scope and the breadth of interest which he still retained. All were mature productions, as might be expected, but some were as highly original as the inventions of youth. Even the final year brought little loss of mental vigor. Significant and fitting it was that his last publication, "The Two Solar Families," which came from the press on his eighty-fifth birthday, should be one of the finest of all his productions. A few days more and his busy life was over. Like the evening star in the mountains which shines, full of glory, undiminished till the end, his work continued to this brilliant close. A companion volume was in prospect, but the light was cut off too soon.

This continued devotion to duty was an expression of his belief in the value of his researches. Partly as the result of a successful life and partly because of his habitual thoroughness in following the method of multiple working hypotheses, he came to have great confidence in his judgment. If, at times, he seemed impatient of some other views which were different from his own, it was only because he felt that their sponsors had not considered the ramifications of the problems so carefully or so thoroughly as he had, and hence were less qualified to express judgment. Filled with enthusiasm by his message he wished to deliver it in full.

He was deeply religious in the truest sense, and the call of duty came likewise from his belief in the honesty of the Cosmos, as he expressed it, and from his prophetic outlook on life and its proper place in the Universe. The long geologic retrospect gave a basis for peering into the future. For hundreds of millions of years balanced processes have kept our globe a fit abode for life. A regulated system with orderly development has prevailed. Life has progressed to higher and higher levels. What has characterized the past is likely to characterize the future and the end seemed to him far distant. The heights to which the human race may attain, if it but utilizes its intelligence and knowledge to the best advantage, are likely to be far beyond our liveliest imagination. He sometimes remarked that if there were only a few thousand years ahead, much of the satisfaction which he took in his work would be lacking. But as the presumption is in favor of ages to come as long as those of the past, he believed that whatever good one truly accomplishes, if the results are to carry on as constructive factors in an advance for millions of years, may in the aggregate prove to be of inestimable value. This was his optimistic dream; he laid his few bricks and mortar with genuine satisfaction, and confidently looked to others to build higher and better in the long time to come. Now his own little bit is done; may the future substantiate his inspiring prophecy.

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