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G. K. GILBERT AND MODERN GEOMORPHOLOGY

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ABSTRACT. A review of Grove Karl Gilbert's contributions to geomorphology yields the following suggestions for future geomorphic research: (1) The explosion of geomorphic data and the profound change in the tools for landform analysis (remote sensing, computers, systems theory) must not be allowed to let the goal of research become information alone, rather than the discovery of relations among phenomena in all their complexity. (2) A new theory for Earth structure needs to be tested, modified, and incorporated into geomorphology. (3) The dynamical basis of geomorphology should continue to be the focus of modern research with the thermodynamic analogy and systemic models guiding the search for economy in geomorphic systems. (4) Quaternary studies should continue to be pursued by interdisciplinary approaches for insights into the dynamics of surficial processes. (5) A new frontier for landform analysis lies in studies of the surfaces of other planets. (6) Processes on our planet must be viewed as open systems. The more significant studies of Earth surface systems will continue to emphasize their timeless rather than their timebound components. (7) Models of those systems (for example, flumes) should be pursued for their insights into systems interactions, but geomorphologists must remain wary of the limitations of both models and incomplete empirical studies for the precise prediction of complex natural phenomena. (8) Geomorphology must increasingly apply its method to predicting the systemic interactions of man and surficial processes. (9) The insights that geomorphology can provide in environmental analysis must be made known to society's decision-makers.

INTRODUCTION

Geomorphologists, like all scientists, entertain a curiosity as to the trends for future research in their discipline. Recent attempts to predict the direction of modern geomorphic inquiry have attempted either to analyze the topics of current concern (Dury, 1972) or to envision the formulation of a new theoretical framework (Higgins, 1975). In this paper we will consider tomorrow's geomorphology by analyzing some of the scientific work of a man who appears, nearly a century ago, to have anticipated much of today's geomorphology. Although G. K. Gilbert's role in modern geomorphology is already appreciated (for example, Strahler, 1952), we feel that much of his work continues to offer insight into the future of a science with which he particularly identified.

Grove Karl Gilbert (1843-1918) is justifiably recognized as one of the grand figures of the heroic age of American geology. It is curious that this much honored geologist, the only person ever to serve twice as president of the Geological Society of America, was indeed somewhat segregated from the intellectual themes of many of his contemporaries.

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He preferred the study of modern processes to the study of ancient Earth history. His major research tool was the physical model rather than the fossil. He avoided aspects of geology that treated Earth history as a directed, irreversible process. Indeed, he remained largely uninterested in the central debate of his era — the age of the Earth. The enigma of the reverence for Gilbert by modern geologists versus Gilbert's dissociation with the largely historical science of his contemporaries has even led to one recent study (Kitts, 1973) that mistakenly attempted to describe Gilbert's science as "primary historical inference."

Another irony of history is that, prior to the research by Pyne (1975, ms), the principal source of bibliographic information on Gilbert was the lengthy memoir prepared by William Morris Davis (1926). Although the relationship between these two men was cordial, frank, and of genuine intellectual power, Davis appears not to have understood Gilbert's science any more than Gilbert did Davis's. In the memoir Davis tried to incorporate Gilbert into the theoretical superstructures of contemporary geology. In particular, Davis attempted to place Gilbert into the context of Davisian geomorphology. Yet the grand Davisian synthesis has now fallen to disfavor (Flemal, 1971), not so much for errors (it remains an elegant teaching tool), but because it cannot address the important new geomorphic questions of our time. Whereas Davis searched Gilbert's writings for antecedents to his own geographical cycle, many modern geomorphologists now review those same passages for refutations of the Davisian theory.

Davis meant his system to form the basis for future deductive reasoning in geomorphology. Gilbert had no such pretentions. Merriam (1919, p. 396) notes that Gilbert was "an authority in many fields, and yet one who never assumed authority; a leader in science, and yet one who never assumed leadership; neither power nor glory did he seek, but the satisfaction of contributing his share to the sum of human knowledge." Gilluly (1963, p. 221) has perhaps provided the best expression of Gilbert's influence: "His significance was not revolutionary, like Gibbs, but like Robert Welkman, our first Nobel laureate, in the general excellence of his works. In his example rather than in the novelty of his philosophy is the reason for the tremendous impact of G. K. Gilbert upon American and world geology." Let us consider this example in relation to the future of geomorphology.

METHOD

In 1866, when the 23-year old Gilbert was employed as a specimen curator, an incident occurred that foreshadowed the subsequent course of his scientific career. The partial skeleton of a mastodon was discovered in a pothole near Cohoes Falls on the Mohawk River. Gilbert wrote an account of the incident for a local newspaper in March, 1867, and met the great paleontologist James Hall at the excavation site. Because Hall injured himself by falling into the pothole, Karl supervised the remaining excavation. With E. E. Howells he visited Boston where two reconstructed mastodon skeletons were available for study. By comparing the bones uncovered at Cohoes with the complete Boston skeletons, they were able to reassemble and reconstruct the new skelton. However, what attracted Gilbert to this problem was not its stratigraphic or biological implications. His interest became centered on the potholes. He counted them (350), measured them, described their shape (like a "chemist's test tube"), and proposed that they were produced by the grinding action of stones moved by water. Then he attacked the problem of the falls itself. To date the falls, he measured the growth rings of cedar trees which grew downstream from the falls. By comparing the distances between these trees and the years that separated them, he could estimate an average rate for the recession of the falls. In his first scientific paper, Gilbert (1871) calculated a retreat of 30 cm (12 in.) a century and approx 35,000 yrs as a "minimum for the time that has elapsed since Cohoes Falls were opposite the Mastodon pothole."

Gilbert (1886) later reflected on the goals of scientific research. With 15 yrs more experience behind him, he concluded that the major scientific goal of discovering relations among phenomena was not adequately served either by "induction" or by "relational classification." The former tended only to provide superficial relations, and the latter, while providing "logical" or rational understanding, could never produce truth by a linear chain of sequence that eliminated relevant data. Gilbert's disdain for classification was matched by his conviction that phenomena could not be simply analyzed for cause and effect. Rather he believed that "antecedent and consequent" relations constitute a "plexus" that pervades nature. Because of this complexity of information and relationships, Gilbert argued that the scientist must compromise his method by using the "working hypothesis." This technique is intermediate between pure Baconian induction and modern model building. It assumes the existence of natural laws, so that the explanation it offers is somewhat more than a mere heuristic device. Rather the etiological web is too complex and the data are too abundant to unravel by simple induction. Moreover, the methodology of working hypotheses recognized the inevitable psychological bias present in any "fact." Hence the conclusion that hypotheses must be tested in competition with each other.

The corollary to the need for hypothesis testing is the need to create hypotheses. For Gilbert the process of making hypotheses amounted to inventing analogies. For this there was a formal procedure (Gilbert, 1886, p. 287):

> 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.

