ART. XXVII.—*The Inculcation of Scientific Method by Example, with an illustration drawn from the Quaternary Geology of Utah;* by G. K. GILBERT. (With a map, Plate VIII.)

Presidential Address read before the American Society of Naturalists at Boston, December 27, 1885.

Mr. President and Gentlemen:—This is an association of teachers of science and investigators. Those of us who are primarily engaged in investigation have come here more especially as educators. It is our function to discuss, not our results, nor the subject matter of the several sciences with which we are concerned, but our methods of investigation, our methods of publication or promulgation, our methods of teaching.

It is fitting that this, one of the first formal addresses before the Society, should deal with some of the most general considerations affecting methods. In the statement of these considerations it is impossible to avoid that which is familiar, and even much that is trite. Indeed all expectation of entertaining or edifying you with the original or the new may as well be disclaimed at the outset. I shall merely attempt to outline certain familiar principles, the common property of scientific men, with such accentuations of light and shade as belong to my individual point of view.

The teacher's work is susceptible of a logical division into two parts. He stores minds, and he trains them. The modern educator believes the second function to be the higher, because the trained mind can store itself. Nevertheless the two go hand in hand and are in great part inseparable. The effort of the intelligent teacher is to employ such methods in storing the minds of his pupils with knowledge that they shall acquire at the same time the best training.

In that particular department of teaching which is called scientific, there is the same logical duality, and to a great extent there is a practical unity; but in this case there is a pre-determined classification of those who fall under the teacher's instruction, which has the effect of practically dividing his methods. A portion of his pupils are preparing to engage in the work of research, and look to a scientific career. Another portion are to be occupied with business or in other pursuits not implying research, at least in the ordinary sense, and desire to obtain, as a part of a liberal education, an acquaintance with the materials and results of science. The first demand a training in methods, the second consciously ask only for a store of knowledge. Nevertheless, the general student can best accomplish his purpose with the aid of a certain amount of training in method, while to him who proposes a career of investigation, there is an equal necessity for a large amount of positive knowledge.

Before proceeding to amplify these propositions it seems best to give consideration to the essential nature of scientific research—to restate, for the sake of a common understanding, the process by which science advances.

Scientific research consists of the observation of phenomena and the discovery of their relations. Scientific observation is not sharply distinguished from other observation. It may even be doubted whether there is such a thing as unscientific observation. If there is a valid distinction, it probably rests on the two following characters. Scientific observation, or the observation of the investigator, endeavors to discriminate the phenomena observed from the observer's inference in regard to them, and to record the phenomena pure and simple. I say "endeavors," for in my judgment he does not ordinarily succeed. His failure is primarily due to subjective conditions; perception and inference are so intimately associated that a body of inferences has become incorporated in the constitution of the mind. And the record of an untainted fact is obstructed not only directly by the constitution of the mind, but indirectly through the constitution of language, the creature and imitator of the mind. But while the investigator does not succeed in his effort to obtain pure facts, his effort creates a tendency, and that tendency gives scientific observation and its record a distinctive character.

Scientific observation is moreover selective and concentrated. It does not gather facts indiscriminately, but, recognizing their classification, it seeks new facts that will augment established groups. The investigator, by restricting his observation to a limited number of groups of phenomena, is enabled to concentrate his attention, and thus sharpens his vision for the detection of matters that are unnoticed by the ordinary observer.

The superficial relations of phenomena are discovered by induction—by the grouping of facts in accordance with their conspicuous common characters—or, in other words, by empiric classification. Such empiric classification is a preliminary work in all sciences. It is a convenient and temporary sorting of our knowledge, and with the increase of knowledge it is perpetually remodeled. But it is more than a mere convenience; it is a stepping-stone to a logical, or rational, or, more strictly, relational classification; for it leads to the understanding of those deeper relations which constitute the order of nature.

Phenomena are arranged in chains of necessary sequence. In such a chain each link is the necessary consequent of that which precedes, and the necessary antecedent of that which follows. The rising of the sun is consequent on the rotation of the earth. It is the logical antecedent of morning light. Morning light is in turn the consequent of sunrise and the antecedent of numerous other phenomena. If we examine any link of the chain, we find that it has more than one antecedent and more than one consequent. The rising of the sun depends on the position of the earth's axis as well as on its rotation, and it causes morning heat as well as morning light. Antecedent and consequent relations are therefore not merely linear, but constitute a plexus; and this plexus pervades nature.

