2 Observation in Geomorphology

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ABSTRACT

Observation traditionally has occupied a central position in geomorphologic research. The prevailing, tacit attitude of geomorphologists toward observation appears to be consistent with radical empiricism. This attitude stems from a strong historical emphasis on the value of fieldwork in geomorphology, which has cultivated an aesthetic for letting the data speak for themselves, and from cursory and exclusive exposure of many geomorphologists to empiricist philosophical doctrines, especially logical positivism. It is, by and large, also an unexamined point of view.

This chapter provides a review of contemporary philosophical perspectives on scientific observation. This discussion is then used as a filter or lens through which to view the epistemic character and role of observation in geomorphology. Analysis reveals that whereas G.K. Gilbert's theory-laden approach to observation preserved scientific objectivity, the extreme theory-ladenness of W.M. Davis's observational procedures often resulted in considerable subjectivity. Contemporary approaches to observation in geomorphology are shown to conform broadly with the model provided by Gilbert. The hallmark of objectivity in geomorphology is the assurance of data reliability through the introduction of fixed rule-based procedures for obtaining information.

INTRODUCTION

Geomorphologists traditionally have assigned great virtue to observation. The venerated status of observation can be traced to the origin of geomorphology as a field science in the late 1800s. Exploration of sparsely vegetated landscapes in the American West by Newberry, Powell, Hayden, Gilbert, and others inspired perceptive insights about landscape dynamics that provided a foundation for the discipline (Baker and Twidale 1991). Con-

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temporary geomorphology continues to have, as it should, a strong field component. The importance of this component and its relationship to observation is nicely summarized by Ritter (1986, p. 3): '. . .the real test of geomorphic validity is outdoors, where all the evidence must be pieced together into a lucid picture showing why landforms are the way we find them and why they are located where they are. A prime requisite for a geomorphologist is to be a careful observer of relevant field relationships.'

Although observation clearly is assigned privileged epistemological status in geomorphology, little, if any, formal justification of this status has been provided by geomorphologists. Instead, the privileged role of observation appears to be an implicit unquestioned presupposition. Recently, debate has arisen concerning the relative importance of theory and observation in geomorphology. Whereas Baker and Twidale (1991) emphasize the primacy of observation over theory, Rhoads and Thorn (1993) have argued that this view is misleading because virtually all scientific observation is inherently intertwined with theoretical presuppositions. The latter position generally has not been widely embraced within science at large, including geomorphology, because of the fear that objectivity will be compromised if the observations used to test a theory depend strongly on that theory (e.g. Feyerabend 1993). If such dependence was common, the power of empirical testing would be undermined because no neutral body of facts would exist against which competing theories could be compared. Under these conditions, theory choice becomes subjective and scientific knowledge relative, a view that is not widely held by scientists in general and geomorphologists in particular. On the other hand, most contemporary philosophers of science fully acknowledge that scientific observation is inherently theory-laden, yet have defended objectivity by defining a naturalistic perspective on scientific observation that accords with its function in actual scientific practice.

The purpose of this chapter is to examine the epistemic role of observation in geomorphology in the light of recent developments in the philosophy of science. The first section of this chapter provides a context for subsequent discussion by reviewing traditional philosophical perspectives on scientific observation. The next section highlights the basic tenets of the emerging, naturalistic perspective on observation and illustrates how this perspective differs from traditional philosophical views on observation. The final section of the chapter explores the relevance of philosophical perspectives for understanding the science of geomorphology by examining G.K. Gilbert's and W.M. Davis's ideas on observation and by analyzing the character and function of observation in contemporary geomorphologic research.

PHILOSOPHIC PERSPECTIVES ON SCIENTIFIC OBSERVATION

Since the origin of modern science during the Renaissance, observation has been viewed by both practicing scientists and philosophers of science alike as a, and in some cases the, fundamental building block of science. Observation occupied center stage in Sir Francis Bacon's early description of the methods of science (Haines-Young and Petch 1986). It also played a foundational role in perhaps the most influential model of modern science: the view developed between 1900 and 1960 by the logical empiricists (also known as the logical positivists) (Suppe 1977).

The Empiricist View of Observation

Logical empiricism is founded on a clear dichotomy between observation and theory. In particular, this language-based analysis of science distinguishes between observation statements and theoretical statements. According to proponents of this view, the vocabulary for observation statements is acquired ostensively and statements made in this vocabulary can be verified (or disconfirmed) empirically through direct observation. In contrast, statements made in the theoretical language are about unobservable phenomena and thus only have truth value if they are related to the observational vocabulary via explicit definitions or correspondence rules.

Logical empiricists never specified a definitive set of criteria governing the distinction between observable and unobservable phenomena, and generally assumed that the notion of something being directly observable is unproblematic (Suppe 1977, p. 46). It is also clear that they associated direct observation with sensory perception:

> To a philosopher, 'observable' has a very narrow meaning. It applies to such properties as 'blue', 'hard', 'hot'. These are properties directly perceived by the senses (Carnap 1966, pp. 225-226).

According to the logical empiricists, assertions about directly observable phenomena can be verified intersubjectively without recourse to theoretical presuppositions; thus, the observation language is theoretically neutral and the truth of statements made in this language is unproblematic (Suppe 1977, p. 48). This argument implies that observation is independent of *all* theory.

Because the truth of observation statements can be determined on the basis of theory-neutral observations, the observations used to verify these statements provide a foundation for scientific knowledge. All other claims to knowledge must rest on the foundation of this base knowledge. Science grows through the steady accumulation of facts and empirical generalizations based on direct observation, and then later introduces theoretical postulates (tied to verified observation-language assertions) to organize these facts and generalizations. This upward growth from facts to theories is similar to the inductive Baconian model of scientific progress (Suppe 1977, p. 15).

Postpositivist empiricism has moved away from a language-based analysis of science toward a view that emphasizes the important role of models in scientific practice (van Fraassen 1980, 1989). However, this form of empiricism still retains observation as an epistemological foundation for knowledge: science is justified only in assigning truth to the claims it makes about observable phenomena, not the claims it makes about unobservable phenomena. Postpositivist empiricists also have not fully articulated what it means for something to be classed as 'observable'. Van Fraassen (1980, pp. 16-17) states that 'observable' is a vague predicate, but maintains that the distinction between observable and unobservable phenomena is nevertheless useful provided it has distinct cases and countercases. He counts seeing something with the unaided eye as a clear case of an observable phenomenon and refers to subatomic particles detected in a cloud chamber as unobservable phenomena. Apparently, the distinction van Fraassen has in mind depends strongly on human perception (Suppe 1989, p. 26).

Problems with the Empiricist View of Observation

Logical empiricism has few supporters within contemporary philosophy of science. The downfall of this philosophical doctrine centered on its distinction between the observational and theoretical. Attacks on this distinction consist of three types: attempts to show that the distinction between observational and theoretical terms is untenable (e.g. Putnam 1962; Achinstein 1968), attempts to show that no sharp ontological distinction exists between observable and unobservable entities (e.g. Maxwell 1962), and attempts to show that observation is inherently theory-laden and thus the notion of theory-neutral observation or observation statements must be denied (e.g. Hanson 1958; Feyerabend 1958; Kuhn 1962; Popper 1968). Although not all of these arguments are convincing (Suppe 1977, pp. 80-86), it is now generally accepted within philosophy of science that the methods and language of scientific observation are theory-dependent (Boyd et al. 1991, pp. xi-xiv).

Recognition that observation is theory-dependent led initially to the emergence of other epistemologies that emphasize the relativistic nature of scientific knowledge. The best known alternatives are those by Kuhn (1970) and Feyerabend (1993). Both of these views embody the notion that the content of observations or observation statements used to test theories is at least partly determined by the theoretical commitments which a group of scientists accepts. Differences in such commitments between groups or over time within a given group will result in differences in the meaning of observational statements and in what constitutes relevant observational evidence for each theory. Thus, disputes about competing theories cannot necessarily be resolved by appeal to a shared body of observational evidence, thereby undermining the adjudicatory power normally accorded to observation. Instead, knowledge becomes relative to the social, cultural, and historical settings of a particular group of scientists. The result is incommensurability of theories, and relativism about what can be known (epistemological relativism), about what is accepted or regarded as being known (epistemic-avowal relativism), but not about what exists (metaphysical relativism or subjective idealism) (Suppe 1989, pp. 302-328).

The relativist views of Kuhn and Feyerabend provide the foundation for what is now known as social constructivism. This epistemological stance derives from ideas about language espoused by Wittgenstein (1969, 1980), who, ironically, earlier in his life contributed to the development of logical empiricism (Wittgenstein 1922). The scope of constructivism is broad, ranging from neo-Kantian idealism, which contends that scientists actively construct theories (and thus the reality they investigate) through social practices (Knoor-Cetina 1983; Collins 1983) to epistemic relativism, which concedes that scientists interact with an objective reality, but maintains that the acceptability or unacceptability of claims to knowledge about phenomena is relative to a particular scientific community (Longino 1990; Pickering 1990; Nelson 1994). Constructivist epistemologies are aligned closely with postmodernist philosophy, which discounts the capacity of human rationality to provide a 'view from nowhere' on the world (Rorty 1979; Parvsnikova 1992).

Observation Naturalized

Over the last 20 years, efforts to formulate a naturalized epistemology of scientific knowledge (see Shapere 1987; Nickles 1987) have led first to an extended view of observation that attempts to accommodate the application of this term to unobservable objects and processes by practicing scientists. In turn, in response to perceived shortcomings in the extended view of observation, more recent philosophical work has focused on the way in which scientists empirically evaluate scientific claims using *data* or *evidence.* These two views are examined in the next two subsections, followed by a discussion of their epistemological implications.

The extended view on observation

A major shortcoming both with logical empiricism and with many relativist views of science is that observation is equated with human sensory perception. In science, on the other hand, this term is used in a less restrictive sense. Many scientists claim to observe or even directly observe phenomena in situations where sensory perception plays only a minor role (see examples in Shapere 1982; Brown 1987a). Whereas the logical empiricists avoided this seeming contradiction by claiming that scientists and philosophers simply use the terms in different ways (e.g. Carnap 1966), such a stance becomes difficult to defend if philosophical usage limits or unnecessarily complicates attempts to understand science as an epistemic activity. Although debate about the theory-ladenness of human perception continues, spilling over from pure philosophy into the domains of neurophysiology, cognitive psychology, and visual psychophysics (see Werth 1980; Weckert 1985; Fodor 1984, 1988; Churchland 1988; Gilman 1992; Brewer and Lambert 1993), it is becoming increasingly evident that philosophical perspectives that equate observation with perception fail to capture the full richness of the epistemic role of observation in science (Suppe 1989, pp. 38-77).