The "analogic proportion" which resulted imitated strikingly the form of Gilbert's mathematics and prose. All were constructed as ratios, that is, with a certain structural proportion. The role of experimentation, the creative process of scientific thinking, was to supply and refine the analogies that served as hypotheses.

Gilbert was the first to disown any startling originality in his discussion of scientific method. Indeed it is Chamberlin's (1897) subsequent writing on the subject that is most often quoted. Nevertheless, Gilbert was the real source of this method in geology. The unique aspect of Gilbert's approach was his insistence that "example" rather than "precept" constituted the method of scientific education and that scientific thinking was fundamentally analogic thinking. It is fitting then that Gilbert's impact on geomorphology is one of example, stemming from his beautifully reasoned monographs and papers, rather than from any chance discovery of new phenomena or from a grand synthesis of explanation. What Gilbert (1885, p. 237) wrote about the work of Archibald Geikie could be applied equally to his own work: "As in all his writings there is nothing sensational, either in description or in speculation. His inductions are not expanded into brilliant, universal theories, but are modestly advanced with all those limitations which impress themselves on the mind of one who constantly questions nature."

What is the relevance of Gilbert's method to modern geomorphology? Geomorphology is today a science transformed by an explosion of new data and a profound change in the tools available for geomorphic analysis. Data on the surficial environment are being generated as images from discrete bands of the spectrum taken by sensors on orbital and suborbital platforms. Pertinent information on Earth-surface form and process is being collected by engineers, ecologists, pedologists, sedimentologists, geochemists, and others. The integration of this information cannot take place under the old evolutionary scheme of the Earth. The new tools for coping with abundant data, computers and associated aids, operate in a systems or cybernetic framework. Data are treated statistically, and, although deterministic hypotheses are still posed, results of individual measurements invariably are found to scatter about regression lines. Random models of drainage networks, fluvial patterns, sediment transport, and other geomorphic phenomena are increasingly being offered as predictive tools for estimating the values of geomorphic variables. Do these new tools constitute the new method of geomorphology?

Gilbert's contemporaries used maps, sections, and photographs to inventory information and to extrapolate from known to unknown phenomena. Many of the new geomorphic tools, such as remote sensing, are also magnificent devices for inventory, saving time and freeing the investigator to analyze relationships. However, as important as the task of inventory may be, it does not constitute the major goal of geomorphology. Gilbert would have liked Brian Bayly's (1968, p. 120) explanation of this point: "Science is not the orderly accumulation of facts; it is the orderly accumulation of rejected hypotheses."

THE STRUCTURAL BASIS

In 1871, after a brief stint with the Ohio State Geological Survey of John Newberry, Gilbert became a geologist in the Wheeler Survey. In three years of touring Nevada, Utah, Arizona, and New Mexico, Gilbert was introduced to the geological problems of the Colorado Plateau and the Great Basin that formed the basis for much of his later work. Gilbert's primary insight during the Wheeler Survey was that vertical tectonics dominated in both regions. The Colorado Plateau was an elevated mass of horizontal strata, whose surface rippled with plateaus and mesas, while the Basin Range (whose strata were complexly folded) represented blocks of crust alternately dropped into parallel troughs. Yet the faults in each case formed rhythmic belts; their features held consistently over broad areas. The invocation of vertical tectonics was a novel idea, for the reigning theories of mountain building involved lateral compression and crustal shortening that was linked by geophysical theory to the concept of a contracting Earth. So pervasive was the Appalachian model for mountain building that King (1878) initially interpreted the Basin Range mountains as the crests of anticlines and the intervening valleys as synclinal troughs. Gilbert (1875, p. 61) carefully contrasted the two systems as follows:

> In the Appalachians corrugation has been produced commonly by folding, exceptionally by faulting; in the Basin Ranges, commonly by faulting, exceptionally by flexure. The regular alternation of curved synclinals and anticlinals is contrasted with rigid bodies of inclined strata, bounded by parallel faults. The former demand the assumption of great horizontal diminution of the space covered by the disturbed strata, and suggest lateral pressure as the immediate force concerned; the latter involve little horizontal diminution, and suggest the application of vertical pressure from below. Almost no eruptive rocks occur with the former; massive eruptions and volcanoes abound among the latter, and are intimately associated with them.

However, Gilbert's analysis of the Basin Range province remained controversial. He returned to the field area again and again, long after the demise of the Wheeler Survey. Eventually a classic monograph (Gilbert, 1928) was published posthumously that resoundingly confirmed many of the insights of his scientific youth.

In Gilbert's day geomorphology, even Gilbert's process-oriented geomorphology, required a firm diastrophic basis. Gilbert was in the forefront of both structural geology and geomorphology. He continually tested hypotheses for various Earth structures by seeing if they were consistent with the landforms developed upon them. Given this hindsight, the relatively small amount of attention that modern geomorphologists have given to the new plate-tectonic model is an enigma. Dewey and Bird (1970), for example, have illustrated several types of plate interactions that have profound geomorphic implications. Future attention to the plate-tectonic model will undoubtedly yield important new hypotheses for a number of perplexing structural-geomorphic problems. The origin of the transverse drainage in the Appalachians is a case-in-point (Judson, 1975).

FORCE AND RESISTANCE

In 1875, Major John Wesley Powell enticed Gilbert to join his survey. That summer found Gilbert back on the Colorado Plateau, traversing the Waterpocket Fold. As he approached the Henry Mountains on August 23, he had already formulated a conception of their structure. His field notebook for that day contains the following entry: "My idea of yesterday in regard to H.M. are confirmed by this view. It is in bubble form or tumor form, the strata being nearly level on top and the crusts controlled by dikes which are radiate in form." In short, Gilbert had conceived the structure of the mountains before he ever actually visited the scene. By the following day he had an explanation for the erosional processes operating on the scene as he wrote in the notebook, *"A General Note on Terraces"*—which he conceived as "not built of *debris* but of rock in *situ* capped by debris."

In September of the next year Gilbert returned to the Henry Mountains for a two-month study of the problem that he had only observed in passing during the previous field season. The field work went quickly because Gilbert knew exactly what he considered to be the important aspects of the study. Even when intense rains stymied his usual round of surveying, Gilbert organized his thoughts on the structural meaning of the Henrys over the course of some 27 pages in his notebooks. Systematically addressing each aspect of the problem, the notes present the entire theory of the laccolith almost exactly as it appeared in the final monograph.

Gilbert's (1877) analysis of the Henrys was published in the 150-page monograph, *Report on the Geology of the Henry Mountains.* The first of Gilbert's major works, this report contained the most succinct statements of the scientific philosophy that he elaborated in subsequent, more lengthy monographs. Paramount was the use of dynamic equilibrium to organize geologic explanation on a foundation of physical laws. Rather than elaborating Powell's genetic classifications of rivers and mountains, Gilbert chose to cite analogies to the mechanics of flexed beams and hydrostatic equilibrium.