Relational classification may be considered as of two sorts, first linear, and second, coördinate as determined by linear, that is to say, phenomena are linearly arranged in chains of sequence, and they are coördinately arranged in natural classes. A natural class is a group of coördinate facts having the same antecedents.

It is the province of research to discover the antecedents of phenomena. This is done by the aid of hypothesis. A phenomenon having been observed, or a group of phenomena having been established by empiric classification, the investigator invents an hypothesis in explanation. He then devises and applies a test of the validity of the hypothesis. If it does not stand the test he discards it and invents a new one. If it survives the test, he proceeds at once to devise a second test. And he thus continues until he finds an hypothesis that remains unscathed after all the tests his imagination can suggest.

This, however, is not his universal course, for he is not restricted to the employment of one hypothesis at a time. There is indeed an advantage in entertaining several at once, for then it is possible to discover their mutual antagonisms and inconsistencies, and to devise crucial tests,—tests which will necessarily debar some of the hypotheses from further consideration. The process of testing is then a process of elimination, at least until all but one of the hypotheses have been disproved.

In the testing of hypotheses lies the prime difference between the investigator and the theorist. The one seeks diligently for the facts which may overthrow his tentative theory, the other closes his eyes to these and searches only for those which will sustain it.

Evidently, if the investigator is to succeed in the discovery of veritable explanations of phenomena, he must be fertile in the invention of hypotheses and ingenious in the application of tests. The practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done. To answer these, we must give attention to the nature of the scientific guess considered as a mental process. Like other mental processes, the framing of hypotheses is usually unconscious, but by attention it can be brought into consciousness and analyzed.

Given a phenomenon, A, whose antecedent we seek. First we ransack the memory for some different phenomenon, B, which has one or more features in common with A, and whose antecedent we know. Then we pass by analogy from the antecedent of B, to the hypothetical antecedent of A, solving the analogic proportion—as B is to A, so is the antecedent of B to the antecedent of A.

Having thus obtained an hypothesis, we proceed to test it. If the hypothetical antecedent is a familiar phenomenon, we compare its known or deduced consequents with A, and observe whether they agree or differ. If it is unfamiliar, we ascertain its consequents by experiment or some other form of observation; and in the selection of the particular experiments or observations to serve as tests, we are guided once more by analogy, inverting the previous formula.

The question, whether or not the function of the mind in devising hypotheses and the tests of them is creative, is foreign to the present purpose. It suffices that we recognize the process as analogic, requiring for its success a preliminary knowledge of numerous instances of consequential relations. The consequential relations of nature are infinite in variety, and he who is acquainted with the largest number has the broadest base for the analogic suggestion of hypotheses. It is true that a store of scientific knowledge cannot take the place of mental strength and training, i. e. of functional ability inherited and acquired, but it is nevertheless a pre-requisite of fertility in hypothesis.

The great investigator is primarily and preëminently the man who is rich in hypotheses. In the plenitude of his wealth he can spare the weaklings without regret; and having many from which to select, his mind maintains a judicial attitude. The man who can produce but one, cherishes and champions that one as his own, and is blind to its faults. With such men, the testing of alternative hypotheses is accomplished only through controversy. Crucial observations are warped by prejudice, and the triumph of the truth is delayed.

Returning now to the subject of education, take first the case in which the student is to become an investigator. He is to observe phenomena, he is to frame and test hypotheses. As a matter of course, in order to learn to do these things he must do them. Sooner or later he must be sent directly to nature, out of doors or in the laboratory, and must in her presence train his faculties by practice. But before he undertakes this, the teacher can aid him by imparting methods. It is probably not best to offer them in the abstract until he has become well acquainted with them in the concrete. Typical investigations should be described in detail, illustrating the varied phases of the method of hypothesis, and not omitting to show how its successes are achieved through series of failures. The history of at least one science should be developed, with the rise and fall of its successive theories. These educational factors are directed to the training of his mind, but his mind needs also to be stored with scientific knowledge, which shall serve as a foundation for analogies. If he would explain some feature of nature, he must depend on the explanations others have reached for other features; and he needs large resources of knowledge of the relations of phenomena.

The course of training for the apprentice of science should give him, in the study room and in the class room, a varied acquaintance with the laws of nature that have been discovered by research. It should not needlessly burden his memory with empiric classifications, for these belong to the humbler walks of science, and it is unwise to impress on the novice the high importance of that which the master regards as provisional. It should teach observation by actual practice,—practice rigorously restricted to selected groups of phenomena. It should illustrate with varied reiteration—by books, by lectures, by demonstrations in the laboratory—the method of discovery by the aid of hypotheses. It should assign him actual investigation and subject his methods to criticism.