Scientific observation involves obtaining information about an item in the world through a causal chain linking the (senses of the) observer to that item. This chain can be either short or long. Perceiving something through the use of our native senses alone is an example of a short chain, whereas exploration of the world through the use of sophisticated instrumentation is an example of a long chain. In any case, background information (i.e. accepted theories and beliefs that have proved successful and are free from specific and compelling doubt) is an integral component of observation because it is only within the context of such information that a causal chain between the observer and the observed can be established and that the outputs of such a chain can be interpreted in a scientifically meaningful (epistemically relevant) fashion (Shapere 1982; Brown 1987b). Thus, background information is an essential epistemological ingredient in our ability to observe items in the world.

The reliance on background information acknowledges that observation is at least to some extent theory-laden; however, this aspect of the observational process does not necessarily threaten scientific objectivity, an issue that is explored in detail in the next section. Furthermore, unlike traditional empiricist views, this naturalist perspective maintains that short causal chains are not inherently more reliable than long causal chains. As Brown (1987b) points out in a detailed analysis, human senses are instruments that operate according to natural laws in the same manner as artificial instruments; thus, the senses, like other instruments, are not only fallible (in contrast to logical empiricist claims to the contrary), but also are limited in their capacity to interact with items in the world. In fact, Brown (1987b) argues that the senses are fairly crude instruments compared to many artificial ones. For this reason, short causal chains involving the human senses alone often produce less reliable and/or more limited observations than the long complex causal chains associated with sophisticated artificial instrumentation.

Data and empirical evidence

The quintessence of extended observation is to eliminate the distinction between the roles of instrumentation and human perception in scientific investigation. In many ways this perspective represents an attempt to salvage a term, observation, and a distinction, observation versus theory, that have been deeply embedded and pivotal in the philosophy of science. Perceived shortcomings with this effort have spawned alternative naturalized epistemologies that differ from the extended view of observation in several important respects. A common concern of those criticizing attempts to retain observation is that this concept, even in the extended form, is epistemologically inadequate, i.e. it fails to capture the full role of empirical results in evaluating scientific claims to knowledge.

An examination of actual scientific practice suggests that the fundamental distinction is between 'data' and 'phenomena', not between observation and theory (Bogen and Woodward 1988, 1992; Woodward 1989). Whereas scientists use terms such as 'observations' and 'observed', they do so in a sense that refers to data or data collection procedures designed to detect phenomena. Data and phenomena differ in important ways.

Data are the products of procedures designed to gather information about the world. These products are accessible to the human perceptual system and thus available to public inspection. Data can be derived solely from human sensory perception; however, this situation usually occurs only in the early stages of development of a scientific discipline. More commonly, the procedures for producing data involve instruments or detection devices that have been developed artificially using background knowledge. Even in cases where data are based on sensory perception alone, such data do not consist entirely of the raw perceptual experiences of individual observers. In all cases, the character of data is tied intimately to the peculiarities of the research design, investigative methods, and instruments or apparatus used to collect the data (including human senses) (Bogen and Woodward 1988). The production of data also tends to be 'noisy' in the sense that virtually all data collection efforts will be influenced by a variety of idiosyncratic causal factors. Controlling for these confounding factors is one of the primary concerns of science.

Phenomena in contrast are objects, entities, events, and processes that exist in the world. Whereas data tend to be idiosyncratic to particular investigative contexts, phenomena are relatively uniform stable features with regular properties or characteristics in a variety of contexts. According to Bogen and Woodward (1988) phenomena cannot be observed, but nevertheless are the foci of explanatory theories. This perspective contrasts sharply with antirealist forms of empiricism, which maintain that theories are merely devices developed to explain or 'save' observable aspects of the world. Instead, it is consistent with the realist notion that science seeks the truth about observable and unobservable aspects of the world. This view does maintain a role for empiricism in the sense that data provide an evidential basis for evaluating scientific claims about unobservable phenomena. Scientists produce data in an effort to infer the existence of phenomena or to test theoretical assertions about phenomena. These inferences and tests occur within the context of background information and theoretical assumptions that govern the reliability of data and that provide connections between data and phenomena. It is the truth or approximate truth of this background knowledge that allows scientists to delve beyond the senses and explore 'hidden' aspects of reality.

Another shortcoming of the extended view of observation is that the definition of observation it puts forward requires an investigator to causally interact with something that actually exists in the world in order for an observation to occur (Brown 1995). In many cases, however, the lack of an observation (i.e. interaction with the world) will, in fact, still produce data, which can provide powerful evidence for theory evaluation. For example, suppose a specific theory under consideration predicts the occurrence of a particular phenomenon. In turn, a well-established background theory indicates that this type of phenomenon, if it exists, should causally interact with a particular instrument, thereby producing a certain type of instrument response. In this case, the failure to detect the expected response would be powerful evidence against the existence of the inferred phenomenon. This type of situation, along with the distinction between data and phenomena discussed above, shifts the focus away from observation as a key scientific concept, and instead emphasizes the important epistemic role that evidence, a concept that subsumes observation, plays in scientific practice.

The epistemological nature of data or evidence

Naturalized perspectives on data and evidence have led to widespread recognition that theory penetrates all aspects of scientific activity, including determinations of (1) what can be measured or observed, (2) which data are important, (3) how 'raw' data are processed to produce 'refined' data, (4) how data are interpreted, and (5) how interpreted data are used in empirical testing (Nickles 1987). Given that most contemporary philosophers subscribe to the notion that a symbiosis exists between theory and data, a key issue concerns the potential effect of the theory-ladenness of data on scientific objectivity. How can scientific objectivity be preserved in the light of this theory-ladenness? An examination of this question requires a definition of objectivity. According to Brown (1987a) objectivity includes two necessary (but not sufficient) conditions: relevance and independence. In other words, the evidence used to evaluate specific claims about the world must be relevant to and independent of these claims.

Relevance demands that a scientific claim about a phenomenon be evaluated using appropriate evidence, i.e. evidence produced by a procedure that is capable of interacting causally with the phenomenon of interest. In turn, determination of which procedures, instruments, analytical techniques, etc. are capable of interacting causally with a phenomenon depends on the availability of appropriate background theories. Thus, for example, geomorphic claims that the Channeled Scablands are the result of a catastrophic paleoflood requires that such claims be evaluated using information collected in the Channeled Scablands, not from landscapes in southeastern Pennsylvania. Moreover, the procedures used to generate appropriate evidence from the Channeled Scablands should rely on established hydraulic and sedimentologic principles as well as knowledge of the glacial history of the region, rather than on genetic theory or quantum mechanics. Relevance does not require that background theories be correct, only that they be appropriate. Advances in hydromechanics someday may show that what was once believed to be relevant geomorphic evidence concerning the origin of the Channeled Scablands has become irrelevant to this problem. Thus, relevance can only be judged within a particular historical setting. This requirement also implies that acceptable background theories exist at the time the decision of relevance is made. It is not appropriate to claim at present that the Channeled Scablands were produced by aliens repeatedly draining the waste disposal tanks on board their spaceships at this location on our planet because no well-established body of background knowledge exists to causally connect evidence from the Scablands to that claim. On the other hand, this explanation cannot be completely ruled out a priori, no matter how ridiculous it may seem, because the development of new background knowledge supporting this claim is not impossible. The key point is that this explanation currently has no epistemic import given that no background theory about alien visits to earth now exists. Should such aliens suddenly show up and tell us that the Scablands is where they like to drain their waste disposal tanks, we would have to reevaluate current geomorphic claims about this area.

The other major factor influencing objectivity is independence of observations from the scientific claim under consideration. The issue of independence relates to the concern raised in the introduction to this chapter: if all scientific observation inherently depends on theory, as is now widely accepted within contemporary philosophy of science, does this theory-ladenness threaten scientific objectivity? Brown (1995) has conducted a perspicacious analysis of this question and has concluded that the answer to it is contingent upon the manner in which a particular observation depends on theory. According to him, observation can depend on theory in at least six different ways (Table 2. 1).

The first two forms of theory dependence (TD1, TD2) provide the basis for constructivist theories of scientific knowledge. Acceptance of TD1 and TD2 leads to radical relativism and incommensurability of competing theories. Although recent experimental studies provide some support for the influence of TD2 at the level of an individual or small group (Brewer and Chinn 1994), the major problem with accepting TD1 and TD2 at the level of an entire scientific community is that anomalies, which the history of science demonstrates play an important role in scientific discovery, become difficult to explain.

The third type of theory dependence (TD3) involves two separate elements: (a) that observational tests of a theory presuppose that theory, and (b) that use of the theory undermines the objectivity of testing the theory because it is impossible for the test to yield an outcome that will contradict a theory that has been presupposed. While Brown (1995) acknowledges that the first element is often the case in scientific testing, he argues

Table 2.1. Six different ways observation can depend on theory (from Brown 1995)

TD1. The items we perceive are already infected with material from the theories we accept
TD2. Scientists ignore evidence that contradicts their favored theories
TD3. Observations that are undertaken to evaluate a comprehensive theory presuppose that very theory in a way that prevents an objective test of that theory
TD4. a. All scientifically significant observations assume theories besides the theory being evaluated; b. it is always possible to protect a favored theory by challenging these auxiliary theories
TD5. Which observations scientists undertake is determined by accepted theory
TD6. Observation reports must be expressed in the language of the theory being tested if they are to be relevant to the evaluation of that theory

persuasively that this element does not logically imply the second and thus the two elements are not necessarily connected as is often assumed. He describes an example from astronomy where evidence that presupposes the theory of relativity still plays an important role in an objective test of relativity (Brown 1993). In many scientific investigations, a hypothesis derived from a theory is assumed valid and then is used, in combination with observational data, to construct (or compute) evidence that provides a test of another hypothesis derived from the theory, a method of empirical testing known as the bootstrapping method (Glymour 1980). As long as reliance on the theory under consideration does not guarantee confirmatory evidence, the testing can be considered objective. One factor that appears to be important for producing disconfirmatory evidence is fixedness of the theoretical considerations that undermine observational procedures within a particular area of science. As long as these considerations are fixed, they will provide constraints on expectations, and it is against this backdrop of theoretical predilections that involuntary, unanticipated evidence occurs (Hudson 1994).