In his structural analysis of the laccolith process, Gilbert constructed a conceptual model for comparison to the actual field record. Laccoliths result from applying the law of hydrostatic equilibrium and the principle of least action. Magma rises with a force derived from the fact that its density is less than that of the surrounding rocks. Consequently it rises until it reaches a level where the pressure from the overburden equals the pressure by which it rises. At this level, the Henry Mountain magma insinuated itself into horizontal bedding planes in the stack of rock strata, preferentially selecting zones of weakness. As more magma is supplied, the resulting sill is subjected to two confining pressures: a lateral resistance to outward spread and a vertical resistance created by the pressure of the overburden. To create a laccolith, the lateral resisting forces must exceed the vertical ones. This circumstance can result because the resistance to horizontal spread increases as the radius of a sill increases. Eventually this resistance exceeds that from the overburden. The size of the sill at this point of transformation into a laccolith varies with the amount of overburden. The magma then lifts the overburden until a new equilibrium of pressure, compounded both of overburden and the laccolithic magma, is reached. Thus, there exists a direct relationship between the size of a laccolith and its original depth of burial.

Gilbert was able to express his mechanical analysis quantitatively. He established the correlation between the size of laccoliths (their limital areas and thicknesses) and the amount of overburden. This subsequently allowed him to predict how much strata had been eroded from above the laccoliths since their emplacement. Subsequent analyses (Johnson, 1970) have used differential equations to elaborate this explanation, but the fundamental principles remain those originally expressed in Gilbert's first approximation to the problem.

The final third of Gilbert's *Henry Mountains* was devoted to erosional processes. Having described the uplift in dynamic terms, he described the erosion by streams according to the concept of energy. The stream, like the uplift process, was analogous to a machine that performed work according to the laws of thermodynamics. The conservation of energy and the principle of least action go a long way in understanding Gilbert's qualitative description of rivers. By applying them he arrived at concepts of "equilibrium of action" in which slope and channel cross sections were adjusted to variations in flow velocity and sediment load. Larger scale concepts of equilibrium were embodied in his three laws of stream erosion. The "law of uniform slope" held that since erosion depended on stream energy, and energy on velocity, the steep portions of a stream's course should erode more rapidly than gentler sections, with the tendency to abolish all differences of slope and produce uniformity. Obviously, this law works against the "law of structure", which holds that hard masses resist erosion better than soft ones. A balanced landscape with all elements in continuing equilibrium is maintained by the "law of divides", which states that "the nearer the water-shed or divide, the steeper the slope; the farther away the less the slope."

None of these ideas were strictly original with Gilbert (*see* Knox, 1975). Indeed he himself had formulated two of the "laws" in a preliminary manner for his Wheeler report and perfectly encapsulated the graded river concept in an earlier article (Gilbert, 1876). What distinguished the *Henry Mountains*, however, was its expression, which was succinct (with crisp, balanced sentences that read like prose equations), and its foundation in the energy concept. This brought more exactitude to stream studies than natural historians had managed to convey with their sense of the "organic" unity of river networks and more breadth than French engineers had conveyed with their study of canals "in regime."

Gilbert's methodology was probably less similar to that of his fellow geologists than to that of contemporary physicists and chemists exploring thermodynamics. Macquorn Rankine, whom Gilbert frequently quoted, was a probable model. Rankine introduced the distinction between potential and kinetic energy and translated this abstract physics into the problems of civil engineering. Perhaps the most revealing parallel is between Gilbert and Willard Gibbs, a man he never met and probably never read. Gibb's papers on equilibrium in chemical systems appeared in almost the identical year as the *Henry Mountains.* In both cases the fundamental achievement was to extend the range of thermodynamics into novel systems of nature. The organizing concepts that resulted were remarkably similar. For Gibbs, a chemical system existed in thermodynamic equilibrium; if the thermodynamic variables changed, a readjustment in equilibrium occurred through a change of "phase." For Gilbert, a river system existed in fluvial-dynamic equilibrium; with a change in the independent variables (sediment load and water), a readjustment in equilibrium came through a change in "grade." Each formulation yielded a surface of equilibrium.

When Gilbert codified the processes of a graded stream, he did for geology and hydraulics what the phase rule did for physiochemical systems — it not only rationalized the known data, but it introduced predictability into the behavior of the systems. Both concepts now operate in geologic phenomena, and each describes a form of metamorphism. With a change in temperature or pressure, a change of phase reconstructs the chemical system, the rock. With a change of sediment load or water discharge, a change in grade reconstructs the fluvial system, the river.

The thermodynamic analogy has become a major theme for research in the modern geomorphic era. The early interest concentrated on mutual adjustment between opposing forces, as in the equilibrium profiles of hillslopes (Strahler, 1950). When Hack (1960) advocated a principle of dynamic equilibrium to explain entire erosional landscapes, he noted that the basis of his approach was the same as applied by Gilbert to the Henry Mountains. Leopold and Langbein (1962) used the thermodynamic analog to evaluate the role of entropy in landscape evolution. Scheidegger (1967) expanded this to a complete thermodynamic analog for landscape evolution. Bagnold (1966) tackled the perplexing problem of fluvial sediment transport dynamics by the analogy of fluvial transport to an engine. All these ideas follow Gilbert's lead in analyzing a variety of geomorphic processes as physiographic engines striving for efficiency but operating in various equilibrium states as dictated by the interplay of force and resistance.

Thermodynamic concepts gave nineteenth century physics and chemistry important tools for prediction. This goal of prediction is precisely why the thermodynamic analog continues its appeal in modern geomorphology. It is really a very general modelling scheme, which does not necessarily require hardware (for example, flumes) or mathematical sophistication. As geomorphology increasingly becomes a predictive rather than a descriptive science, various forms of model building will be incorporated (Chorley, 1967). Although seen by Gilbert a century ago, this realization has only recently permeated our science.

If models are an appropriate goal for modern dynamic geomorphology, then systems analysis must be its forecourt game. Chorley (1967, p. 77) defines a geomorphic system as an integrated complex of landforms which operate together according to some discernible pattern. As in thermodynamic systems, we now recognize that geomorphic systems have "states," controlled by mass and energy flow, composition, and organization (Howard, 1965). They have "phases," expressed as the number of dimensions (Melton, 1958). A complete organization for the study of systems interaction has now been advocated (Chorley and Kennedy, 1971). One challenge for the future is to see if the new tools appropriate for this approach (computers, statistical analysis) can keep up with the mushrooming amount of data and the pressing needs for answers, especially in applied or "environmental" geomorphology. The other challenge is to cope with the scientific limitations of systems theory.