Students whose projected careers are not scientific, but who are unwilling to ignore so important a subject, naturally wish to cover a wide field in a short time. Their teacher, imbued with the vastness of science, is tempted to give them a maximum number of facts, with such order and classification as best favor their rapid statement. If he yields to the temptation, there is reason to fear that a permanent misapprehension is established, and the essence of science is not communicated. In my judgment he will do better to contract the phenomenal, and enlarge the logical scope of his subject, so as to dwell on the philosophy of the science rather than its material. For such students laboratory work may or may not be expedient, but they can at least accompany the pupils who look forward to careers in research in some of the descriptive illustrations of methods of scientific achievement.

The investigator becomes an educator when in giving his work to the world he describes the route by which his end was reached. It is not denied that the publication of sound conclusions is in itself educational, but it is maintained that the publication of the concrete illustration of a good method is educational in a higher sense. It is not insisted that all skillful investigations should be published *in extenso;* it is only affirmed that the number of such publications is far too small. We need for educational purposes more narratives of good work in all departments of research. Let the discoverer of a new principle recite every hypothesis that occurred to him in the course of his search, telling, if he can, how it was suggested. Let him lay bare the considerations which rendered it plausible, the tests that were conceived, and those which were applied. Let him show in what way the failure of one hypothesis aided in the invention of another. Let him set forth not only the tests which verify his final hypothesis, but the considerations which leave a residuum of doubt as to its validity. And finally let him indicate, if he can, the line or lines of research that promise to throw more light.

By so doing he will accomplish many things. He will guard himself against an overestimate of the strength of his uneliminated hypothesis, and he will thus diminish his self-conceit. By conscious attention to his methods he will improve them. He will therefore educate himself.

He will inspire the young investigator by his example, and even his experienced compeer will take courage from the success that after many failures finally crowns his efforts. He will give to every investigator who reads his paper a lesson in method—a good lesson if his method is good, and not necessarily a bad one if his method is bad. He will therefore educate his fellow workers.

If his work admit of popular presentation, he may be a missionary as well as a teacher, for he may help to dissipate the wide-spread impression that there is something occult in the ways of science. He will at least aid in showing that, whenever a theory is created and tested, knowledge is the gainer, whether the theory itself stands or falls; and that the demolition of hypotheses, instead of testifying to the futility of research, is the method and condition of progress. His educational influence will thus extend to that great lay member, the general reader.

In making this plea for education by example, it would be unfair to ignore another point of view. Not all are willing to be educated, not all need be; the majority of those who examine an essay seek only to learn its conclusions and have time for nothing more. For their use there should be appended or prefaced a concise summary of results.

And, on the other hand, it should be observed that the service rendered to science by one who describes his course of investigation is not educational merely. Rejected hypotheses have a positive value in the domain of the subject to which they belong, and he who makes them public gives to his fellow-workers in the special field the fullest advantage of his material. Some steps of his progress, which did not prove suggestive to him, will find fertile ground in the mind of another and bear fruit. This consideration places the progress of knowledge before the glory of the individual, and is opposed by a natural egoism; but it is only the man of small calibre who has no ideas to spare, and secretiveness in matters of science is ordinarily a confession of weakness.

It was intimated a moment ago that precept unsupported by example could not be depended on to infuse method—and the dictum applies even to the burden of this discourse. I am persuaded that my meaning will be better apprehended if I supplement my disquisition by an outline of an investigation of my own. The seeming egotism must be condoned, for it is manifestly impossible for me to trace out the actual course of observation and reasoning in the case of another's work.

To guard against possible misapprehension it is necessary to emphasize the fact that the following discussion contains an outline merely of its subject. Its subject is a certain geologic uplift that has been observed in Utah. To render it intelligible to those who are unacquainted with the literature of the geology of Utah, it will be introduced by a short account of Lake Bonneville.

The basin of Great Salt Lake lies in a region of mountains; to picture its character to your mind, conceive a plain the surface of which is embossed by parallel ridges of moderate length, from fifteen to twenty-five miles apart, and from 2,000 to 6,000 or 7,000 feet high. Conceive further that portions of this plain are uplifted, together with their mountain ridges, so as to enclose a basin 150 miles in either dimension, and you have the general structure of the district in question. The debris washed down from the mountains has for ages accumulated in this depression, so that the central-lying mountain ridges are nearly buried; indeed there is reason to suspect that some of them are quite buried, a plain of fine silt being spread smoothly over them. Great Salt Lake itself lies on the east side of the basin; the western half, which is only a few feet higher, is a saline desert.