The fourth type of theory dependence also has two parts. Auxiliary theories play a part in virtually all observational procedures and also are required to derive theoretical predictions that can be compared with data. Because in many cases these background theories can be viewed as largely or wholly independent of the theory under consideration (see Hacking 1983; Kosso 1989), this type of theory-ladenness does not inherently bias empirical testing. Greenwood (1990) draws the distinction between exploratory and explanatory theories; the former enable scientific observation and interpretation, but in many cases are largely independent from the latter, which directly generate testable scientific claims within a specific scientific discipline. In general, exploratory theories are uncontroversial and can be considered established knowledge relative to explanatory theories. It is this characteristic of exploratory theories that provides the basis for addressing the second part of TD4. The notion that a favored theory can always be defended against disconfirmation by modifying an auxiliary hypothesis is known as the Duhem-Quine thesis. This argument represents the greatest challenge to Popper's falsification methodology (cf. Putnam 1974). The greater epistemic force of exploratory theories compared to explanatory theories, however, usually precludes pursuit of the Duhem-Quine strategy when the scientist is faced with a situation where observations clash with predictions of explanatory theories. Exploratory theories often have widespread use in science that transcends the range of explanatory theories of particular disciplines; thus, modification of an exploratory theory to 'save' a favored explanatory theory will often have consequences across a broad range of science. Changing an exploratory theory usually will undermine prior observational evidence not only for the explanatory theory under consideration, but also for explanatory theories (and perhaps other accepted exploratory theories as well) in other disciplines. An extreme example would be if geomorphologists tried to protect a favored geomorphic theory by modifying the principles of Newtonian mechanics. Such a maneuver would have ramifications not only throughout other areas of geomorphology but throughout science at large. Clearly, this type of maneuver generally is unacceptable. The interconnectedness among accepted background knowledge (i.e. exploratory theories) within science is referred to as the web of belief (Greenwood 1990). When scientists are faced with recalcitrant observations, this web of belief tends to preclude modification of auxiliary hypotheses (exploratory theories) to preserve an explanatory theory.

The distinction between exploratory and explanatory theories is consistent with Woodward's (1989) division of scientific labor between those who gather data in support of phenomena (experimentalists or field scientists) and those who attempt to explain or predict phenomena (theorists). Whereas theorists focus mainly on theory construction, either to explain an existing phenomenon or to predict the existence of some new phenomenon, data gatherers are concerned primarily with methods of generating reliable data that can be used to test theoretical predictions about phenomena or infer the existence of new phenomena. Separation of these activities is necessary in science because it helps to ensure independence of data and phenomena. When a theorist devises a theory that predicts the existence of a new phenomenon, the details of the procedures, devices, and research design required to produce reliable data to detect the existence of this phenomenon typically are not prescribed by the theory, but are left to the data gatherers to determine (Woodward 1989). The considerations involved in such determinations are related more closely to potential exploratory theories and to the issue of data reliability, than to the content of the explanatory theory. Thus, theory and data are not connected by correspondence rules or explicit definitions in the manner envisioned by the logical empiricists; instead, an explanatory theory often provides little guidance about how one should go about obtaining evidence of a phenomenon. Conversely, data collectors not only engage in theory confirmation, but also search for new phenomena in a manner largely independent of explanatory theory. Although this activity generally does not occur in a complete theoretical vacuum, it is data-driven (inductive) in the sense that it is fueled by the development of new instruments, techniques of data analysis, investigative procedures, or methods for controlling error. This type of research, which also draws on exploratory theories and theories governing data reliability, generates discoveries of phenomena that subsequently require theoretical elaboration. Discoveries often occur when puzzling data are produced. The problematic character of data is determined by a conflict with accepted background theories and beliefs (Shapere 1984, p. 283). An excellent geological example of the way in which 'bottom-up' construction of evidence using exploratory theories interacts with the 'top-down' deduction of theoretical implications from explanatory theory is discussed by Kaiser (1991).

The selectivity aspect of theory dependence (TD5) is closely related to the issue of relevance. Theory plays a key role in determining which observations, out of the infinite variety of possible observations, scientists actually make. It not only serves to focus attention on aspects of reality that are relevant to scientific inquiry, but also is used to sort out the relative importance of observations that are considered relevant. Whereas this aspect of theory dependence may *delay* challenges to theory because scientists deem certain types of information as irrelevant to a problem based on theoretical presuppositions, it does not determine the content of scientific evidence. Thus, TD5 does not inherently threaten scientific objectivity.

The final form of theory dependence (TD6) relates to the requirement of logical positivism that observation reports be described in theory-neutral language. Arguments against the existence of such a language are numerous and persuasive, and few contemporary philosophers of science defend the view that all theoretical predictions and empirical evidence can be expressed in some universal language. The issue at hand is whether the lack of a theory-neutral language prevents comparison of rival theories. Brown (1995) argues rather convincingly that in the absence of a universal language, theories can still be compared as long as proponents of competing theories agree on a body of relevant evidence and then use that evidence to compare the relative success (agreement between predictions and evidence) of the two theories. Even if each proponent does not understand anything about how their rival generated predictions, they should still be capable of discerning whether the predictions of their rival's theory conform with the evidence to a greater or lesser extent than the predictions of their own theory. In this case, neither scientist understands the language of the other. All that is required is some shared ability to communicate with one another (e.g. pointing, recognition of symbols). Obviously, comparison will be easier if one investigator understands the theoretical perspective of the other or if they share some common language independent of their theoretical commitments. Scientific examples of each of these situations indicate that this form of theory dependence also does not inherently compromise objectivity (Brown 1995).

In sum, only the first two forms of theory dependence seriously compromise scientific objectivity. Most scientists and many philosophers of science view TD1 and TD2 as inconsequential given the importance of anomalies in science. The remaining genuine forms of theory dependence do not by necessity lead to subjectivity. On the other hand, TD3, TD4, and TD6 individually and collectively undermine the epistemic and semantic foundationalism of observation reports inherent to logical empiricism. The conclusions are that inherent epistemic relativism appears untenable, foundational forms of empiricism cannot be sustained, and despite the symbiosis of theory and observation, science can still produce objective knowledge. This naturalized perspective does emphasize, however, that science is considerably more complex, internalized, and fallible than logical empiricism would lead us to believe.

OBSERVATION IN GEOMORPHOLOGY

Superficially, the strong emphasis placed on fieldwork in geomorphology accords with the foundational view of observation embodied in logical empiricism. Geomorphologists who emphasize the importance of field observations (e.g. Baker and Twidale 1991), like classical empiricists, tend to draw an alleged distinction between theoretical and non-theoretical components of the scientific process. The basic message is that the true nature of the 'observed' (i.e. perceived) landscape will be revealed to those who venture forth into the field unshackled by theoretical constraints. This view undoubtedly can be attributed in part to the long-standing positivist traditions in geology and geography, the parent disciplines of geomorphology. The value of inductive reasoning from (implicitly theory-neutral) observation to theory has long been cherished in geology (e.g. Oliver 1991). According to Kitts (1977, p. xiv) 'the geological tradition has fostered from the beginning a radically empirical and therefore, in the minds of many geologists, a notably untheoretical geology'. The accuracy of this comment is reflected in the recent debate about the increase in deductive theoretical modeling in geology, which has caused some to yearn for a return to the golden age of Hutton or Gilbert, when the emphasis was on developing generalizations from field-based observational facts (Brown 1974; Baker and Twidale 1991). In geography, many geomorphologists subscribe to the logical empiricist view of science unwittingly because they have been trained in the methods of science by human geographers (e.g. Harvey 1969), who tend to view the physical sciences in a positivist vein (Rhoads and Thorn 1994). The long tradition of inductive empiricism in physical geography is still very much alive. A recent textbook emphasizes that the process of discovery in physical geography involves a pathway from observation of geographic phenomena to theoretical explanation (Bradshaw and Weaver 1993, pp. 27-29).

The persistence of an implicit undercurrent of radical empiricism within geomorphology is somewhat ironic, given that the discipline lacks any formal philosophical statements in support of this perspective. Perhaps the strongest indicator that geomorphologists have assigned a foundational status to observation is the paucity of serious debate about this aspect of scientific inquiry. Geomorphologists appear to have reached an implicit consensus about the adjudicatory primacy of observation, and thus do not regard this element of scientific inquiry as a topic in need of further elaboration. However, taken together, these comments suggest a dilemma. If geomorphologists have not engaged in serious discussion about observation, but have implicitly deemed it central to geomorphic inquiry, on what grounds is this judgment based? As noted by Schumm (1991, p. 2) many earth scientists seem content 'to go about their scientific endeavors without giving much thought to the manner in which they proceed'. Glib comments such as 'study nature, not books' have a certain intuitive appeal among field scientists, but in the end merely discourage philosophical introspection and in the process raise suspicion that as a group geomorphologists fear what such introspection might reveal.

Because geomorphologists have been reluctant to engage in serious philosophical examination of their scientific activities, it is not possible to turn to a substantial body of previous commentary by geomorphologists-cum-philosophers. Instead, insights must be gleaned from a small body of formal commentary about the process of geomorphic inquiry, from analysis of case studies, and from general deliberation on the character of the discipline. Certainly, a comprehensive examination of observation in geomorphology would require a depth of treatment beyond the scope of a single chapter. The following discussion seeks merely to determine whether particular philosophical perspectives inform the role of observation in geomorphology *in at least some instances.* The choice of specific topics included in the discussion is admittedly subjective, but includes historical as well as contemporary examples. A brief survey of the views of G.K. Gilbert and W.M. Davis on observation examines the character and role of observation in the research of these two founding fathers. This discussion provides a basis for evaluating the role of observation within contemporary geomorphology.

Observation in the Science of G.K. Gilbert

Most of G.K. Gilbert's formal commentary about scientific methodology is contained in two papers: Gilbert (1886, 1896). His emphasis in these papers on observation as a first step in scientific inquiry has led some to associate his methodology with pure Baconian inductivism (e.g. Gilluly 1963); however, his own words reveal a more sophisticated insight into the relation between observation and theory:

> Scientific observation ... 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 judgement 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 (Gilbert 1886, p. 285).

Gilbert clearly recognized that all observation is theory-laden to some extent, but he did not feel that this characteristic seriously compromised objectivity. His comment also suggests that he associated observation with perception; a conjunction that is not supported by his research. Gilbert (1886) went on to claim that observation is not indiscriminate, but rather is guided by theoretical presuppositions about classification. These presuppositions sharpen 'the vision for the detection of matters that are unnoticed by the ordinary observer' (Gilbert 1886, p. 285).

Gilbert (1886) introduced the concept of multiple working hypotheses as a safeguard against extreme forms of theory-ladenness that threaten objectivity. The purported advantages of multiple working hypotheses are that:

1. An investigator is less likely to be biased by excessive attachment to a 'pet' theory (thus avoiding TD1 and TD2).
2. Consideration of more than one hypothesis helps the investigator to determine which observations are most relevant for discriminatory tests (TD5) (Table 2.1).