In a sense, modern geomorphologists are now attempting to use systems analysis bolstered by quantification to describe carefully the "plexus" of antecedent and consequent relations that Gilbert envisioned in geomorphic phenomena. The pitfalls in a short-sighted approach of this type have been well-described by Mackin (1963). The investigator must not discard reason to achieve statistical objectivity. Because systems require arbitrary definition, they may exclude phenomena that are later determined to be significant. Systems theory gives an approximation to truth, streamlined to achieve the practical goal of prediction. The geomorphic systems analyst must continually balance his arbitrary simplification against the realities of nature. No better example for this necessary scientific endeavor exists than in the work of Grove Karl Gilbert.

QUATERNARY DYNAMICS

When Gilbert began his studies of Lake Bonneville in 1879, only the broadest themes of its paleoclimatic history were known. Clarence King, then director of U.S. Geological Survey, had described the region during his celebrated survey of the 40th Parallel. King had even worked out a sequence of wet and dry epochs from the chemistry of sediments deposited in Pleistocene Lake Lahontan, a pluvial lake closely related to Bonneville. Gilbert himself during the Wheeler Survey of 1871 had deduced that the story of Bonneville was that of a basin that had episodically flooded and drained. Sediments, alternately of lakes and of alluvial fans, told the general story, while the shorelines etched into the Wasatch related the most recent episode. In this case the lake waters had gradually risen, although with considerable oscillations, until they finally crested through a mountain pass. The pass eroded, dropping the lake level until its erosion eventually slowed, and, acting like a spillway, U.S. Geological Survey

PLATE I

Lake Bonneville Plate IX

The great bar of Lake Bonneville at Stockton, Utah. Gilbert is depicted in the foreground with planetable (pl. 9, U.S. Geol. Survey Mon. 1).

it regulated the lake level at a particular elevation. With a change in climate evaporation reduced even that lake to a vestigial relic — the Great Salt Lake in Utah.

Gilbert directed his Bonneville research toward what he considered to be the most important specific questions in the paleohydrologic reconstruction of the lake: the processes of bar formation, the location of an outlet, the measuring of bars (pl. 1), once level, but now warped, a quantitative study of changes in water level, and so on. His work on the problem continually accelerated as new questions gained significance from the field relationships. Gilbert installed gages for measuring the fluctuation of the modern Great Salt Lake, he conducted experiments on the sedimentation of evaporites, and he completed a careful essay on hypsommetry. Unfortunately, his train of thought was cut in 1881. Clarence King had resigned the directorship of the U.S. Geological Survey. His successor, Major John Wesley Powell, transferred Gilbert to administrative duties, supervising the Great Basin studies of I. C. Russell, W J McGee, and Willard Johnson. It took nearly a decade before Gilbert's report was finally published.

Lake Bonneville was one of the consummate works of nineteenth century American geology; Gilbert himself called it his magnum opus. That methodology which he had pioneered with distilled, epigrammatic style in the *Henry Mountains*, he amplified and embellished in *Lake Bonneville*. Gilbert (1890) stated a twofold theme for the work: "the discovery of the local Pleistocene history and the discovery of the processes by which the changes constituting this history were wrought." Gilbert's history, however, did not relate to the stock chronologies of historical geology; there was no stratigraphy, no paleontology (in the usual sense), and no evolutionary summary of the Bonneville region. Instead his correlations were founded on "physical evidence," the most important of which was the record of waves. The effort to correlate correctly the physics of wave processes with the sculpturing of distinctive topographic features is the dominant theme of *Lake Bonneville*.

Most geologists in Gilbert's day organized their reports as a progressive history of geological events. Gilbert organized his major works as a study of dynamical geology. First, by axiomatizing shoreline processes, he made it possible to deduce certain topographic forms logically rather than historically. Second, by establishing a physical horizon, the shoreline, rather than a fossiliferous or stratigraphic one, he eliminated the need to rely on precisely those tools that led others into evolutionary interpretations of the Earth. Third, he extended the mechanical analogy he had designed at the Henry Mountains to explain the Bonneville topography. By equating geologic processes with mechanical forces, Gilbert was able to organize those processes by the same principles that engineers were applying to structures and machines — namely, equilibrium.

The Bonneville story had implications beyond the mere rise and fall of lake levels. The volume of water in Bonneville had represented a substantial load applied to the Earth's crust; its removal an equal unloading. The story was incomplete, until an equilibrium was reached. The response of the crust was a question in mechanics of the sort that delighted Gilbert. His precise leveling of the Bonneville shorelines had begun in 1872. The data revealed a warping that increased systematically as one moved to the center of the lake. This suggested to Gilbert that the upwarping was a consequence of stress release from the rapid removal of lake waters. Clearly, the weight of Lake Bonneville had been a load sufficient to deform the crust. Yet the Wasatch Mountains adjacent to Bonneville were continuing to grow. That is, the weight of these mountains was not sufficient to depress the Earth. Gilbert had gained an important insight into the remarkable physics of the Earth's crust. It was not always as plastic as the geologists of his day held, nor was it as rigid as the contemporary geophysicists maintained. Gilbert's study had cleverly transformed the surface geology of Lake Bonneville into a tool for exploring the physics of the Earth's interior. In that sense *Lake Bonneville* epitomizes nearly all his work in geomorphology.

Gilbert continued to study the Pleistocene throughout his life, but he did so for his own special purpose. His single most comprehensive study of glaciation was done in 1899, when he served as a member of the Harriman Expedition to Alaska. Gilbert's study used the same formula he employed for structural evolution, land forms, and gravity: the surface of the Earth expressed a competition between uniformity and variety. A large force — climate, gravity, or erosion — dissolved into a mosaic of particularized forms according to the local resistances offered it. This was Gilbert's answer to the theories of glaciation offered by Whitney, Croll, Chamberlin, and others. When he described glacial motion, he adjusted old hydrodynamic concepts to the new engine of glaciation: the glacier behaved like a stream except that viscosity rather than momentum was the decisive variable. Glacial work proceeded by processes of both abrasion and plucking. Abrasion was a function of four principle factors: velocity, pressure, the material abraded, and the quality and quantity of abrasive particles. Viscosity was an intervening variable in each case; the amount of velocity, pressure, and resistance to abrasion varied according to the internal resistance to flow present in the glacial ice. Even with this modification, the outcome of glacial activity, as with streams, was to equalize its work, or "to reduce the profile of the bed to simple forms." The chief measure of glacial energy was velocity, and "most of the inequalities of velocity are determined by gravity in conjunction with the friction of the ice on the channel and the resistance of ice to internal shear; and the processes are essentially the same as with water" (Gilbert, 1910, p. 203).

