I wish that Richard Hartshorne were alive. He would know what to write in an essay on methodology—even one concerning geomorphological methodology! He had a strong, unshakeable and clear vision of the nature of geography, and what the proper methodological approaches ought to be. His disciplinary vision included the place of physical geography, and geomorphology in particular, within the scholarly framework of geography. Few would have dared to assail his pronouncements, and those that did would have been excoriated in reply. With one hand clutching a tattered volume of Hettner as a shield, Hartshorne would once again be driving geography’s methodological bus toward intellectual oblivion, pushing geomorphology as a vanguard. Perhaps some would dare get off. A very few jumped from a similar bus about half a century ago, keeping a remnant of geomorphology viable in American geography.

Geomorphology is the science concerned with landforms. Geomorphologists are concerned with the evolution of landforms, and their geometry, origins, and locations. Geomorphologists, like all scientists, really, are reductionist, at least in the sense that we do not treat the form of the entire earth as the entity for our research. Indeed, the titles of many of our subdisciplines indicate a first-order reduction of the unit of study: fluvial, tropical, glacial, arid, coastal, soils, and so forth. The unification of the subdisciplines is through the generic of landform, and through commonly accepted norms of methodology. With few exceptions, geomorphologists are trained either as geologists or as physical geographers, lending great methodological diversity to the discipline. My concern here is mainly with physical geographers, but the arguments are relevant for both schools.

Hartshorne (1939) viewed physical geography, with special attention to geomorphology, as a subdiscipline of geography (in literal and figurative senses), necessary only as its products relate directly to human geography. He did not think much of geomorphology as a part of geography, at least to the extent that the former is concerned with landforms as entities or with the origins (e.g., processes) of landforms. Indeed, he went so far as to denounce geographers who are geomorphologists for “the confusion that many of them have introduced into methodological thought in geography” (Hartshorne 1939: 423–24). Hartshorne was methodologically prescriptive in the sense that, following Longino (1990), he prescribed the proper methodology to be employed by geographers in meeting their disciplinary goals. His prescription for geographical geomorphologists required that landforms be studied only as a set of interrelated objects. Specifically, Hartshorne (1939:425–26) argues that “it is immediately clear that the interest of the geographer is not in the phenomena themselves, their origins and processes, but in the relations which they have to other geographic features.” Reduced to a logical minimum, this means that geographically oriented geomorphologists make maps. Many scholars have since subscribed to and defended versions of this
perspective, especially during geomorphology's disenfranchisement from geography through the middle of this century. This prescription results in a sterile discipline, however, one that I would rather not practice.

Sauer (1941) was one of the first to note some of the holes in Hartshorne's arguments concerning physical geography and relevant processes, especially their supposed derivation from Hettner. In reaction to Hartshorne's prescription, Sauer (1941:45) wrote that it is a fallacy to believe "that descriptive studies, done without regard to process, i.e., genesis and function, can add up to a science, either physical or social." Geomorphology without process was certainly not a position advocated by Hettner (e.g., Butzer 1989:37), and Hartshorne's representation of that view appears misleading. The perspectives described by Hartshorne and Sauer are the extremes of a methodological spectrum for modern geomorphology. At one end is geomorphological enquiry as the description of form and location, relative and absolute. At the other end is geomorphological enquiry as the understanding of space-independent processes and the resulting responses of the earth's surface. Between these extremes, the spectrum opens a place for each of us. Closing doors, à la Hartshorne's manifesto, just takes us backwards.

By undertaking this essay, I have accepted an opportunity offered by the associate editors of the Annals to discuss modern methodologies in geomorphology. In particular, I was charged to address three main questions. First, what are the key methodologies in geomorphology? Second, what are the practical implications of making certain methodological choices for the production of knowledge in geomorphology (how have methodologies influenced where we have been and where we are going)? And third, to what extent can methodological discussions enable us to achieve both an integrative and interdisciplinary perspective on physical geography and geography at large? The key methodological stances taken by past and present geomorphologists (e.g., positivism, realism, functionalism, idealism, and so on) have been reviewed or discussed at several levels of detail (e.g., Marston 1989; Bishop 1980; Schumm 1991; Tinkler 1985; Rhoads and Thorn 1996; Gregory 1985; Phillips and Renwick 1992; a thorough review is presented in Beckensale and Chorley 1991). Approaches to the second question must be inextricably bound to what will always be idio-
syncratic surveys of disciplinary history, producing Whiggish interpretations of cause, effect, and efficacy (discussed by Livingstone 1984; Bauer 1996; or Golinski 1998, for example). Central to the concept of Whig history is the interpretation of past developments as foundational to present positions. A fascinating example of this approach is Strahler's (1992) account of geomorphology at Columbia University in mid-century.