Not only has the method of multiple working hypotheses enjoyed sustained support in geomorphology (Chamberlin 1890, 1897; Johnson 1933; Haines-Young and Petch 1983), it has been embraced within science at large under the rubric 'strong inference' (Platt 1964). Recently, the method has been the target of minor criticism within geology (Johnson 1990) and has been called into question more generally on rational and empirical grounds within the philosophy of science (McDonald 1992). In any case, geomorphologists have accepted this notion uncritically because of its intuitive appeal and have not rigorously evaluated its validity in actual practice. At a superficial level the method appears to foster healthy scientific skepticism; however, without rigorous empirical evaluation its true efficacy cannot be determined.

Background knowledge played an important role in shaping Gilbert's scientific methodology. His approach to hypothesis formulation and observation was greatly influenced by combined training in Newtonian mechanics and geology (Pyne 1980, pp. 95-103; Sack 1991). He saw the earth as a natural machine, operating according to mechanistic principles in which equilibrium played a central role. A hallmark of Gilbert's work is that he never presumed knowledge of cause, except in the sense that underlying causal mechanisms consisted of natural processes or events that obeyed the laws of physics and chemistry. Instead, he formulated various hypotheses about causal events and then proceeded to determine which evidence is relevant to, yet independent from, these hypotheses. Both of these steps relied heavily on background knowledge (Gilbert 1886, 1896). Gilbert's engineering mindset is also reflected in the value he placed on precise, reliable data, especially those produced by instruments. Although in formal discussion of scientific method Gilbert equated observation with perception (e.g. Gilbert 1886), the 'observations' he collected in actual practice are synonymous with contemporary notions of data and evidence.

Gilbert's analysis of Coon Butte (now known as Meteor Crater), which Gilbert (1896) himself presented as an archetype of his research methodology, illustrates how background knowledge played an important role in his research. In this investigation Gilbert drew upon his background knowledge in the earth sciences and physics to formulate two competing hypotheses about the possible origin of this feature, namely by volcanic explosion or by meteorite impact. Based on deductive reasoning and his familiarity with certain types of field techniques, Gilbert (1896, p. 5) then argued that 'any observation which would determine the presence or absence of a buried star might therefore serve as a crucial test'. Gilbert tested only the meteorite hypothesis explicitly. Although he did not directly test the volcanic origin hypothesis, he did present some qualitative arguments in support of it.

Gilbert noted that exploratory drilling beneath the crater floor would produce the best evidence for or against the meteorite impact hypothesis; however, because this option was too costly, he proposed two other options. His first test was drawn from the physical principle of conservation of mass; he deduced that if the crater was caused by a meteorite impact, part of the crater would be occupied by material from the meteorite, and the volume of the material surrounding the crater would be greater than the volume of the crater. Calculations based on Gilbert's topographic survey of the crater and its surrounding rim revealed that the two volumes are equivalent, which constituted disconfirmatory evidence for the hypothesis.

The second test was based on the assumption that the meteorite consisted primarily of iron, an assumption suggested by the scattered masses of iron around the crater, and on background knowledge concerning the magnetic properties of iron. The data in this case were drawn from a magnetic survey and consisted of 'observation ... of the three magnetic elements: the horizontal component of the direction, or the compass bearing; the vertical component of direction, or the inclination of the dip needle; and the intensity of the magnetic force' (Gilbert 1896, pp. 8-9). Gilbert predicted that the presence of an iron mass would produce systematic deviations in the compass readings; however, he (1896, p. 6) found that within the limits of measurement error the magnetism was constant in direction and intensity around the crater. Gilbert did not automatically view this result as evidence against the impact hypothesis; instead, he conducted a series of experiments at the Navy Yard in Washington, DC to determine the sensitivity of the compass to the presence of iron bodies of different sizes and different depths of burial. Gilbert's effort to determine the sensitivity of the compass to an iron body shows that he was deeply concerned about the reliability of the data as evidence for or against the impact hypothesis (e.g. Woodward 1989). Based on the results of the calibration experiments Gilbert (1896, p. 9) concluded that 'the theory of a great iron meteor is negatived by the magnetic results, unless we may suppose that the meteor was quite small as compared to the diameter of the crater, or that it penetrated to a very great depth'. Clearly, here is a case supporting Brown's (1995) contention that the failure to causally interact with something in the real world (in this case the failure of the compass needle to interact causally with a large iron meteorite) can be important evidence in an empirical test.

Although Gilbert was forced to conclude that the available evidence did not support the meteorite impact hypothesis, his critical attitude did not lead him to presume that the tests were conclusive. Gilbert proposed several possible scenarios that could account for this evidence even if the crater was produced by a meteorite impact. Although he cautioned that scientific results are 'ever subject to the limitations imposed by imperfect observation'

(Gilbert 1896, p. 12), it was not his observations that were to blame; the same results would be obtained today were the tests to be repeated. Rather, it was limitations in the background knowledge available at the time that led Gilbert to a conclusion (albeit tentative) about the origin of meteor crater that today is viewed as incorrect.

The Coon Butte example shows how Gilbert used the term 'observation' to refer to data. He did rely on visual information to infer that the depression known as Coon Butte was a crater; however, the evidence used in hypothesis testing was based not on visual information but on precise data collected with instruments. Whereas the functioning of the instruments relies on theories governing their operation, knowledge of these theories is not necessary to use the instruments. Moreover, Gilbert did not use raw data produced by these instruments directly in hypothesis testing, but instead employed theories of method and analysis to transform the raw data into evidence. He used surveying theory to convert instrument readings into volumetric values that could be used to test the volumetric hypothesis, which itself depended on conservation principles. In a similar fashion, the individual compass readings had to be arranged in a measurement scheme and the absence or presence of a spatial pattern be determined before the readings constituted evidence.

The Coon Butte example also shows that Gilbert was using data or evidence in a search for a causal phenomenon. This phenomenon could not be observed directly because it occurred in the geologic past; moreover, the infrequency of the phenomenon prevented Gilbert from 'observing' directly its effects on the earth's surface. In this geohistorical investigation, Gilbert was employing a combination of abductive reasoning and deductive inference. To develop his hypotheses he inferred possible cause (initial condition) from effect (antecedent condition) and background knowledge about geological processes (meteorite impacts, volcanic eruptions). The evidence for the effect was the characteristic shape of the depression, which Gilbert's background knowledge permitted him to recognize as a crater and consequently the product of only a limited number of possible geologic processes. The hypothesis under test (i.e. the origin of the crater) was a singular causal statement, rather than a generalization or law. This pattern of inference from effect to potential cause based on background knowledge of cause-effect relations is known as abductive inference; such reasoning is common in geohistorical investigations (Kitts 1963; von Englehardt and Zimmerman 1988), including those conducted in geomorphology (Rhoads and Thorn 1993). Abductive inference is greatly strengthened when it is accompanied by deductive reasoning in which the abductively inferred initial conditions are combined with physical principles to deduce other effects associated with the inferred cause (Kitts 1963; Rhoads and Thorn 1993). Gilbert followed this strategy when he combined the meteorite impact hypothesis with laws governing magnetism and mass conservation to deduce hypotheses, the testing of which relied on evidence independent of that involved in the initial abductive inference.

Although Gilbert's methods of hypothesis formulation, data collection, and data analysis depended on background theories, the type of data that resulted from the Coon Butte investigation was not strongly influenced by the hypothesis under test. Perhaps the ultimate measure of objectivity is whether one can explicitly state the conditions under which the hypothesis could be disconfirmed by the data. Gilbert clearly stated these conditions both for the survey and the compass data. Gilbert also elaborated in detail the relevance of the data to the hypothesis; again background theories (conservation principles; magnetic theory) played a key role.

In sum, Gilbert's example shows that his investigative style generally avoided the more serious forms of theory-ladenness (TD1, TD2) (Table 2.1). Gilbert's mechanical view of the world certainly influenced the manner in which he conducted his research, but it did not predetermine what he would 'observe' about the world, nor did it cause him to ignore evidence that contradicted what was most likely his favored hypothesis prior to the investigation, i.e. the meteorite impact hypothesis (El-Baz 1980). Although he used auxiliary theories to guide hypothesis testing, data collection, and analysis (TD3, TD4a), and reported results in the language of the hypothesis being tested (TD6), neither of these factors seriously compromised the objectivity of testing. Gilbert could have protected the meteorite impact theory by challenging auxiliary theories (TD4b); and in fact identified how such theories could be challenged, but in the end chose not to do so. The fact that Gilbert could clearly state the conditions under which the data would disconfirm the meteorite impact hypothesis suggests that the data do not presuppose this hypothesis in any viciously circular way (TD3). The method of multiple working hypotheses seems to have helped prevent vicious circularity by providing valid alternatives for explaining the existence of the crater; TD3 may be more of a concern where only a single theory is considered plausible, as in the relativity example cited by Brown (1993).

Perhaps the most important lesson to be learned from Gilbert's investigation of Coon Butte is that objectivity alone does not guarantee truth. Instead, improvements in the accuracy of hypothesis testing in the earth sciences occur as objectivity is combined with development and growth of background knowledge from other areas of science. Although objective testing did not prevent Gilbert from reaching an erroneous conclusion, the fault appears to lie, not in Gilbert's investigative style, but in the inadequate, relevant background knowledge in the late nineteenth century. Had Gilbert been armed with current theories of shock metamorphism and impact energy (e.g. Chao et al. 1960; Shoemaker 1960), he probably would have reached the conclusion that Coon Butte is the result of a meteorite impact (von Engelhardt and Zimmermann 1988, pp. 278-282, 285).

Observation in the Science of W.M. Davis

Whereas Gilbert's view was imbued with the principles of mechanics, Davis's perspective on geomorphology was influenced heavily by evolutionary theory (Stoddart 1966). During the late nineteenth century, the concept of evolution had a distinct nonbiological connotation wherein antecedent forms were considered as stages in an orderly sequence leading to subsequent forms (Chorley et al. 1973). In such a view, time itself is a process, not merely a framework within which events occur. This characterization suggests that the general concept of evolution in the late 1800s was more consistent with contemporary notions of ecological succession than it was with the modern biological theory of evolution. The concept of orderly sequential change obviously is one Davis drew upon to formulate his theory of the cycle of erosion, which he originally referred to as 'the cycle of life' (Davis 1885). The cycle of erosion, with its progressive development of landscapes through a series of stages, is clearly an example of a developmental theory, or one that specifies an irreversible process 'characterized by a certain direction or tendency so that beginning and end stages as well as intermediary stages may be differentiated' (von Engelhardt and Zimmermann 1988, pp. 322-323). Developmental theories often are substituted for causal theories (i.e. those that relate processes or mechanisms specifically to effects) for systems with complex interactions between structure and process (e.g. geomorphic systems) (von Engelhardt and Zimmermann 1988, p. 177).