Gilbert's (1906) brief study of the crescentic gouge, a relatively minor erosional form, is a clear example of his logic for the study of process. The crescentic gouge had a shape similar to the "chattermark" identified by Chamberlin, but a different genesis. Gilbert imagined the gouge as geometrically and physically related to the "conoid of percussion." While the conoid is normally the product of a sharp blow, Gilbert conceived an alternative force in the differential pressure of a glacier acting on a boulder, where the boulder is positioned on a prominence and cushioned by sand and other basal debris. Actually rupture occurred along two stress surfaces — one was the conoid of percussion caused by compressive stress exerted downward or obliquely; the other, a tensile stress that produced a vertical face around the crescent. The pattern of the gouges was rhythmic, so Gilbert reasoned that the glacier must experience a rhythmic movement. Moreover, the required forces exerted by the ice on the boulder led to implications of glacial viscosity. To achieve this viscosity Gilbert conceived glacial flow, as Chamberlin did, as a product of interstitial melting and regulation by rigid crystalline grains.

Although Gilbert studied glaciation and its indirect effects throughout his life, he never attempted to derive a major theory of glaciation nor a detailed chronology of Quaternary events. What appealed to him was not glacial history but the freshness of the glacial record, and not glacial stratigraphy as much as the mechanics of glacial flow. Both his study of crescentic gouges and of isostatic rebound at Lake Bonneville, after all, used geomorphic evidence to analyze certain geophysical processes like the internal motion of glacial flow and the plasticity of the crust. His correspondence to Clarence King shortly after beginning the Bonneville study beautifully illustrates his thoughts (Gilbert, 1880, p. 24): "To the geologist, accustomed to speak familiarly of millions of years, it was the veriest yesterday when all these things were wrought; nor can anyone who stands on the quartzite shingles of one of the old beaches, and contemplates the rounded pebbles, gleaming with the selfsame polish they received when the surf laid over them, fail to be impressed by the freshness of the record."

On the first page of the new journal *Quaternary Research,* A. L. Washburn (1970) restates the reason for Gilbert's fascination with Lake Bonneville and the Pleistocene — the same reason modern geomorphologists must analyze the Quaternary. It is the converse of the famous dictum by Gilbert's good friend Sir Archibald Geikie, "the present is the key to the past." Gilbert's study of an extremely well-preserved record of Quaternary processes provided magnificent insights into the operation of processes active in modern environments. These insights can be used equally well to elaborate the general nature of landscape-forming processes or to establish the implication of those processes for man. Gilbert understood the political and economic meaning of the Bonneville paleoclimatic record. "The history of Bonneville," Gilbert (1890) wrote, "is therefore the history of the ancient climate of Utah and is thereby closely linked to the material interests of the Territory." Agricultural settlements had matured along the ancient shoreline, and the "problem of secular change" in climate was "of such vital importance to the agriculture of an arid domain" that, Gilbert wrote, "the public domain presents no more important problem to the survey." The investigation of Bonneville involved theoretical work into climatology which translated into geosocial meaning.

One of the major strengths of Gilbert's Bonneville monograph is exactly the strength that needs to be cultivated in modern Quaternary studies — its interdisciplinary relevance. Thus, process-oriented sedimentologists can utilize its analyses of delta formation, littoral transport, and laboratory experiments in chemical sedimentation (Jopling, 1975). Structural geologists and geophysicists are provided with detailed data on epeirogeny with which to test mathematical models of long-term crustal strength. Climatologists have a record of rhythmic lake-level oscillations as a hydrologic response to alternating climate. Even today geomorphologists have remained generalists in a time of increasing scientific specialization. Although this circumstance may have impeded the development of geomorphology, it emerges as an advantage in the modern era. Someone must be able to integrate knowledge concerning diverse surficial processes to forecast the complex man-environment interactions on our planet. What better model for the accelerated human alterations of environment than the detailed investigation of actual alterations that have already occurred during the Quaternary?

EXTRATERRESTRIAL LANDFORMS

By 1888, Powell had promoted Gilbert to the post of "chief geologist." Gilbert's work continued to be administrative, directing the geologic branch of the survey while Powell handled the political chores. Powell's direction of the Survey, especially in matters of Western land reform, earned him enemies, many of them in the U.S. Congress. When he succeeded in attaching a clause to the Irrigation Act of 1888 that closed the public domain until the U.S. Geological Survey completed a survey of irrigable lands and reservoir sites, he gave his critics a common cause. He aggravated the situation by insisting that the irrigation work necessitated a national topographic map, and he bootlegged appropriations to achieve it. By 1890 he had overreached himself. Congress gutted his map and irrigation projects. In 1892, the U.S. Geological Survey suffered a halving of its budget.

During the political storm raging about the Survey, Gilbert revived his field research through the only device he could manage — a leave of absence. He studied the crater at Coon's Butte, Ariz. (now Meteor Crater). When the maelstrom over Survey mapping and conservation practices struck full blast in 1892, Gilbert was elaborating on the Coon Butte scenery and spending his evenings at the Naval Observatory telescope in Washington. What time he did not give to the Survey, he gave to scientific research. Controversy only marred science; he would no more permit the political currents to halt his observation of the moon than he had let the Colorado River prevent his sighting of Venus from its gorge in 1871. If anything, his philosophical abstraction from the practical affairs of the Survey worked against it. Davis (1926, p. 176) notes that one congressman used the affair as a means of taking the Survey to task. "So useless has the survey become," he huffed, "that one of its most distinguished members has no better way to employ his time than to sit up all night gaping at the moon."

Gilbert's lunar observations led to simple experiments. He projected pellets of varying density into dishes of mud, clay, and lead. The combination of observation and experiment led to conclusions that rejected either volcanism or meteoric impact alone as a primary process. Instead Gilbert (1893a) proposed an alternative process which subsumed their effects. To accomplish this he had to subordinate the mere surface features of the moon to the larger process of its origin. This resulted in his "moonlet theory."

His arguments swung on three hinges: first, the homology of forms between lunar craters and those produced by explosive impact; second, the observations he never tired of reiterating that "all solids are in fact both rigid and plastic;" and third, a clever mathematical analysis on the angle of incidence of "meteors" and the subsequent ellipticity of impact craters. On all counts the analysis bears remarkable resemblance to his analysis of the laccolith process at the Henry Mountains and to isostatic rebound at Lake Bonneville. His theory was that the Earth was surrounded by a ring of debris analogous to Saturn's. Gradually the debris began to coalesce into asteroidal "moonlets" and these merged by impact to form the moon. By assuming a single plane of orbit for the moonlets, Gilbert could account for the production of circular rather than elliptical craters: the moonlets struck the moon normal to its surface rather than obliquely. To discover this "general law" relating angle of impact and resulting crater ellipticity, he undertook his experiments, systematically varying angle of incidence, velocity, of impact, and softness of materials. By allowing for a gradation between plastic and rigid behavior in moon rock, Gilbert was able to account for the apparent anomaly between the size of the crater and the amount of the material thrown from it. This was reinforced by a careful analysis of fusion created during the impact. Finally by allowing the successive impacts to dislodge the axis of rotation, Gilbert could account for the equitable distribution of craters across the moon. The "moonlet theory" marks the beginning of modern speculation about the origin of the moon. Its model, indeed, was generalized by Chamberlin and Moulton into the famous "planetismal hypothesis" and serves as the godfather of modern models of planetary formation by impact cratering.