The third question poses an interesting springboard for discussion, but it implies that physical geography is somehow not already an integral part of geography. I cannot agree with this implication. Those that argue most urgently for this integration may be those that believe most strongly that a profound division exists, and they may act accordingly. I do not see a fundamental chasm—the differences between geomorphology and historical geography (for example) do not appear to be much greater than the differences between social theory and regional science approaches to geography.

Every geomorphologist applies a singular methodology in a particular circumstance. By "singular" (as opposed to "unique"), I mean a characteristic methodology that represents an integration of personal experience, perception, and ability. Most of us nourish more than that singular example—we maintain collections of methodologies, or sets of tools, that have been developed, deliberately or through the accident of experience, to enhance the probability of personal and/or professional success. These tools comprise our integrated approach to the practice of geomorphology. Practice is meant in two senses: the generic practice of our research agenda, and the specific practice associated with a research problem. We set, perhaps unconsciously, our research agenda through the types of problems that intrigue us (or reward us) to the extent that we decide to study them. Conversely, we may also establish an agenda by deciding what we do not want to do. In either case, we see landforms through eyes conditioned by that agenda, and vision sharpened by our methodological understanding.

The broadly cast research agenda then provides the general boundaries for what we choose to be our particular research problems. For example, a research agenda may be to understand and model flow regime sequences in fluvial systems, whereas a specific research problem might address flow separation in the...
lee of bedforms. Our methodological approaches to the study of landforms are not awkwardly grafted appendages on our disciplinary psyches. Rather they are subtly woven through our perceptions of landform systems. Sometimes methodologies require such concentrated devotion that the selection of one effectively precludes the development of others. Perhaps for those at the cutting edge of applying GIS methodologies in geomorphology, for example, the rapid expansion of methodological (technical) resources demands near-constant effort to remain current (e.g., Longley 1998). The requisite focus often leads to the atrophy of other methodologies, and the subsequent dimming of related visions. Similar effects may occur as one’s research interests change. The implications of choice are critical to understanding the evolution of geomorphology, but they always bring us back to the “here and now.” The broader issues of choice reflect deeply on what kind of a science community we comprise. Because of the underlying importance of methodology, it would be worthwhile to consider more closely how methodologies are chosen, as the process of choosing defines directly the realm of practical methodologies. But that is a different challenge—probably one for the psychologists.

I want to use this opportunity to begin a conversation about what methodology might really mean to the practice of geomorphology in the next millennium. This attempt demands a set of self-serving, preambulatory cautions. First, this review is personal, and I have not read all of the potentially relevant geomorphology, physical geography, geography, and philosophy literature. Second, I do not believe that there is only one correct approach to methodology in geomorphology. The purpose of these statements is not to trivialize the essay, but, I hope, to keep it in perspective. Hartshorne (1948:116) has argued that it is important to take methodological debate seriously and to apply “responsible scholarship” to methodological problems, and he was correct. All such debate, however, must stem from an individual perspective. The choice of a methodology is, after all, personal, based upon the intent and accidents of training, personality, opportunity, conviction, fashion, politics, and most likely, diverse combinations of these.

Defining Methodology

Wittgenstein (1972:4e) wrote that “Uttering a word is like striking a note on the keyboard of the imagination.” The word methodology, however, is much more like a chord, gracelessly played. The word is not very pretty—it sticks to the tongue and often makes the eyes roll back in dread. It is ambiguous in meaning and often conjures notions of stricture on our otherwise unconstrained intellects. In many disciplines, especially in the social and behavioral sciences, issues of methodology are frequently foci for intense debate. In geomorphology, however, it is frequently remarked that methodological complacency seems to be prevalent. Is this because geomorphologists like the chord, or because we have learned to ignore it?