Previous criticisms (e.g. Strahler 1950; Judson 1960; Chorley 1962, 1965; Flemal 1971; Higgins 1975; Beckinsale 1976) of the cycle have focused on the sociological factors or conceptual shortcomings associated with its downfall. Here discussion examines the manner in which theory and observation are interrelated within the Davisian scheme of geomorphic inquiry and how this interrelationship may have contributed to its eventual demise.

Davis's formal comments about the relationship between theory and observation were marked by inconsistency, suggesting that he saw this relationship as complex. At times, he drew a sharp contrast between theory and observation, at others, he discussed these concepts as intertwined. Davis claimed that 'the scheme of the cycle ... is by intention a scheme of the imagination and not a matter for observation, yet it should be accompanied, tested, and corrected by a collection of actual examples that match as many of its elements as possible' (Davis 1905, p. 152). Theory testing to Davis did not represent an attempt to compare independent evidence with precisely specified evidential conditions; instead it involved an effort to match elements of the cycle with selective pieces of morphological information from actual landscapes, which were then subsequently construed as evidence for these theoretical elements (Davis 1915).

Although Davis paid lip-service to the importance of proposing various hypotheses to explain observed facts (see Davis 1922), his dogmatic allegiance to the cycle of erosion conformed more closely with Chamberlin's (1897) notion of the method of the 'ruling theory', than with the method of multiple working hypotheses. Davis was willing to entertain various hypotheses about the development of a particular landscape only to the extent that they conformed with sacrosanct tenets of the cycle (e.g. rapid uplift followed by progressive, sequential change in landforms). Thus, his method of landscape analysis became a rigid scheme in which 'each element of the landscape is treated as the surface of a structural mass which has been carried forward from an initial form to some specified stage of development in the cycle of erosion. . . ' (Davis 1911, p. 30). In other words, the cycle served as a regulative principle for developing geohistorical theories for specific landscapes in much the same sense that mechanics served as a regulative principle for Gilbert. The important issue is that whereas by the late nineteenth century physical principles of mechanics had been extensively tested and widely applied to natural systems, such was not the case for the sequential, directed evolutionary concepts embodied in the cycle of erosion. More recent analysis by Chorley (1965) reveals that in fact the cycle can be reduced to a physical interpretation, and that this interpretation is inconsistent with contemporary knowledge of the physical mechanisms that govern the geomorphic processes responsible for landform change. Thus, not only was the background knowledge supporting the cyclic conception much weaker than that supporting Gilbert's mechanical conception, but the cyclic conception also could not readily accommodate the infusion of new background information into the discipline from other areas of science.

Bishop (1980) has argued that the cycle of erosion is inherently nonfalsifiable because of the irrefutability of the central concept of stage. This assessment is a bit misleading. Certainly, one can conceive of types of evidence, such as experimental studies of drainage basin evolution (e.g. Schumm et al. 1987), contemporary information on plate tectonics, rates of orogenic uplift, and isostatic adjustment (e.g. Summerfield 1991), and absolute dating of preserved paleolandscapes (e.g. Young and McDougall 1993), that, had they been available in the late nineteenth century, may have caused Davis and his followers to modify or even abandon completely the basic tenets of the cycle. Because the cycle is an evolutionary theory, the best type of evidence for testing it is temporal data. The problem with the cycle is not that it is inherently irrefutable, but that it was assessed with a type of evidence that in essence made it irrefutable (i.e. information on existing morphology). Moreover, important types of temporal evidence required to test it rigorously often could not be readily produced due to the unavailability of appropriate techniques for generating these data. It is also true that when applied to the interpretation of a specific landscape, the evidence required to refute a cycle-based explanation may for all practical purposes be impossible to obtain, no matter what information-gathering procedures are available.

Another reason the cycle of erosion was difficult to refute is that Davis lacked a healthy dose of skepticism concerning the validity of his ideas. In contrast to Gilbert, Davis never specified exactly the conditions or type of evidence required to refute the basic tenets of the cycle. Instead, when faced with observational evidence that was seemingly inconsistent with the theory, Davis introduced 'various kinds and degrees of interruption at various stages in a cycle' with the remarkable result that 'the variety of possible combinations becomes so great that there is no difficulty whatever in matching the variety of nature' (Davis 1905, p. 156). As Bishop (1980) has correctly noted, such modifications were often *ad hoc* in the sense that they made the cyclic theory less, rather than more, falsifiable. This strategy was adopted because of Davis's bold assurance in the fundamental correctness of the basic tenets of his theory. On one occasion he even proclaimed that 'explanatory concepts, deduced from general principles, are much more intimately and reasonably knowable than empirical concepts or even than facts of observation usually are, and in this quality of being intimately and reasonably knowable lies their highest value' (Davis 1912, p. 106). Davis recognized the limited power of our senses in observation, but unlike most scientists, he felt that theorizing, and not the development of sophisticated nonsensory observational techniques, was the best way to overcome this limitation:

> If we wish really to understand the natural world, surely those of its phenomena which are not immediately detectable by our limited senses must be detected in some way or other; and the way usually employed is - theorizing (Davis 1922, p. 198).

Davis clearly associated observation with visual perception, rather than with the collection of field data with instruments (Beckinsale 1976). In this particular sense, he was a consummate empiricist. A global traveler, Davis based his theories mainly on personal examination of many landscapes around the world. He also relied heavily on maps, which he used primarily to supplement visual information on landscape form, rather than as a source of quantitative data (Chorley et al. 1973, p. 232). Whether he was in the field or inspecting maps, Davis's genetic perspective strongly influenced his way of observing the landscape. It is this aspect of his methodology that is problematic.

The genetic-historical approach of Davis involved morphogenetic classification, i.e. assigning landforms to categories based on their morphology and genesis. Davis certainly was not the first to use morphogenetic terms to describe observations; however, by embedding such terms within an overarching evolutionary model, he developed this approach ('explanatory description') to an extreme. Although Davis (1913, pp. 686-687) claimed to recognize the 'explicit distinction between ... inferred conclusions ... and the observed facts on which the conclusions are based', in many cases this distinction became blurred when morphogenetic terms were used in conjunction with cyclic principles to interpret the landscape.

Davis's method of analysis in specific instances often was complex (Chorley et al. 1973, p. 212); however, some general aspects can be identified. Davis commonly stressed the importance of confronting theory with observed facts (e.g. Davis 1909, pp. 342-343). To Davis, 'observed facts' were uninterpreted morphologic features, such as accordant ridge tops. Yet assignation of a feature to a category, such as 'accordant ridge tops', is based in part on inference, an inference that must be supported by evidence independent of the theory used to explain the development of the landscape of which the ridge tops are a part. Davis, on the other hand, viewed the first stage of inference as occurring at a higher level when the observed evidence is used to assign the feature to a morphogenetic category, e.g. when accordant ridge tops are recognized as the remnants of a former peneplain. This inference from 'morphologic fact' to 'landform associated with a particular stage of the cycle of erosion' was an abductive one within which the cycle itself played the role of law. Davis readily acknowledged that morphogenetic terms are inherently theory-laden in that they implicitly involve inference about the genesis of the feature being observed. Nevertheless, he advocated the use of morphogenetic terms on the grounds that their theory-ladenness promoted deeper insight into the nature of landscapes than mere empirical description (Davis 1912).

The first stage of inference was followed by a second, more complex stage of reasoning, the purpose of which was to adduce the history of the landscape based on (1) the results of the morphogenetic classification of the first stage, (2) 'facts' concerning the stratigraphy and geologic structure of a region, and (3) the cyclic model. This stage also involved abductive inference, in which the inferred resulting states of affairs (e.g. peneplains, graded rivers, patterns of folds, stratigraphic sequences, etc.) along with the cyclic model, which once again served as inviolate law, were used to infer the controlling state of affairs (sequence of events that produced the current physiography). This two-stage chain of abductive reasoning is common in geohistorical investigations (von Engelhardt and Zimmermann 1988, p. 216).

Although Davis often presented his arguments in deductive form, the objective of his analysis was to determine initial (historical) conditions, not the effect of these conditions. Because he did not have direct knowledge of the initial conditions, his method cannot be properly characterized as deductive. However, Davis had great confidence in his cyclic theory, and in many instances he treated it as certain knowledge. Thus, in contrast to Gilbert, Davis proceeded as if he knew the cause and character of landscape development, and simply had to deduce the effects. This assumption resulted in a circular pattern of reasoning in which Davis deduced effects (resultant landforms) from uncertain knowledge of initial conditions and untested principles of landscape evolution, and then used the perceived match between deduced and observed forms as evidence for the correctness of his postulates (initial conditions combined with cyclic principles) - a problem alluded to by Flemal (1971) and Bishop (1980). However, because cause was inferred, and effect does not guarantee cause, this pattern of reasoning was specious.

The hierarchical chain of abductive reasoning involved in Davisian explanation was only as solid as the initial evidence upon which it was based. This evidence usually consisted of inexact visual information on morphology, which had to be processed through the human mind and therefore had a high potential for subjective bias. Davis was aware of the influence of theoretical preconceptions on visual perception, but apparently felt this influence was an advantage, rather than a problem:

> The more complete the mental scheme by which an ideal system of topographical forms is rationally explained, the more clearly can the physical eye perceive the actual features of the land surface … (Davis 1894, p. 68).

Davis rarely based his inferences about landscape genesis on any evidence other than subjective, theory-laden visual perceptions of landscape morphology. Thus, the critical independent evidential support so critical to geohistorical inference usually was absent from Davisian analysis. Objectivity suffered as Davis and his followers began to 'see' the landscape in cyclic terms.

The debate about peneplains between Davis and Ralph S. Tarr illustrates rather well the circular nature of Davisian analysis. Tarr (1898) began this debate by expressing concern about the weak evidential basis for identifying peneplains. He clearly recognized that the identification of these landforms was highly inferential and required strong supporting evidence. Tarr (1898, p. 36 1) felt compelled to argue against the concept of peneplanation, but was frustrated by limited data that made it 'difficult to find positive evidence against this explanation'. Most of his arguments were not evidential in nature, but were based on reasoning that mixed accepted background knowledge of geologic processes, which at the time was inadequate to decide the issue, with various speculative assumptions. Unconvinced, Davis (1899), the master of science by debate, responded in kind to Tarr with equal or greater effectiveness.