Gilbert's moon study showed clearly that a scientist experienced in the analysis of Earth surface processes could make a valuable contribution to the study of other objects in the solar system. Although his work contained several errors, it was only during the modern geomorphic era that Gilbert's study was clearly surpassed. With the advent of orbiting spacecraft, flyby missions, and actual landings, the amount of extraterrestrial landform information has literally skyrocketed. Consider Mars alone. Sagan (1975) estimates that before 1965, telescopes only permitted a ground resolution of 100 km. By 1972 the Mariner 9 high-resolution orbital television cameras were resolving 100 m objects on the planet surface. In 1976 television cameras on two Viking landers were producing images showing objects only several millimeters in diameter.

Besides the wealth of extraterrestrial landform data, the modern era is being burdened with a wealth of conflicting hypotheses to be tested. Lunar landforms (Mutch, 1972; Schultz, 1976) have received detailed study, yet arguments over fundamental morphologic features are as basic as the debates that prevailed during the "classic" period of geomorphology in the nineteenth century. The lunar rilles, for example, have been variously ascribed to structural control (grabens), collapsed lava tubes, surface lava channels, coalesced gas vents, and even running water. The conflicting hypotheses of volcanism versus impacting persist today in the interpretation of central crater peaks. Certainly the resolution of these problems will require adherence to the scientific method so well elaborated by G. K. Gilbert.

Analogic reasoning is as fundamental to the analysis of extraterrestrial landforms as it was to the analysis of the new lands explored by Powell, Dutton, and Gilbert. Thus, studies of relatively rare terrestrial impact craters (for example, Milton and others, 1972) aid the interpretation of the pervasive impact features observed on other planets. Mariner 9 resulted in the startling discovery that Mars was far more earthlike in its landforms and processes than previously believed. This has now been reinforced by orbital Viking photographs that show fluvial channels, mass movement phenomena, sand dunes, huge shield volcanoes, scabland erosion, and permafrost terrain.

The frontier of new landforms discovered in the American West gave vitality to geomorphology in Gilbert's late nineteenth century. After nearly another century, geomorphology now has a new frontier of extraterrestrial landforms. Several geomorphologists have already made important contributions to these studies (for example Mackin, 1969; Schumm, 1970; Sharp and Malin, 1975). Many more geomorphologists should be studying the rapidly accumulating information available on the surfaces of other planets.

TIME AND EQUILIBRIUM

With the publication of *Lake Bonneville* in 1890, Gilbert's reputation as the premier American geologist was confirmed. In 1892 he gave the presidential address to the Geological Society of America. Gilbert posed several fundamental questions pertaining to continents. Prodded by new oceanographic data discovered by the expedition of the British ship *Challenger*, he questioned the belief in continental permanence, a doctrine initiated by Dana and maintained by Joseph LeConte, Eduard Suess, William Morris Davis, and Thomas Chamberlin. Then Gilbert

(1893b, p. 187) attacked the corollary, supported by biostratigraphic evidence, that the continents grew:

> We have been told by the masters of our science, and their teaching has been echoed in every text-book and in every classroom, that through the whole period of the geologic record the continents have grown; not that the pendulum has moved always in one direction, but that the land area has, on the whole, steadily increased. From this doctrine there has been no dissent—and possibly there should be no dissent—but the evidence on which it is founded appears to me so far from conclusive that I venture to doubt.

Gilbert's instinct for balance in natural systems made him continually critical of most geophysical and geological concepts that were predicated upon continual growth or decay. He was a consummate Newtonian in his insistance that every action have an equal but opposite reaction. Whereas most American geologists followed LeConte's example in praising Dana's laws of progress, rising out of analogies to embryological development, Gilbert persisted in loyally advocating Lyell's steady state model — much to the dismay and confusion of his admirers and memoirists like William Morris Davis. Lake Bonneville, for example, did not exhibit a continuous growth of form but an oscillation of events, often in broken sequence. Its history meant stages of equilibrium upset by sudden, discontinuous events. In physical terms this led Gilbert to insist on the influence of small "cataclysms" like floods and storms in the shaping of the river profile and lake shoreline. Eventually the more frequent processes such as sustained streamflow or "normal" waves would bring the system back into balance.

A paramount question in geology for Gilbert's day was the age of the Earth. In his address as retiring president of the American Association for the Advancement of Science, Gilbert (1900) announced his own thoughts on the matter — a criticism not so much of dates as of assumptions behind geological and geophysical methods of estimating age. Although he did not denigrate the efforts of evolutionary geologists and thermodynamic geophysicists to solve this problem, he did urge a redirection of geological effort. He proposed a search for natural rhythms by which to correlate geologic events. Given some direct correlations a general time scale could be constructed by applying ratios from that fixed point. In particular, Gilbert (1900, p. 1007) recommended the "precessional motion," a planetary cycle of 21,000 yrs, which "pulses steadily on through the ages, like the swing of a frictionless pendulum."

In all his works Gilbert avoided any directional concept of geological time. He considered time to be a matrix for causes, but not itself causal; a plexus of processes, but not itself a process. Mathematically Gilbert organized time as he would mass or force, by ratios of its quantities, not by the calculus, which was better suited for measuring the rate of change of some process over time. The world was neither growing as the evolutionists contended, nor degenerating as the physicists held. It simply oscillated in space and time. The regularity of events in time was manifest as "rhythms"; the geologic record at any moment revealed a plexus of such rhythms.

As reviewed by Melhorn and Edgar (1975) geomorphology has long concerned itself with rhythmic (more often "cyclic") development of erosional and depositional landscapes. In the past such concepts were unfortunately tied to specific theories of landscape evolution, such as peneplanation or pediplanation. Today the new global tectonic models and the synthesis of stratigraphy that they can afford (for example Bird and Dewey, 1970) are providing a framework in which to evaluate the rhythmic development of paleolandscapes on the surface of a planet conceived to be in dynamic equilibrium. On the finer time scale, the formerly enigmatic oscillation of Quaternary climate now appears to be firmly tied to changes in the Earth's orbital geometry (Hays, Imbrie, and Shackleton, 1976). Gilbert would be pleased.

Strahler's (1952) introduction and Chorley's (1962) subsequent elaboration of systems theory in geomorphology have now given us a perspective in which to view Gilbert's concept of time. As Bucher (1941) had pointed out, geologists study two types of information, the *timeless* and the *timebound.* Because Gilbert arbitrarily chose open geological systems for study, the equilibrium states he observed were self-regulated by internal adjustment to outside change in mass and energy. The equilibrium was independent of time (steady state). Other geologists of the day conceived closed-system models in which equilibrium was only achieved when entropy attained a maximum. One holding this latter view would conceive of every system state as time-dependent. Little wonder then that William Morris Davis (1926, p. 107) should state, "The absence of the important physiographic factor, time, from Gilbert's reports is . . . perplexing. He must have known perfectly well that the existing conditions of drainage systems as well as the existing forms of the land surfaces are the product of erosional processes acting upon structural masses through longer or shorter periods of time; yet his account of streams and of land forms is much more concerned with their existing status than with their evolutionary development from an earlier or initial status into their present status."