In the last geologic epoch—the Glacial Epoch—the lake expanded so as to fill the basin to overflowing. The water surface was then very much larger, and as its area included the basins of several lakes now independent, it has been given a separate name. Lake Bonneville was very irregular in form; the mountain ranges of the basin ran long peninsulas from its north and south shores, and projected from its surface in numerous islands. The Quaternary winds, playing on its surface, dashed waves against its shores, and the spits and beaches and cliffs wrought by these waves remain in a high state of preservation to testify to the position of the ancient water margin. Through the greater part of its extent this shore-line forms a conspicuous feature in the topography of the country, and is readily traceable. It has been actually traced out, surveyed, and mapped with much care, and our knowledge of the old lake is in many other respects definite and full.

Its width from a few miles east of Great Salt Lake to the west side of the Great Salt Lake desert was 125 miles. From the mountains on the north of the desert to those on the south its expanse was about the same, but it did not terminate with the southerly mountains. It extended through them in several straits and formed beyond a second and much smaller body of water. The main body was 1,000 feet deep, the minor body about 500 feet.

Manifestly, when this old shore-line was made, all parts of it lay in the same horizontal plane, with no other curvature than that which belongs to the figure of the earth; that is, all parts of it were level. If it is not now level—if some parts are higher than others, it seems equally manifest that there have been local elevations or subsidences of the land. It is to such differences of level in the shore-line as it stands, and to their interpretation, that I desire to call your attention.

As far as the eye can judge, the shore is still level, and so long as no measurements were made its horizontality remained unquestioned. The two geologists who were probably the first to measure its height recorded their results in language implying no suspicion that more than one determination was necessary.

It happened that in the year of my first exploration of the Bonneville area I saw the shore-line not only in the Salt Lake basin but in the more southerly basin, and that I passed from one basin to the other by a route that did not reveal their connection. In doubt whether two lakes were under observation or only one, I sought to answer the question by determining the height of the shore-line in each, my instrument for the purpose being the barometer. The verdict of the barometer was that the southerly shore-line was somewhat higher than the northerly, but the computations necessary to deduce it were not made until the mutual continuity of the two shore-lines had been ascertained by direct observation. The barometric measurement was therefore superseded as an answer to the original question, but it answered another which had not been asked, for it indicated that the ancient shore at one point had come to stand higher than at another. The postulate of horizontality was thus overthrown.

An hypothesis immediately took its place. It is one of the great inductions of geology that as the ages roll by the surface of the earth rises and falls in a way that may be called undulatory. I do not now refer to the anticlinal and synclinal flexures of strata, so conspicuous in some mountainous regions, but to broader and far gentler flexures which are inconstant in position from period to period. By such undulations the Tertiary lake basins of the Far West were not only formed but were remodeled and rearranged many times. By such undulations the basin of Great Salt Lake was created. As to their cause, geology is absolutely ignorant, and she is almost absolutely silent. When it was ascertained that the Bonneville shore-line at two distant points had not the same height, the first hypothesis to suggest itself merely referred the difference to this gentle undulatory movement of the crust. As other hypotheses are to be mentioned, it will be convenient to christen this one the hypothesis of unexplained undulation.

A few years later the discovery was made that a fault had occurred along the western base of the Wasatch range of mountains since the Bonneville epoch. This range lies just east of Great Salt Lake, and the Bonneville shore is traced across its western face. The effect of the fault was to lift the mountain higher, with reference to the lake bottom, and to carry that part of the old shore-line upward. The amount of the uplift varied in different parts of the fault from ten to fifty feet.

This discovery was something more than the finding of a post-Bonneville fault; it was the discovery also of a new method of recognizing faults—of a peculiar type of cliff produced by faulting, which, though by no means obscure, had previously been overlooked by geologists. It gave rise to a new tentative explanation for the displacement of shore-line discovered by barometer, namely, that it arose by faulting; and it opened a new line of observation.