Tarr (1898) did, however, present some quantitative evidence to support his argument that the ridge crests in New Jersey and southern New England do not represent a relict peneplain. Because Tarr (1898, p. 356) believed that evidence based on 'appearance to the eye may be most deceptive', he used topographic maps to plot profiles across the regions of supposed accordant summits. These profiles revealed 'a very distinct lack of uniformity of the upland crests, even if all the lower hills are eliminated' (Tarr 1898, p. 356). Local differences in elevation exceeded 500 feet (150 meters) and regional differences were in some cases greater than 1100 feet (335 meters). Tarr (1898) noted that advocates of the peneplanation theory accounted for variations in elevation by proposing that they are the result of regional tilting and the preservation of monadnocks following uplift. He clearly recognized that without independent evidence to support these inferences, the explanation becomes theory-determined:

> I am unable to find that there is any other proof that this interpretation is correct than that which comes from the necessity of such an explanation, made necessary by first accepting the existence of the peneplain (Tarr 1898, p. 358).

Tarr was concerned that a single piece of highly dubious information (observation of pseudo-accordant summits with a regional trend) was being used to support multiple, interrelated inferences (former existence of a peneplain covered with monadnocks, uplift and tilting of the peneplain, preservation of monadnocks following uplift and erosion). Tarr (1898) also used topographic maps to determine the areal extent of summits with elevations within 300 feet (91 meters) of one another in relation to the total area of all summits. This analysis revealed that summits within 300 feet (91 meters) of one another occupy only 10-25% of the total summit area. In other words, 75-90% of the peneplain was being 'filled in' on the basis of reasonable accordance among 10-25% of the summit crests. Based on these results, Tarr (1898, p. 359) concluded that 'there seems to be very little real evidence upon which to construct the ancient peneplain, and I am led to raise the query whether, even granting in its entirety the evidence claimed, we would be warranted in drawing so broad a conclusion from so small a basis of fact'.

Davis's response to Tarr was nothing short of astounding, revealing just how strongly the cyclic theory infected his science. With regard to the topographic evidence on discordance among the summits Davis remarked:

> Considerable as the inequalities of altitude are, frequent study of the maps and repeated views of the uplands from various hill tops impress me much more with the relative accordance of their altitudes than with their diversity. I cannot admit that the appearance of accordance from hill top to hill top is an optical deception. There is an important matter of fact behind the appearance (Davis 1899, p. 210).

To Davis, the eye was more accurate than the surveying instrument on which the map is based! The fragmentary nature of the so-called peneplain did not strike Davis (1899, p. 212) as a 'serious or novel difficulty' because 'geologists are often compelled to work on fragmentary evidence'. Extending a peneplain based on a few fragments was no worse than extending a rock formation based on scattered outcrops. As a rejoinder to Tarr's concern about introducing tilting and monadnocks to explain variations in summit elevations, Davis simply retreated to the cycle to defend the cycle:

> the lack of uniformity of the uplands - a fact perfectly familiar to those who accept the peneplain idea - is partly the result of tilting ... and that for the rest the unevenness of the uplands of to-day is a natural result of imperfect peneplanation followed by submature dissection (Davis 1899, p. 210).

Davis (1899, p. 212) fully agreed with Tarr’s assessment that one had to accept the existence of a peneplain with monadnocks on faith and then assume it was tilted after it was uplifted: 'I have repeatedly insisted that it was only by recognizing the existence of a peneplain that uplift or deformation could be determined in certain cases....' Arguing that it would be 'as extraordinary to find no slanting peneplains as to find no inclined strata', Davis (1899, p. 213) stated that 'it does not seem warranted to conclude that the peneplain theory is invalidated because certain peneplains are now uplifted on a slant... '. In these statements, Davis merely reaffirms Tarr's (1898) concern that cyclic explanations are excessively theory-laden.

With regard to the existence of monadnocks, Tarr (1898) challenged Davis to produce independent evidence showing that the most discordant summits are underlain by the most resistant rocks. Davis (1899, p. 219) admitted that he 'has given no particular attention to monadnock rocks; indeed, it has generally seemed to me reasonable to infer their greater resistance on account of their form'. He also stated it is equally encumbent upon Tarr to produce independent evidence for or against the existence of monadnocks. Davis concluded this discussion by insisting that he is not an advocate of the theory of peneplanation, but in disagreeing with Tarr is merely following the guidance of the best evidence he can find:

> ... I cannot say too emphatically that the peneplain idea shall find no 'defense' from me. Let us all set forth the pros and cons to the best of our ability, and then the peneplain idea must look out for itself, and stand or fall according to its value (Davis 1899, p. 221).

Fortunately for Davis, the fall did not come until after he had died.

The exchange between Davis and Tarr suggests that at least in some instances Davis's approach to observation encompassed all six forms of theory-ladenness (Table 2.1), including the extreme forms, TD1 and TD2, that seriously compromise scientific objectivity. Davis's strong adherence to the cycle infected his way of perceiving the landscape; he 'recognized' relict peneplains as 'an important matter of fact' based on visual perceptions of accordant ridge tops that were not in fact accordant (TD1). He also ignored, or at least dismissed as unimportant, precise data which showed that these ridge tops were not accordant (TD2). He was able to reject these data as evidence against his hypothesis because cyclic explanation did not specify precisely the conditions required to refute the underlying assumptions upon which it was based. This extreme theory-ladenness resulted in a substantial degree of incommensurability between Tarr and Davis. Throughout his response to Tarr, Davis repeatedly retreated to the cycle of erosion to defend peneplanation and the existence of relict peneplains. Thus, he had difficulty 'interpreting' the evidential status of information outside of the context of cyclic explanation. This difficulty also led to a certain amount of exasperation on Davis's part concerning Tarr's inability to 'see' what Davis saw, an exasperation that is expressed succinctly in the following statement about whether or not the uplands in Connecticut and Massachusetts are accordant:

> Hence it must be agreed that with the same facts before us, both outdoors and on maps, our descriptions and interpretations of them do not correspond; one of us being impressed with the diversity of upland altitudes, and the other with their accordance (Davis 1899, p. 211).

Later in his career, this inability to extricate himself from cyclic thought helped to fuel the famous debate over landscape development between Davis and Walter Penck (Chorley et al. 1973).

Finally, the constructivist element of Davisian geomorphology should not be underestimated. In his reply to Tarr, Davis recounts the following story about a trip to the upper Mississippi River basin to visit a friend:

> During an excursion to that part of the country, I pointed out the very even skyline of a dissected peneplain. My friend dissented, thinking no such special explanation necessary; ordinary denudation would suffice, he thought, to produce the observed forms, without specification of control by different baselevels. A year later, on meeting the same friend, our talk happened to turn on peneplains, and he said: "I should like to show you an excellent example of that sort of thing," proceeding to describe the very region we had seen together. "How" I asked, "did you come upon that explanation?" "I cannot say precisely how," he replied; "it is nothing new" (Davis 1899, pp. 222-223).

Although Davis (1899, p. 223) attributed his friend's change in attitude to 'the unconscience encouragement given to the idea of peneplanation in a quiescent environment', it seems equally, if not more likely that the friend had fallen victim to the influence of a powerful personality. Davis certainly had a deep, profound influence on the inchoate discipline of geomorphology during the first half of the twentieth century (Chorley et al. 1973; Pyne 1980, pp. 254-261; Tinkler 1985, pp. 152-153). Davis's powerful personality, along with the simple elegance of his model, created a situation in which adherents of the cycle of erosion had difficulty viewing the landscape in terms other than those proposed by Davis (Hart 1986, p. 21). In the end, extreme theory-ladenness probably had as much to do with the downfall of Davisian geomorphology as other factors.

Observation in Contemporary Geomorphology

The assessment of observation in Gilbert's and Davis's science provides a context for evaluating the character and epistemic function of observation in contemporary geomorphology. Any attempt to identify the inauguration of contemporary geornorphology necessarily will be subjective. However, one possible point of departure is the benchmark paper by Strahler (1952), in which he announced the need for a process-oriented approach to geomorphic research grounded in the principles of physics, chemistry, and biology.

The 'return to Gilbert'

A careful reading of Strahler (1952) indicates that the primary focus of his paper is the call for a shift in the underlying basis for explanatory theory in geomorphology. Geomorphologists were coming to the conclusion that the Davisian approach was limiting in the sense that it did not readily accommodate background knowledge from other areas of science, particularly physics and chemistry, which was perceived as relevant to geomorphic phenomena. By relating geomorphic phenomena to background knowledge from other areas of science, geomorphologists were infusing the discipline with a large body of well-established theoretical presuppositions that could be used to guide the formulation of geomorphic theories. This background knowledge served several purposes. First, it enhanced the scope of laws and principles used in geohistorical inferences, while at the same time subsuming the Davisian model and highlighting the limitations of this model (e.g. Chorley 1965). Second, it provided a conceptual basis for field and laboratory investigations that seek to disclose the physical, chemical, and biological mechanisms constituting geomorphic processes. Third, in stark contrast to the Davisian scheme, it provided a flexible framework for importing new background knowledge into the discipline. Over the past 45 years this framework has easily accommodated the infusion of a large variety of scientific concepts into geomorphology, including those of systems (Chorley 1962), thermodynamics (Leopold and Langbein 1962), mathematical physics (Scheidegger 1961; Middleton and Wilcock 1994), equilibrium (Thorn and Welford 1994), catastrophe theory (Graf 1979), and nonlinear dynamics (Phillips and Renwick 1992) to name a few. Fourth, because physical and chemical principles have been and continue to be viewed by geomorphologists as 'established', they also play an evidential role in rational decisions among competing geomorphic hypotheses. Geomorphologists, like geologists (e.g. Kitts 1974), do not use physical and chemical laws merely as inference tickets, but proceed on the assumption that these laws constitute inviolate 'truths' about nature; therefore, all geomorphic theories must not only be consistent with these laws, but should embody them. Judgments about the explanatory adequacy of competing theories commonly reflect this underlying sentiment. Geomorphic theories that explicitly and formally include physical or chemical laws are often seen as having greater 'explanatory adequacy' than alternative theories that have greater empirical adequacy, but are only loosely or informally tied to underlying physical/chemical principles.