It is certainly true that Gilbert's achievements were made without any specific reference to systems theory. He encompassed his approach within prevailing thermodynamic models. However, as pointed out by Smalley and Vita-Finzi (1969) thermodynamics contains the essential elements of systems analysis. Probably it is the First Law, the conservation principle, rather than the Second, the entropy principle, that is most relevant for both Gilbert and systems theory. Rather than seeking the ultimate states of closed systems, as Chorley (1962) has described the Davisian concern with the peneplain, Gilbert selectively studied process-response systems over time intervals in which entropy remained constant. In a similar way, modern systems geomorphology looks to the information content of landscapes rather than to the entropy content.

Gilbert's emphasis on the timeless components of landscape development produced precisely the types of dynamic models most useful in today's predictive geomorphology. The multiple equilibrium states of open geomorphic systems form the relatively stable surficial environments with which the expanding human population must contend. The recognition of equilibrium states has now focused attention on the disequilibrium that ensues when a threshold for a geomorphic system state is exceeded (Schumm, 1973). Geomorphologists must continue their investigations into the equilibrium states of Earth surface processes and the factors that alter equilibrium for the simple reason that mankind suffers the consequences of those alterations.

THE TROUBLE WITH FLUMES

In 1905 Gilbert was assigned the problem of hydraulic mining in the Sierra Nevada. Increased stream sediment loads, induced by mining wastes, had resulted in severe damage to California communities downstream from large-scale placer operations. Dams and levees had been constructed, but the problem persisted. Gilbert recognized that the engineered rivers of the Sierras required a very different explanation than he had provided for stream behavior in the Henry Mountains. The study demanded a quantitative, precisely instrumented analysis of sediment transport to predict the influence of increased sediment loads and to evaluate the effects of structural counter measures. He approached this problem by constructing a set of flumes to measure painstakingly the relative influences of different variables in stream transport. He errected his flumes, the first "hydraulic laboratory" of the U.S. Geol. Survey on the Berkeley campus in 1908. Because of interruptions by the 1906 earthquake and a near fatal illness in 1909, the flume study and the field investigations took over 10 yrs.

Gilbert's flume study combined the largely separate fields of hydrodynamics, hydraulics, and geomorphology. Gilbert's hydraulic laboratory was the fourth to be built in America, but it was the first to address geologic questions and thus marks the origin of experimental technique in sedimentology and fluvial geomorphology. So accurate and ambitious was the project that Gilbert's figures continue to be cited today (Rouse and Ince, 1957; Bogardi, 1974). However, his empirical plots of "capacity for hydraulic traction" against various variables were not the ultimate goal of the study. In the end he expected to discover a unified set of equations, both simple and predictive. On the contrary, however, Gilbert (1914, p. 109) concluded, "the development of complexity within complexity suggests that the actual nature of the relation is too involved for disentanglement by empiric methods."

Gilbert found his study to be disappointing because it failed to fulfill the beliefs that inaugurated it. Gilbert (1914, p. 236) noted, "if the formulae were rational, the result of an adequate mathematical treatment of the physical principles involved, the constants measured in the laboratory would be of universal application, but the constants of an empiric formula afford no basis for extensive extrapolation." He suggested that the form of his equations might apply, but not their exponents, and that, "since the principles discovered in the laboratory are necessarily involved in the work of rivers," they might serve in the case of "natural streams which are geometrically similar to the laboratory streams." Despite the "natural desire" of an experimenter "to do his work over in a better way," he recommended a different approach to the problem in future studies. In the realm of experimentation, he suggested more "synthetic" models of streams than those afforded by flumes. In terms of larger questions, "it is possible," he argued, "that the chasm between the laboratory and the river may be bridged only by an adequate theory, the work of the hydromechanist."

Gilbert's insight into the sediment transport problem is all the more remarkable when we consider the 60 yrs of further experimentation that followed. In reviewing this work Maddock (1969) concluded that the nine equations required for the description of flow in alluvial channels will probably never be discovered. Maddock (1973) also noted that the detailed mechanics of particle motion in natural sand bed streams is so complex that even this restricted problem will defy a rational deterministic solution. The major advances in the prediction of sediment transport were achieved largely by the development of turbulence theory (Prandtl, 1926; Keulegan, 1938) and then by combining theory with laboratory-verified principles of momentum transfer (Vanoni, 1946) and stochastic effects (Einstein, 1950). An adequate rational theory of sediment transport has not yet been developed. Indeed it can be argued that the sediment transport problem is of the precise sort that will involve an irreducible level of uncertainty (Leopold and Langbein, 1963).

Geomorphology's new problem-solving role will require wider use of both physical models (such as flumes) and stochastic approaches. What is also required is that geomorphologists retain Gilbert's healthy skepticism for the universality of predictions based on these methods. By analogic reasoning, models will suggest important systems interactions that would escape the attention of a field investigator. However, certain complexities of the field prototype, which may have escaped the attention of the model-builder, pose a sobering limit on the precision of any model for prediction.

THE ENGINEERED ENVIRONMENT

Ironically, Gilbert's (1917) analysis of hydraulic mining began after most of the environmental damage had already been done. In 1884 the agricultural interests of the lower Sacramento River had succeeded in obtaining an injunction against the hydraulic mining of Sierran gold placers. The study of the relationship between mining and downstream fluvial regime changes was reopened when the mining interests, seeking a lifting of the injunction, influenced the federal government to give the problem a thorough investigation.

Gilbert's study is a treatise on the natural and engineered environment of the Sacramento River system, from its mangled headwaters in the Sierra Nevada to its twice daily discharge over the tidal bar outside

PLATE 2

A Sierra canyon showing aggradation resulting from the introduction of mining debris (pl. 6, U.S. Geol. Survey Prof. Paper 105).

the Golden Gate. The abrupt overloading of the Sacramento River system with the tailings from hydraulic mining was only a trigger. This mass passed through the river system in the form of a "debris wave." Yet each part of the river responded to the flood differently, and many of the natural processes of disposing the debris had furthermore been altered by engineering works. As the wave proceeded downstream, debris accumulated until the crest of the wave passed (pl. 2), at which point it began to erode away except for a certain proportion permanently lodged in the landscape. At each locale the actual effect was a complex product of individual functions, and for each locale Gilbert had to determine the natural regime of the stream before analyzing the disequilibrium created by the sudden invasion of debris and engineering projects.