Two hypotheses were now under consideration, but they were not strictly alternative. Perhaps it would be better to say that the origination of the second not only gave an alternative but also modified the first. The Wasatch fault must affect the height of the shore-line, and wherever it crossed the shore-line that line must be discontinuous and exhibit two levels. The admission of disturbance by faulting was therefore compulsory, but faulting might or might not be sufficient alone. If it was not sufficient, then undulation might complement it. The modified first hypothesis was, undulation and faulting combined, the second, faulting alone—both undulation and faulting being themselves unexplained.

It was not difficult to devise tests. An instrumental level line might be carried along the old beach so as to ascertain whether it rose or fell in regions where no faults occur; or its height might be carefully measured at two points and the difference of altitude compared with the total throw of the intervening post-Bonneville faults. The second of these tests was applied, and with success. By means of the surveyor's level the height of the old shore above the water surface at the shore of Great Salt Lake was measured at two points twenty miles apart. One of these points is on the Wasatch range near Salt Lake City; the other is on the next range west, the Oquirrh. The only post-Bonneville fault between them is that at the base of the Wasatch, and its throw is there about fifty feet, the west side having gone down. If then faulting is alone responsible for shore-displacement, the beach on the Oquirrh range should be fifty feet lower than that on the Wasatch. The measurement however showed it to be twenty-eight feet higher, and thus demonstrated a difference of seventy-eight feet to be referred to undulation.

Thus a step was made in advance, but the resulting position was not final, for the inquiring mind could find no satisfaction in the knowledge that crust undulation and crust faulting were conjointly efficient, so long as both these remained without explanation. No new hypotheses were at once invented, but it was determined to continue observation until the solitary phenomena at command were expanded into a group, and to seek new light in the classification of this group. As the basin was traversed in the conduct of the general investigation of the old lake, a search was made for the records of recent faults and at every opportunity the height of the shore was accurately measured. Six such measurements were made in the immediate vicinity of Great Salt Lake, the lake affording a common datum plane. Ten others were made on the lines of railways, where the leveling data of the railway engineers could be utilized. At some points the height was found greater than on the Oquirrh, at others less than on the Wasatch, the range from highest to lowest being 168 feet.

Faults were discovered at the bases of numerous mountain ranges, but none of them are so great as that along the Wasatch, and nearly all are very small. None were found associated with the half-buried mountains of the center of the desert, and yet on these same mountains are the highest shore records to which measurement was carried.

In general it was found that the displacements recorded by the shores have been much larger than the displacements demonstrated by faults, so that faulting can be appealed to in explanation of shore displacement only to a small extent. It was found that the throw of the faults was in some cases opposed in direction to the total deformation on the shore-line and in other cases coincident. For these reasons faulting was provisionally regarded as a disturbing factor merely, and the deformation demonstrated by the measurements of shore-height was treated as simply flexural or undulatory.

To classify the shore-heights, as a basis for further hypothesis, they were platted on a map, and their grouping was compared with geographic features. It appeared that the highest measured points lay within the area of the main body of Lake Bonneville, that the lowest points lay at the extreme north and at the extreme south, that the eastern shore of Lake Bonneville in the vicinity of Great Salt Lake was intermediate in height, and that the single point determined on the western shore of the old lake agreed in height with the eastern shore. Unfortunately, the distribution of the measurements, which had been largely determined by the distribution of railroad lines, was not equable throughout the basin of the lake, and nearly the whole of its western shore was undetermined in altitude. When lines of equal altitude were drawn among the figures representing measurements, after the manner of the isobars on a Signal Service weather map, it was found that they were not fully controlled by the determined points; but when they had been given the most satisfactory adjustment, they contoured a figure of deformation which may be characterized as a low, broad dome, having its crest over the center of the main body of Lake Bonneville, and extending a subordinate member to the region of the southern body of the lake. One half of this figure was fairly inferred from the data of observation; the remaining half was imaginary and its drawing merely gave graphic expression to the hypothesis suggested by the incomplete contours—the hypothesis that the deformation stands in some necessary or causal relation to the lake and its disappearance.

Now it has been independently determined that the cause of the lake and the cause of its disappearance were climatic; it was not drained by the wearing down of its outlet, nor emptied by the unequal uplift of portions of its rim, but it was dissipated by evaporation. If then the disappearance of the lake and the deformation of the land are connected in a causal way the change in the lake was the cause, and the change in the land was the effect. How can we suppose the drying up of the water to have produced the up-arching of the plain on which the water lay?

In the attempt to answer this question three tentative explanations were suggested, and these will be stated in the order of their origination.