Strahler (1952) had surprisingly little to say about the implications of his proposed shift in the conceptual basis for geomorphology either for observation or for the manner in which geomorphologists conduct scientific inquiry. However, the discussion near the end of the article, which is littered with terms such as 'data', 'variables', and 'quantitative determinations of landform characteristics and causative factors', provides a hint of what this shift was to imply for observation in geomorphology. Ironically, despite Strahler's (1952) emphasis on implications for explanatory theory, the shift in background knowledge had its greatest immediate impact on the character of geomorphic observation. Initial studies under the new process-based approach focused largely on data-driven analyses (e.g. Leopold and Maddock 1953; Strahler 1957; Melton 1958; Chorley 1966), leading to concern and criticism that the discipline was sliding into raw empiricism (Mackin 1963). These 'empirical studies', however, did not occur in a theoretical vacuum but were performed within the context of statistical theory, systems concepts, and incompletely specified physical theory. It was not until the appearance of Scheidegger's (1961) landmark treatise that physical theory was employed in a formal, mathematical fashion to develop explanatory geomorphic theories in the manner envisioned by Strahler (1952). Since that time there has been steady growth in the infusion of physical, chemical, and biological theory into geomorphology, both at the explanatory theoretical level and the exploratory observational level.

The emergence and rise to dominance of the process-oriented approach to geomorphology over the last 40 years have been referred to within the geomorphic community as a 'return to Gilbert'. This characterization is appropriate not only in the sense that systems concepts and physical principles once again provide the basis for explanatory inference in geomorphology, but also in the sense that geomorphologists now 'observe' geomorphic systems and use their observations in a manner consistent with Gilbert's example. This contemporary approach to observation in geomorphology also is generally consistent with the naturalized view of observation in the philosophy of science.

The character of geomorphic observations

One need only refer to a volume such as *Geomorphological Techniques* (Goudie 1990) to recognize that geomorphologists now largely equate the term 'observation' with quantitative data, rather than visual perception. This characterization is as true for geohistorical investigations as it is for process-oriented studies. The term 'technique' emphasizes the procedural, rule-based nature of contemporary observational methods in geomorphology. These methods range from relatively simple procedures (e.g. taping of distances) to highly sophisticated analytical techniques (e.g. atomic absorption spectrometry, remote sensing of planetary surfaces). The sophistication of observational techniques clearly has increased over the last 45 years and all indications suggest that this trend will only accelerate in the foreseeable future. By embracing a conceptual framework based on established background knowledge in other areas of science, geomorphologists have positioned themselves to take advantage of a smorgasbord of technological developments. Like many other sciences, geomorphology, is being fueled and invigorated by these technological developments.

In accord with the naturalized view of observation, the sophistication of a specific geomorphic technique clearly is not viewed as a deterrent to its reliability. In fact just the opposite is true. Geomorphologists readily adopt sophisticated observational techniques because they view data generated by these techniques as far more reliable than impressions resulting from visual perception. What geomorphologists often fail explicitly to recognize, is that because all observational techniques depend on background theories, the data resulting from them are theory-laden to some extent. Even when data are derived solely from visual perception, such as when sedimentary structures are identified in a depositional sequence, background theory plays an essential role in data production. It is the background theory (in this case from sedimentology) that allows the investigator to convert raw sensory stimuli into meaningful data. Explicit recognition of the theory-ladenness of observation seems to have escaped many geomorphologists, even though this issue has been discussed in relation to geology by Kitts (1974) and to physical geography by HainesYoung and Petch (1986, pp. 37-40).

Observation and objectivity in geomorphology

Given that geomorphic data are theory-laden, a crucial concern is whether this theory-ladenness seriously undermines the objectivity of empirical testing. Through their temporary detour into the world of Davisian science, and their subsequent return to Gilbert, geomorphologists seem to have learned implicitly that at least five factors influence the objectivity, reliability, and certainty of empirical testing:

1. The degree of independence between exploratory and explanatory theories;
2. The availability of independent information on cause and effect (as embodied in scientific arguments);
3. The degree to which information on cause and effect has been modified, altered, or confounded by environmental factors;
4. The fixedness and precision of observational techniques;
5. The fixedness and precision of theoretical arguments with which the empirical evidence is compared.

Although these five components are isolated for the purpose of discussion, in actuality they are often interrelated in specific instances.

The discussion in the first part of this chapter has shown that independence between exploratory theories and explanatory theories contributes to objectivity by helping to ensure that data constitute unbiased evidence, thereby preserving the adjudicatory power of data. In many cases in geomorphology, the theory (theories) underlying an observational technique is (are) independent to some degree from the explanatory theory under test:

independent in the sense that the outcome of data production will not, by necessity, be determined by the explanatory theory alone (i.e. if the explanatory theory is incorrect, the data can be inconsistent with theoretical expectations). For example, a fluvial geomorphologist who uses an electromagnetic flow sensor to evaluate a hydrodynamic theory of stream channel dynamics will draw upon the explanatory hydrodynamic theory to define the variables to measure and to design the sampling scheme required to produce appropriate spatial and temporal coverage within the stream. It is also true that the operation of electromagnetic current meter depends in part on hydrodynamic theory, a dependency that is evident in calibration experiments designed to assess the sensitivity of the sensor to various types of flow conditions (e.g. Lane et al. 1993). In other words, the data resulting from the measurement program depend at least to some extent on the explanatory theory. Although this dependency partly influences the type of data obtained from the overall measurement program, it does not wholly predetermine individual data values because the operation of the sensor also relies on electromagnetic theory, which is largely independent of hydrodynamic theory. Of course, the role of independence in objectivity is predicated on the sensor's capability to interact causally with the phenomenon of interest in the real world.

Independence between exploratory and explanatory theory is common in geomorphology because geomorphologists draw upon a background of established theoretical principles from other areas of science to formulate their theories and to select observational techniques, rather than devising fundamentally new principles or developing observational techniques that involve underlying principles which are poorly understood (as sometimes occurs at the forefront of physics, chemistry, or biology). In most instances, the issue of independence can be decided a priori based on consideration of the types of theoretical knowledge underlying the explanatory theory and a potential observational technique. Such a priori decisions are possible because the issue of independence has largely been resolved by the parent discipline in which the theory underlying the technique originally emerged. For example, an adequately trained fluvial geomorphologist need not deliberate long and hard about testing a hydrodynamic model of channel change with survey data on channel form because the independence of surveying theory and hydrodynamic theory is apparent from his/her store of background knowledge. The lesson to be learned is that geomorphologists should become familiar with a broad spectrum of background knowledge and stay current on recent developments within other areas of science so they put themselves in an optimal position to employ this knowledge to their full advantage.

Independence between exploratory and explanatory theory helps to ensure that data are reliable evidence; however, the degree of certainty and reliability assigned to empirical testing also hinges on the capacity of an investigation to generate independent evidence for cause and effect. The results of studies that produce multiple types of independent evidence both for cause and for effect are viewed as more reliable and certain than results that include only single pieces of evidence for cause and effect, that include evidence for effect only, or that are based on evidence that is known to be highly modified, altered, or otherwise confounded. Many contemporary process-oriented research programs are designed to yield (at least over a series of investigations) various types of independent evidence for cause and effect. For example, an investigation directed toward evaluating a theoretical model of meander bend dynamics may use independent measurements of velocity fields, water surface topography, and patterns of bedload transport to corroborate the existence of predicted hydrodynamic phenomena, and independent measurements of bed topography and grain-size patterns to document how the existence of the predicted phenomena produce certain expected effects (in this case changes in equilibrium channelbed conditions). If strong agreement is found between model predictions and evidence, the results are accorded a high degree of reliability and certainty.

In geohistorical investigations, data on cause cannot be obtained directly because the cause occurred in the past. Thus, geohistorical inferences inherently have a weaker empirical basis and greater degree of uncertainty than inferences derived from process-oriented studies (Rhoads and Thorn 1993). This characteristic of geohistorical inferences in no way is the result of an inferior methodology or scientific approach; instead it reflects a limitation imposed on such investigations by nature. Kitts (1977) has emphasized the crucial importance in geohistorical investigations of obtaining multiple types of independent evidence on effects to support inferences from effect to cause. Gilbert's (1896) investigation of Coon Butte is a nice example of this approach. On the other hand, the exchange between W.M. Davis and Tarr illustrates how inferring cause (i.e. origin and genesis) from a single piece of evidence on form (accordant summits) can lead to science by debate. The danger involved in basing an inference about the cause of a landform on a single type of evidence, especially perceptual information on morphology, has been recognized for some time. Russell (1949, p. 3) remarked that it makes about as much sense to infer the development of a landform based on form alone as it does to amputate one's leg before entering a footrace.

The exchange between Tarr and Davis also emphasizes that the problem of decisive testing is enhanced when available evidence on effect is weak or ambiguous because it has been highly altered or modified over time. Recent work indicates that explanatory theory is less likely to strongly influence visual perception and interpretation if the incoming sensory information is strong; on the other hand, weak or ambiguous sensory inputs often lead to disagreement, even among individuals with shared background knowledge (Brewer and Lambert 1993). Brown (1995) has shown that even when two individuals have separate conceptual backgrounds clear evidence can prevent incommensurability as long as the two individuals have some shared basis for communication - a situation that certainly exists within geomorphology (or any other scientific discipline) where proponents of different theories share enough knowledge to provide a basis for common ground. The need for multiple types of independent evidence on effects is especially acute when each of the types, considered individually, is ambiguous. Debate about the cause of a particular relict landscape feature can be tentatively resolved if several pieces of weak evidence, when considered in combination, are consistent with only one of several possible explanations.

Another factor that contributes to the objectivity of testing in contemporary geomorphology is the definiteness and precision of observational techniques. Most contemporary techniques involve standardized rule-based procedures. In many cases, geomorphologists have imported techniques from other disciplines and thereby have *de facto* adopted established conventions for implementing the technique. An example is sieve analysis of sediments. Geomorphologists who use sieve analysis are expected to follow certain procedures for the data to be reliable (i.e. unbiased, capable of being reproduced with minimal error). These procedures transcend disciplinary boundaries; they represent standards that apply to the entire scientific community.

Standardized, rule-based observational techniques contribute to scientific objectivity by increasing the precision and intersubjective repeatability of data (or observations). By reducing the potential for different investigators to obtain different information about a specific property of an object of investigation, such techniques minimize the volitional character of the data and improve the basis for consensus about its evidential status. In other words, conventional procedures enhance the reliability of data as evidence for or against an explanatory theory. This concern about reliability of data explains why geomorphologists, along with other scientists, are interested in the capacity of a procedure to isolate information of interest (i.e. its sensitivity to confounding factors or background noise) and to replicate results precisely in calibration experiments and in investigative applications (Woodward 1989). The importance of calibration to objectivity has been demonstrated by Franklin et al. (1989), who argue that even in cases where the technique used to test a theory is an instance of the theory, the technique can still be used objectively if it is calibrated against known information (determined, of course, by an alternative, independent technique). Such cases may occur in geomorphology, but probably are rare. Most geomorphic techniques are drawn from other areas of science; thus, the issue of independence is obvious a priori, based on casual consideration of background knowledge in the area of science from which the technique derives.