The progress of the wave under entirely natural conditions was complex enough; compounded with artificial factors associated with human settlement, it became considerably more complicated. To divert the debris from urban and agricultural environments, a system of levees, dams, and by-pass channels had been erected; at the same time agricultural reclamation continually encroached on marsh and delta lands and demanded protection from floods of both water and debris. This altered the hydraulics of the Sacramento considerably. Some of these measures were clearly ephemeral. Gilbert described a dam barrier on Yuba River designed to store debris. As a reservoir its up- and down-stream *influence* extended for little over a several kilometers, and as the river deposited upstream and eroded downstream in an effort to adjust its profile, the life of the dam was brief. Gilbert recorded its failure in a 1906 flood and with it the narrowsightedness of the entire scheme temporarily to embargo or export debris via government engineering extravaganzas. He also carefully mapped the residual deposits left after the flood, and knowing the flood discharge from a nearby gaging station, he tried to use this information to measure the tractive velocity that had eluded him in his flumes.

The ultimate consequence of the debris from mining, augmented by soil erosion input from unwise agricultural practices, was found 80 km from the point where the sediment was trapped. How ironic that the commercial interests in San Francisco whose mining had initiated the debris wave were also affected by it! Gilbert showed that the tidal bar outside the Golden Gate was in equilibrium with factors in San Francisco Bay. By demonstrating how agricultural reclamation of Sacramento marshlands and bay-filling from agricultural and industrial detritus reduced the "tidal prism" — the volume of water affected by tidal action — he showed that the cumulative effects were to steepen the tidal bar and move it closer to shore. Ultimately it then threatened the harbor entrance. Even as he wrote, dredges, at great expense, were working shoals that prior to mining and settlement had allowed shipping to cross over them.

Gilbert's hydraulic mining study is a clear example of what the National Science Board (1971, p. 2) carefully defined as environmental science: "the study of natural processes, their interaction with each other and with man, and which together form the earth systems of air, land, water, energy, and life." Modern environmental science strives for a system-level understanding of earth-process interactions. It is distinct from engineering which strives to apply science most efficiently to the design of human works. Gilbert was able to view his problem in dimensions of time, space, and interaction with other systems that went far beyond an engineering study. It is precisely to provide this overview that geomorphologists must increasingly involve themselves in environmental analysis. Strahler's (1952) warning that engineers would increasingly dominate the dynamic study of Earth surface processes continues to haunt geomorphology, but the past decade has witnessed a revitalization of environmental studies (Chorley, 1969; Coates, 1971; Cooke and Doornkamp, 1974).

Geomorphologists must no longer be merely content with the application of their science to qualitative interpretations of environmental response to specific actions. The new tools for landform analysis must be combined to provide quantitative predictions that will meet the accelerating needs of our society. Areas in immediate need of predictive capability include (1) reducing the effects of natural disasters (earthquakes, landslides, floods), (2) alleviating chronic damage (accelerated erosion, subsidence, desertification), and (3) abating pollution, both by man and nature (for example, sediment). Merely one of these many problems (soil erosion) poses profound implications for world food prospects, especially in underdeveloped nations (Eckholm, 1976).

Some geomorphologists may view the increasingly applied nature of their science with alarm. Certainly the narrowly conceived geomorphic study may, like some engineering design analyses, only have a direct application to a single problem. But this is not the "pragmatic science" exemplified in Gilbert's work, nor should it be the goal of modern environmental science. Gilbert welcomed the hydraulic mining study as a chance to synthesize both pure and applied science. In the natural and engineered transportation of fluvial debris, he was able to study bedforms, channels, shorelines and terraces, the fluvial transportation of sediment, and its modification of landscape induced by human settlement. Gilbert's research, as George Otis Smith memorialized, won the respect of both the geologist and the engineer. "Pure science as given to the world by Grove Karl Gilbert was useful science" (Smith, 1918, p. 11).

The applied geomorphologist should not overlook the unique opportunities afforded him for the study of accelerated analogues to processes that may elude a rational explanation in their natural state. So it was that Gilbert (1909a) was able to analyze an exception to the "law of slope" that he had enunciated in his Henry Mountains monograph. In his 1876 field notes from Utah he had speculated that hillslope convexity might be produced by differential weathering. In studying the erosion of mounded mining debris in California, however, he was able to observe hillslope processes in action and to conclude that the actual cause for convexity was differential transportation. Gilbert (1909a, p. 346) noted, "On the upper slopes, where water currents are weak, soil creep dominates." He imagined that the transportation process was in equilibrium. The slope was everywhere adjusted to provide just the velocity sufficient to move a uniform layer of surface debris. "In other words," he concluded, "the normal product to degradation by creep is a profile convex upward." This brief study is a clear predecessor of much of process-oriented hillslope geomorphology (Carson and Kirkby, 1972, p. 306-307).

COMMUNICATION

In 1906 Gilbert was diverted from his flume studies at Berkeley by the great San Francisco earthquake. He was named to the official California commission and to the U.S. Geol. Survey teams assigned to study the disaster. Much of this work was descriptive, tracing the active fault zones and recording the damages to structures. Nevertheless, his insatiable interest in force and resistance led to speculations concerning the possibilities of earthquake forecasts. As with many other geologic phenomena, Gilbert (1909a) felt that earthquake frequency and intensity should occur in a rhythmic sequence. He did not know the ultimate source of the force, but he thought that the resistances might be understood by geological studies of the fault zones. Such studies would also be useful in assessing the potential locations of future earthquakes.

Gilbert noted that geologic studies could delineate earthquake hazard zones. It was quite another problem for geologic knowledge to influence building codes in such zones. Gilbert (1909b, p. 135) was skeptical of the public response: "This policy of assumed indifference, which is probably not shared by any other earthquake district in the world, has continued to the present time and is accompanied by a policy of concealment. It is feared that if the ground of California has a reputation for instability, the flow of immigration will be checked, capital will go elsewhere, and business activity will be impaired. Under the influence of this fear, a scientific report on the earthquake of 1868 was suppressed."

Here then is the final direction for environmental geomorphologists: they must communicate their knowledge of the land's tolerance for human activity to the responsible makers of public policy. In most cases this will mean bringing relevant scientific information to the attention of nonscientists such as engineers, planners, lawyers, and politicians.

EPILOGUE

In this paper we have briefly considered some of G. K. Gilbert's major scientific contributions. These works clearly have continued relevance for the future conduct of geomorphic research. Gilbert may have had a vision of the high regard with which geomorphologists would regard his work in the latter half of the twentieth century. Gilbert (1884, p. 452) narrated the following incident that occurred on a plateau in Utah. "Standing on the verge of the cliff just before sunset," he wrote, "I saw my own shadow and that of the cliff distinctly outlined on the cloud. . . . About the head was a bright halo with a diameter several times greater than the head." He concluded with mock solemnity: "The observation has more than a scientific interest, because, in the popular imagination, the heads of scientific observers are not usually adorned with halos."

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