It is well known to geologists that in several instances a great formation thousands of feet in thickness consists wholly or chiefly of shore deposits. To account for them it is necessary to suppose that the sea floor locally sank down as rapidly as the sediments were added. Conversely there is reason to believe that the adjacent continent, which by erosion furnished the sediment, rose up as rapidly as its surface was degraded. It is a favorite theory—at least with that large division of geologists who consider the interior of the earth as mobile—that the sea-bottom sinks in such cases because of the load of sediment that is added and that the land is forced up hydrostatically because it is unloaded by erosion. A similar theory might explain the up-arching of the desiccated bed of Lake Bonneville, for the unloading of 1000 feet of water from an area more than one hundred miles across would give to the supposed liquid interior an irresistible uplifting force. This was the first explanation to suggest itself.

The second suggestion did not spring from any geological theory consciously retained in memory, but I have since suspected that the germ of the idea may have been caught from a passage in Croll's 'Climate and Time.' It is this: The geoid of which the ocean's surface is a visible portion is not an ellipsoid of revolution, but differs from that symmetric surface by undulations which depend on local inequalities in the density and in the superficial configuration of the earth. The water level is everywhere normal to the plumb-line, but the plumb-line, as geodesy has shown, is subject to local deflection. Now the ocean itself is one of the attracting factors, and if the ocean were to be removed, the geoid would thereby be modified. The surface of Lake Bonneville was part of a geoid at a higher plane than that of the ocean surface, and the removal of the water of the lake unquestionably modified the local form of the geoid. Only at first blush the cause seems too small for the effect observed.

The third suggestion relates to the distribution of temperatures beneath the surface of the earth. It is well established that the inner parts of the earth are extremely hot. The outer surface is relatively cool, and in the intermediate region there is a gradation of temperatures. The isogeotherms, or planes of equal temperature, are not even surfaces, but undulate in response to variations of conductivity and of superficial temperature. At the poles, where the external surface of the crust is exceptionally cold, the isogeotherms lie lower down than in warmer latitudes; and if a portion of the earth's surface undergoes a permanent change in temperature, the influence of this change is propagated slowly downward through the crust, and the isogeotherms are locally raised or lowered. Where they are raised, the crust is locally expanded, and its surface is uplifted; where they are depressed the surface of the crust subsides. If, therefore, it can be shown that the temperature at the bottom of Lake Bonneville was raised in connection with the dessication of the lake, we have a true cause of upward movement, and if we can show furthermore that the temperature of the surrounding region was not equally raised, we have at least a qualitative explanation of the differential uplift, the phenomenon to be accounted for.

It is now several years since these explanations were first suggested, and subsequent reflection has developed no others. While all of them appear perfectly rational, only a very slight inspection was necessary to raise a doubt as to the quantitative sufficiency of the second and third. It was therefore determined to ascertain as accurately as possible the maximum change which might be ascribed to each of the three suggested causes, and to compare it with the actual change. The actual change is susceptible of various statements. If we consider only the measurements on the margin of the main body of water, and in its center we find a difference of 100 feet; by including observations on outlying bays we get a maximum difference of 168 feet; and a study of the peripheral slopes of the uplift suggests that they extend somewhat beyond the boundaries of the lake. Crude extrapolation gives 200 feet as a maximum estimate of the height of the crustal dome.

Take first the hypothesis that the crust of the earth, floating on a molten nucleus, rose up in the region of the basin when its weight was locally diminished by the removal of the water of the lake. The weight of the load removed is measured by the depth of the water before evaporation, 1000 feet. The theory supposes that as the crust rose there flowed in beneath enough molten rock to replace the weight of the evaporated water. If the rock was very heavy, a layer of moderate depth was necessary; if it was less heavy, more was required; but in any event the thickness of the introduced layer must be equal to the amount of the superficial uplift. It is known that the density of the earth's material increases downward, for the mean density of the earth, expressed in terms of the density of water, is about 5·5, while that of the upper portion of the crust is about 2·7. Nothing is known however of the law under which the density increases, and nothing is known as to the depth of the zone at which matter is sufficiently mobile to be moved beneath the Bonneville basin. We may, however, indicate limits, and I think this is fairly done by assuming that the density of the introduced matter was not less than 3, nor more than 5·5. If it was 5·5, the uplift consequent on the evaporation of 1000 feet of water would be 182 feet. If it was 3, the uplift would be 333 feet. Now it has already been stated that the greatest value observation suggests for the amount of the uplift is 200 feet. The postulate is therefore abundantly competent in a quantitative way.