Kaplan (1964:18) defines methodology as “the description, the explanation, and the justification—of methods, and not just the methods themselves.” Recognizing that the term is ambiguous, he identifies four common usages of methodology. He terms these techniques, honorifics, epistemology, and methods. By techniques, he refers to specialized sets of procedures used by a particular discipline. He offers the “techniques of carbon dating” as an example. Techniques are the accepted (often standardized) practices for doing scholarly work in a science. This usage is quite common in graduate theses and dissertations, where the grander term methodology is often substituted for the more mundane techniques (especially in chapter headings). Second, methodology is also used as an honorific. By this, Kaplan refers to a ritualistic incantation to the scientific method, usually as an introduction to a piece of research. The purpose of this usage is to enhance the value of the work through the implication that the rules of science have been recognized and obeyed. He notes (even in 1964) that this usage is much less common than it had been. Third, Kaplan (1964: 20) recognizes methodology as epistemology. This term, used mainly by philosophers, especially those concerned with the philosophy of science, implies that methodology is a means of seeking truth and gaining knowledge. Kaplan offers that appeals to induction, as a methodology for obtaining knowledge, is consistent with this usage. Discussions of “Real” geomorphology (Richards 1990; Bassett 1994; Rhoads 1994) are recent disciplinary examples of methodology as epistemology. Several of the essays in Rhoads
and Thorn’s (1996) *The Scientific Nature of Geomorphology* also touch specifically on this meaning (e.g., Brown 1996; Baker 1996a). The essay by Yatsu (1992) is especially entertaining and challenging in this context. Yatsu rails at several of the modern personal myths comprising Whig histories of geomorphology and their rather loose methodological stances.

Finally, Kaplan (1964: 23) recognizes *methodology* as signifying *methods*, “techniques sufficiently general to be common to all sciences, or to a significant part of them. Alternatively, they are logical or philosophical principles sufficiently specific to relate especially to science as distinguished from other human enterprises and interests.” In a hierarchical sense, this is a definition that falls between the *techniques* and *epistemology* usages. It is this meaning that is manifested as one basis for dividing geomorphologists into methodological camps, e.g., conceptual modelers versus simulation modelers. Here I spend very little time with the *techniques* and *honorific* usages of methodology, focusing more closely on the *epistemology/methods* border.

**Methodological Traditions**

Historians and sociologists of science devote considerable time and effort to the description and interpretation of methodologies. Certain communities of philosophers are concerned almost solely with the epistemological aspects of methodology. Their conversations relate coherently to our construction of geomorphology as a science that treats landforms, and they have produced a voluminous methodological literature. Three themes are identified for discussion here. These relate to the fundamental methodological stances embraced, implicitly and explicitly, by geomorphologists, and the potential impacts of methodological debate on the evolution of geomorphology as a scientific discipline. The first theme is Kaplan’s (1964) view of scientific autonomy, the second the methodological traditions of Lakatos (1978), and the third is a perspective on pragmatism as a methodological philosophy that has greatly influenced geomorphology in recent years (e.g., Baker 1996b).

Kaplan’s (1964) book, *The Conduct of Inquiry: Methodology for Behavioral Science* (an interesting forerunner to Harvey’s (1969) *Explanation in Geography*), makes several arguments relevant to this discussion. From the outset, Kaplan (1964:3) catches the imagination with his concept of scientific autonomy: “the principle that the pursuit of truth is accountable to nothing and no one not a part of that pursuit itself.” By this standard, the work of a scientist (e.g., a geomorphologist) is only rightly appraised by the judgments of other scientists (e.g., geomorphologists). This is a grand image, and boldly stated. The principle applies both to the individual and to the discipline—both are empowered by it. Kaplan develops the argument by noting that disciplines are minisocieties that develop implicit and explicit professional standards, a view echoed by Harvey (1969:62, for example). The standards are established and enforced by a disciplinary elite—e.g., journal editors, peer reviewers, association officers (this may be seen as one of the controlling roles of an “invisible college,” e.g., Crane 1972). The operation of such processes is implicit in our use of the word discipline, as it (according to Golinski 1998:69) “refers both to a form of instruction to which one submits and to a means of controlling behavior. The word has this dual significance deeply impacted in its etymology. It is worth recalling this original connection to pedagogical power . . .” This is a notion that we usually take for granted, but one that speaks again of the autonomy of science. The central tenets of the model presented by Kaplan have been described frequently, often with other terminology, and certainly many aspects of scientific autonomy are discernable in an examination of geomorphology as a discipline (aspects of this are discussed in Sherman [1996], as a fashion process). The argument appeals to our egos. The constitution of good geomorphology is the responsibility of geomorphologists and no one else. We oversee the development of the discipline, we set the standards, and we are the judges. It is our privilege and obligation to prescribe proper methodology, especially to our students. Enforcement of the standards, according to Kaplan (1964:5) will “on the whole and in the long run make for success.” I enjoy that feeling of making a difference, of having responsibility and acting with authority. Kaplan says that this is correct and defensible.