Once again the contrast between Davis and Gilbert illustrates these points. Gilbert relied on data derived from precise surveys and compass readings to test his hypotheses concerning the origin of Coon Butte. Both the surveying procedure and use of the compass conform with standardized procedures that would still be used today were the measurements to be repeated. Gilbert also performed calibration experiments to assess the reliability of his data. Davis, on the other hand, used visual perception as his primary source of information on the relative elevations of adjacent ridge tops; moreover, when faced with precise data on these elevations from maps, he rejected it as misleading. The disagreement between Tarr and Davis about the visual appearance of the ridge tops shows that this observational technique produced data that was neither precise nor intersubjectively repeatable. Davis could use perceptual information to his advantage because he did not (probably because he could not) specify standardized rules for determining the elevations of ridge tops visually. This specific example highlights the importance of mensuration as a component of observation. Contemporary geomorphology, perhaps as an unwitting consequence of implicit dissatisfaction with Davis's visual approach to observation, has fully embraced mensuration in an effort to avoid the subjectivity that accompanies testing of inferences with impressionistic evidence.

The complement of definiteness and precision of observational techniques is definiteness and precision of theoretical expectations. The more rigid the limits placed on these expectations, the more restrictive the theory becomes regarding the data. The combined influence of invariable, tightly prescribed observational techniques and theoretical expectations defines the 'fixedness' of the observational setting (Hudson 1994). Assuming a technique has the potential to interact causally with real-world phenomena (i.e. produce involuntary information), the chances of obtaining informative data about the theory under test are greatest when the observational setting has a high degree of fixedness (i.e. a precise theoretical expectation is tested using a standardized, precise observational technique). In fact, Hudson (1994) has argued that as long as the theory provides fixed constraints on expectations, dependence of the observational setting on the theory under test actually enhances, rather than detracts from, objective testing because it is this fixedness that leads to 'unexpected' (i.e. disconfirmatory) results. For example, consider the extreme case where a geomorphologist proposes the theory that all rivers are 3 meters wide, and then tries to prejudice testing of the theory by: (1) developing a measuring device that has a length equal to the distance between the banks of several nearby rivers, all of which happen to have the same absolute width; (2) assigning a value of 3 meters to the length of this device. Clearly here is a case where the observational technique depends incestuously on the theory under test; it has been intentionally rigged to provide only confirmatory evidence for the theory under test. However, despite this dependency, the device can in principle be used to provide disconfirmatory evidence for the theory, provided the observational setting remains fixed. Suppose a flood erodes the banks of the streams, increasing the distance between them by twofold. If the same device is used to measure the widths of the channels after the flood, it will produce disconfirmatory evidence for the theory *as long as the rules governing the observational procedure are not changed arbitrarily (e.g. the value of 3 meters is not arbitrarily reassigned to the new bank-to-bank distance) or the theoretical expectation is not salvaged through the introduction of* ad hoc *auxiliary hypotheses (e.g. the 'real' channel is 3 meters wide, the other 3 meters of width constitute a separate entity called 'excavated space beside the channel following a flood').*

The concept of fixedness draws attention to a fundamental shortcoming of the Davisian scheme of landform analysis. By not specifying precisely theoretical expectations, Davis was able to accommodate virtually any anomalous data, even data generated by standardized rule-based procedures (e.g. Tarr's data on ridge-top elevations), through the introduction of *ad hoc* assumptions designed to preserve the evidential basis for an explanatory theory. In contrast, Gilbert (1896) clearly recognized the importance of a fixed observational setting and did not attempt to salvage the meteorite impact hypothesis for Coon Butte by introducing *ad hoc* qualifications, even though it was probably his preferred explanation.

The emphasis on precise data and quantitative modeling in contemporary geomorphology shows that geomorphologists implicitly recognize the value of fixed observational settings. On the other hand, unlike theorists in foundational sciences, geomorphologists generally treat certain theoretical principles, especially those from physics and chemistry, as sacrosanct. When data obtained within a fixed observational setting do not conform with theoretical expectations based on physical principles, geomorphologists do not discard the physical principles. Instead, they view the data as evidence *against the particular specification of these principles, including the underlying assumptions.* The explanatory theory may have invoked the wrong principles, combined these principles in an incorrect manner, or incorporated too many untested assumptions, but the fundamental validity of physical and chemical laws is unquestionable. In this sense, chemistry and physics provide foundational principles for explanatory theories in geomorphology, much as the cycle of erosion was foundational in Davisian geomorphology. In contrast to Davis, contemporary geomorphologists are willing to view specific formulations of a physically based theory as incomplete or even incorrect based on empirical testing. *A posteriori* qualifications are introduced not to salvage the evidential basis of a fixed explanation, but to modify the explanation so that it yields a new set of precise theoretical expectations that must accommodate the fixed observational data.

The Search for Geomorphic Phenomena

A final issue is the manner in which observation contributes to the general goal of geomorphology: to generate knowledge about landforms and landform-shaping processes on the earth and other planetary surfaces at a variety of temporal and spatial scales. An attribute of explanatory theory that is highly valued among scientists, including geomorphologists, is specification of cause and effect. As discussed in the first part of this chapter, many causal phenomena cannot be perceived directly; instead their existence must be inferred from and justified by data (Bogen and Woodward 1988; Woodward 1989). This situation is common both in process-oriented and geohistorical geomorphologic investigations, but for slightly different reasons in each case.

Process-oriented studies, such as field experiments dealing with stream channel dynamics, are designed to test theories that refer to imperceptible causal processes (phenomena) responsible for landform development and change (e.g. boundary shear stresses associated with a distinctive pattern of fluid motion). Data are gathered both to document the existence of the processes specified by an underlying physical theory as well as to evaluate the impact of the processes on landscape morphology (e.g. structuring or restructuring of the bed and banks of the stream channel into a specific morphology). In such cases, geomorphologists clearly are engaged in a search for causal phenomena. Because the phenomena of interest are assumed to be governed by physical, chemical, or biological principles, explanatory theories commonly include such principles. However, it is the phenomena, not the principles, that are of interest. Geomorphologists are not concerned about testing the validity of physical or chemical principles, rather they are seeking the ways in which various physical and chemical mechanisms combine in nature to constitute distinctive processes that shape planetary landscapes. In some cases, the search for geomorphic processes is guided explicitly by a formal explanatory theory expressed in the language of physics or chemistry. In others, it occurs within an abductive framework where evidence on effects (often morphology or morphologic change) and background knowledge of physical and chemical principles are used to infer the causal mechanisms responsible for landform development and change. In process-oriented investigations, the causal phenomena, because they are believed to be governed by underlying physical or chemical principles, are assumed to be generalizable; therefore, the focus is on testing of generalized theoretical statements about the relationship between cause and effect.

Geohistorical investigations also involve a search for unobservable causal phenomena, in this case historical events that produced relict landscape features or deposits. Here again the search relies heavily on data. The search for geohistorical events is almost always cast within an abductive framework (Rhoads and Thorn 1993) because it is not possible to measure a historical event itself (because it has occurred in the past), only the effect of the event. When reasoning abductively, it is essential to construct a coherent web of evidence, each piece of which provides an independent element of support for a particular explanation, while at least some of the same data serve as disconfirmatory evidence for competing explanations.

The high degree of complexity, and thus the seeming uniqueness of a geohistorical event, makes it difficult to develop generalizations about the event as a type of phenomenon (Kitts 1974). Thus, the focus, at least initially, is on determining the character of the event, not on formulating generalizations about the relationship between the event as a type of phenomenon and the effects of this type of phenomenon. However, any geohistorical event can be viewed as a mix of environmental/historical contingencies and general processes governed by physical laws. Because an event consists of a multitude of interacting phenomena manifested at different scales, causal generalizations resulting from process-oriented research programs play an important role in reconstructing the character of a specific event. These physically or chemically based generalizations, by assigning an immanent character to an event, also provide the basis for classifying a seemingly unique event as a type. In cases where a type of event that at first appears to be unique, such as cataclysmic glacial outburst floods, is found to have other instances, the discovery of new instances is guided by generalized background knowledge about the various forms of evidence that an event of that *type* produces (e.g. Rudoy and Baker 1993).

CONCLUSION

This chapter has endeavored to illustrate six main themes with regard to the relationship between observation and theory in geomorphology:

1. That a naturalized philosophical perspective on science, in which accurate depiction of the scientific process takes precedence over prescription or proscription, provides a powerful conceptual tool for understanding the scientific character of contemporary geomorphology.
2. That the naturalized philosophical perspective characterizes scientific observation in a manner that is readily recognizable to practicing scientists, including geomorphologists.
3. That at least since the time of Gilbert and Davis, despite an implicit undercurrent of radical empiricism, geomorphologists have not practiced theory-neutral observation, but have pursued and employed theory-laden observational techniques.
4. That the character of observations in contemporary geomorphology is broadly consistent with naturalized philosophical notions of data or evidence, which despite their theory-ladenness, preserve objectivity of testing.
5. That geomorphology differs from the basic sciences (e.g. physics, chemistry) in that established principles from these sciences are viewed as sacrosanct, regardless of the outcome of empirical testing (i.e. geomorphology is not involved in testing the validity of principles developed in the basic sciences).
6. That it is the search for geomorphic phenomena, not a reliance on novel methodologies or theoretical principles, that distinguishes geomorphology from other sciences.

A quintessential point to emerge from this essay is that observation is theory-dependent, but that this dependence does not necessarily undermine its vital role as the policing agent in geomorphic inquiry. A corollary of this view is that geomorphology will be best served by explicit recognition of the symbiotic relation between fieldwork/experimentation and theorizing. Because geomorphology depends strongly on other sciences, particularly physics, chemistry, and biology, both for its observational techniques as well as for the formulation and justification of its explanatory theories, scientific progress in the discipline is largely a function of how well informed geomorphologists are about theoretical and technological developments in other areas of science. Similarly, this dependency suggests that attempts to explore geomorphology philosophically should at least consider philosophical discussions about other sciences. Seen in this light, it is not a lack of fieldwork that historically has acted as the primary constraint on the discipline's advance, but rather a reluctance to embed geomorphology directly within the theoretical and technological contexts of physics, chemistry, and biology - a reluctance that may have been overcome by a stronger tradition of philosophical debate among geomorphologists.

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