To evaluate the effect produced under the second hypothesis (the hypothesis, that is, that the geoid represented by the water surface of Lake Bonneville has been deformed by the withdrawal of the attraction exerted by the water itself) it is necessary to employ mathematical analysis of a high order. As my schooling in mathematics did not qualify me to undertake this, I submitted the problem to an eminently competent colleague, who has solved it rigorously and deduced for the deformation of the geoid within the area of Lake Bonneville a maximum amount of two feet. The second explanation is therefore eliminated from consideration, because quantitatively insufficient.

It remains to consider the rise of the isogeotherms, and to evaluate the resulting elevation of the basin. It has been established by numerous observations that in all lakes having a depth as great as 1000 feet, the temperature at the bottom is about 39° F. This depends upon the fact that water at that temperature is heavier than at any other, and having once reached the bottom of a deep lake, it is withdrawn from the circulation to which the upper layers are subject, and remains undisturbed. The meteorological records show that the mean annual temperature of the desiccated basin of Lake Bonneville at the present time is 52°. The change from a humid to an arid condition has therefore raised its temperature 13°. The temperature of the surrounding regions has at the same time undergone a change, of which we have no precise estimate. The epoch of Lake Bonneville was the Glacial Epoch, and the local climate was then in all probability cooler. If it was 13° cooler, the isogeotherms would be no more affected at the center of the basin than at its margins, and there would be no differential elevation. If it was cooler by less than 13° a differential uplift would occur. For the sake of giving this uplift a maximum value, we will assign a very small figure to the general change of temperature, namely 3°, and assume that the differential change with respect to the basin was 10°. A formula devised by Fourier enables us to estimate the rise of the isogeotherms, if only we know the conductivity of the material of the earth, and the time which has elapsed since the Bonneville shore line was carved. Then, if we know additionally the rate of expansion of rock for a degree of temperature, we are able to estimate the upheaval. Sir William Thompson has determined experimentally a coefficient of conductivity. The late Prof. Bartlett, of West Point, has determined the coefficient of expansion for several building stones, which may be assumed to represent the crust beneath the Bonneville basin. We do not know how these coefficients are affected by high temperatures and great pressures, such as exist deep in the crust, and an element of uncertainty attaches for that reason.

In order to obtain a maximum result despite this uncertainty, I have made an extreme assumption in regard to time. The shore line of Lake Bonneville is in a wonderfully perfect state of preservation. While one stands upon it, it is easy to believe that it is but a few centuries old, and the geologist, accustomed as he is to the contemplation of eons of time, hesitates to estimate its antiquity in greater units than thousands or at most tens of thousands of years. When therefore we postulate its antiquity at one hundred millions of years, we pass so far beyond the range of probability as to protect ourselves against a possible underestimate. With these data the computation has been made, and it has been ascertained that a maximum uplift of 36 feet* can thus be accounted for. Since observation shows an uplift of not less than 100 feet, the thermal explanation is shown to be entirely inadequate; and if we were able to substitute for our imperfect data the actual data, we should probably find the computed uplift too small to be taken into consideration.

If therefore we admit that the removal of the water of the lake was the cause of the upheaval of the lake-bottom, there seems no way to avoid the conclusion that the efficient *modus operandi* was an upbending of the solid crust of the earth, caused by hydrostatic pressure communicated through a mobile substratum. But we are far from being forced to that admission. The coincidence in locus of the uplifted dome and the Quaternary lake may have been fortuitous; or there may even have been no coincidence, for the contoured figure of deformation was in part supplied by the imagination; and in either of these cases we can fall back on the agnostic hypothesis of unexplained undulation. In the present state of observation and inference the hypothesis of the hydrostatic restoration of equilibrium by the underflow of heavy earth-matter is the only explanation which explains, and none of the observed facts antagonize it; but the alternative hypothesis is not barred out.

To reach a satisfactory conclusion more observation is necessary, and this discussion of the subject would be premature were it not that the necessary observation is very expensive, and there is no immediate prospect that it will be supplied. It is fitting, however, that the desirable lines of research be pointed out.