The work of Lakatos, however, has made me much less comfortable with Kaplan’s notion of a scientific autonomy. Much of Lakatos’s thinking centers on epistemology and philosophies of knowledge and science, and he includes niches for scientific autonomy and for pragmatism.
Several of his key themes can be traced through the practice of science, and are directly pertinent to appraisal of methodologies. Especially relevant is his description of the major traditions used to appraise scientific theories (Lakatos 1978): skepticism, demarcationism, and elitism. Within these three broadly drawn traditions is contained a gamut of “isms” that runs from radical empiricism through feminism and postmodernism and includes the philosophies mentioned in the introduction.

Skepticism is the (ancient) view (e.g., Empiricus, trans. 1933) that there is no absolute means to evaluate scientific theories—there is no absolute criterion of truth. The modern skeptical argument was formalized by Descartes (one of Popper’s [1972] “belief philosophers”). The methodological implications of this perspective have been revived in the cultural-relativism approaches espoused by modern sceptics such as Feyerabend (a longtime friendly rival to Lakatos—see Feyerabend 1978), who was a fierce critic of contemporary methodological positions. He decried not just that scientists believed in one methodology, but that, in fact, they could only pretend to believe: “Methodologists may point to the importance of falsifications—but they blithely use falsified theories . . . . In practice, they slavishly repeat the most recent pronouncements of the top dogs in physics, though in doing so they must violate some very basic rules of their trade” (Feyerabend 1978:65). Feyerabend’s anything-goes approach to methodology is reincarnated, in one sense, under the rubric of methodological postmodernism (e.g., Dear and Wassmansdorf 1993). There is a surficial appeal to this approach, as it appears to free the geomorphologist to attack a problem with any toolset available (e.g., Yatsu 1992; Butzer 1973). From a methodology as methods perspective, this is fine, but probably not efficient, as some toolsets will not work well under some circumstances (sure, we can drive screws with a hammer, but . . .). From an epistemological approach, this would indeed be the chaos that Feyerabend dreamed of.

The demarcationist view is that there are criteria (standards) by which better knowledge may be demarcated from worse knowledge (or science from nonscience). The demarcationist perspective derives from a belief that universal criteria exist in the “world 3” of ideas, where “world 1” and “world 2” comprise the physical phenomena of the natural world and human and social belief systems, respectively (Popper 1972; Lakatos 1978). The products of science (geomorphology) exist in world 3 with their evaluative criteria and methodological knowledge. Popper (1972) claims that this is also the domain of the contents of scientific literature, theoretical systems, problems, problem situations, and critical arguments. Logic is the keystone for validation (but not proof) in a process that is, arguably, open to the public. But although the public may potentially participate in the appraisal (Lakatos 1978:110, used the term lay jury), the judges are the philosophers of science. Lakatos subscribed to the demarcationist perspective as the only rational means of appraising research programs. Demarcationism is a camp that embraces most of the approaches that we would commonly recognize as one or another aspect of the “scientific method,” although we might disagree substantially about what that means in a geomorphological sense (e.g., Bassett 1994; Bauer 1996; Richards 1990; Rhoads 1994).

The third perspective is elitism. The elitist denies the existence of universal criteria for appraising the products of science. Instead, assessments are made on a case-by-case basis, and only scientists themselves can judge the quality of the science. In Lakatos’s (1978:111) words, “academic autonomy is sacrosanct and . . . the outsider must not dare to judge the scientific elite.” Elitism is a seductive stance. It empowers the practitioner of a discipline while dismissing the value of outside opinions. Lakatos (1978:113) notes that Descartes and Bacon (perhaps ironically, as we frequently recognize him as the “father” of the scientific method) were “the first two modern elitists.” The disciplinary community sets and enforces the standards, and establishes preferences. This is a formalized description of Kaplan’s scientific autonomy. It is not coincidental that Descartes has been tagged as both elitist and skeptic—both stances lead to the social organization of knowledge.

The elitist tradition is not intrinsically evil. Indeed, regardless of whether we approve of the concept, the tradition is played out in most academic disciplines, often for the good of science. Even so the strong undercurrents of authoritarianism inherent in the elitist stance pose a risk to intellectual freedom. In either of the nondemarcationist traditions there is a predilection, perhaps a requirement, for socially organized
control within disciplines. Because these traditions recognize no universals, judgments are typically rendered by an empowered peerage as opinions. The aims of the empowered need not have the advancement of science as the sole motive. Seemingly innocuous stances can be reduced ultimately to less benign positions. This can be exemplified through a consideration of pragmatism—another oft-noted methodological stance in geomorphology with close linkages to scientific autonomy and elitism.