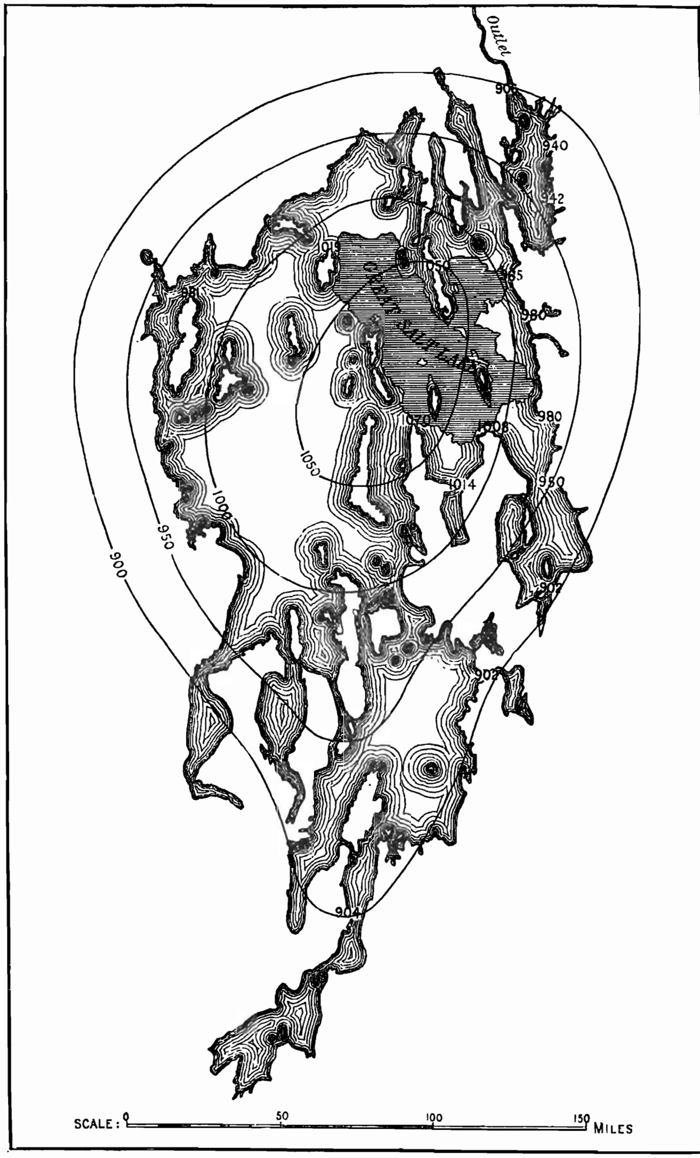
The undertaking that promises most is an exhaustive hypsometric survey of the Bonneville shore line, including all bays and islands. If this were executed, it would be possible to deduce

* In the original paper, as read, 12 feet instead of 36 were erroneously given. A friend has since pointed out that the estimate of 12 feet includes expansion in the vertical direction only, whereas the coincident horizontal expansion would almost necessarily be converted into uplift.

a much more satisfactory expression of the shape of the uplift, and to determine either that it is intimately related to the form of the body of water removed, or that it is not so related. If the relation were demonstrated, the observations might so far indicate its nature as to render possible an evaluation of the rigidity of the earth's crust.

Another profitable method of continuing the inquiry would be to make a similar investigation of the shore of another extinct lake, for example, the one to which Clarence King has given the name of Lahontan, and which ranks second to Lake Bonneville among the Quaternary lakes of the Great Basin. If in a second instance the center of the desiccated lake were found to be the locus of upheaval, the hydrostatic theory would be practically established.

It is hardly necessary for me to assure you that my personal regret in abandoning this research at its present stage is very great. I have discussed it as an investigation of the deformation of the Bonneville basin, but it has a broader meaning. The condition of the interior of the earth is one of the great problems of our generation. Those who have approached it from the geologic side have based a broad induction on the structural phenomena of the visible portion of the earth's crust, and have reached the conclusion that the nucleus is mobile. Those who have approached it from the physical and astronomic side have reached the conclusion that the nucleus is rigid. Here seems an opportunity for a crucial observation. If the crust of the earth floats upon a fluid nucleus, the evaporation of Lake Bonneville, by lifting from it a great weight, must have produced an uplift of determinate form. If the whole earth is solid, such a result could not have been wrought. The decisive phenomena are known to exist, and to be accessible, but they are scattered over a broad desert, and they can be gathered in only at the cost of much money and great labor.



Map of the extinct Lake Bonneville, with hypothetic contours to illustrate the subsequent deformation of the earth's crust, by G. K. Gilbert. The horizontal figures mark determinations of the height of the old shore-line above Great Salt Lake.