Pragmatism is a philosophy of science based upon the belief that truth is converged upon through successive inquiry (e.g., Peirce 1877), and that there is no truth aside from its manifestation. This is a “world 2” view of truth, and thus, nominally, pragmatism and elitism are distinct. Lakatos (1978) has demonstrated, however, that the distinctions may be removed through two simple assumptions, and he suggests that most elitists will be pragmatists. Pragmatism is a belief philosophy, founded upon the doctrine of “critical common sense” (e.g., Peirce Edition Project 1998:433). It is “no attempt to determine any truth of things” (Peirce Edition Project 1998:400), but a method of searching for meaning, the resolution of which is determined by consensus. The relationship between pragmatism and elitism is critical in examining methodological prescriptions in geomorphology, especially to the extent that we consider our discipline to be egalitarian. Baker (1996b), for example, has detailed the influences of pragmatism on the development and practice of geomorphology, and he has prescribed it for our approaches to knowledge. Baker’s work, and related pieces by Fujiki (1987, cited in Yatsu 1992) and Yatsu (1992) leave no doubt that the tenets of several versions of pragmatism are wound through geomorphology. The pragmatic perspective offers one venue for disciplinary evolution, wherein an elite can direct developments without maintaining a strict view of objective truth (Sperber 1990).

Baker’s (1996b) review demonstrates that the pragmatic approach has offered much to geomorphology. It has been and can be an extremely useful perspective. There is a dark side to pragmatism, however, at least in its implementation. Because there is no objective truth, all decisions about knowledge are subjective, and therein lie concerns about the judges of science. When subjective understanding is the foundation for judging science, the decisions of individuals and scholarly communities may be corrupted. Lakatos (1978) notes the small conceptual distance separating pragmatism from elitism, and repeats cautionary arguments concerning the pitfalls of subjectively derived truth. Russell took a stronger position. He was extremely critical of pragmatic philosophy (although he directed his arguments more toward Dewey’s pragmatism than Peirce’s). Russell (1961:637) wrote, “The third attraction of pragmatism . . . is love of power.” And “if power is all you want from science, the pragmatist theory gives you just what you want, without accretions that to you seem irrelevant.” Although the Peircean vision of pragmatism was based on an appeal to common sense, he presumed the common sense of the scientist (or philosopher). Peirce (1878: 302) claimed that “I know that in the matter of ideas the public prefer the cheap and nasty.” Pragmatism was meant to be a democratic philosophy, but in academia, there are always eligibility tests for voting.

### Making Methodological Choices

The skeptic tells us that there are no standards. The demarcationist tells us that there are universal standards. The elitist tells us that there are social standards. The pragmatist tells us that standards are what we think they are. Many of us might not be sure what standards we apply; alternatively, we may find on reflection that our standards vary from occasion to occasion. Where is the firm methodological ground? Which tradition is followed in developing methodological foundations for geomorphology, and which tradition should be followed? It is difficult to accept the skeptical perspective that anything goes (although many geomorphologists seem to). Potentially, the resulting anarchy would exhaust the intellectual resources of the discipline. Imagine, for example, the in-print analogy of accepting papers for presentation at the American Geographers’ annual meeting—no judgment rendered. Journal editors would become watchdogs of spelling and grammar (and maybe not even these!), but they could not use judgments about the quality of science as a basis for decision making because there would be no defensible basis for their authority. Tribulations such as those experienced in 1994 by the journal Social Text would multiply, wherein faux papers (or, more likely,
just bad papers) are passed by editors unwilling (unable?) to appraise them critically, and they are published. Geomorphologists would share “a gathering storm of protest against the collapse in standards of scholarship and intellectual responsibility with which vast sectors of the humanities and social sciences are currently afflicted” (Boghossian 1999:273). Each article would need to be read carefully to determine its value in a given context. The burden of evaluating each piece of literature would fall to each of us. My conjecture is that something like reading rings (chat groups?) would evolve to deal with this challenge, and we would again be socially organized.

It is difficult to imagine all geomorphologists fiercely maintaining sets of independent (i.e., skeptical) approaches to landform analysis. This vision would presume that we are somehow immune to any human impulse to form communities. It is difficult to imagine us divorced from the working groups and specialty groups of professional organizations, where we are always touched by other opinions. It is much easier to imagine the formation of intellectual cliques and the subsequent development of frictions between cliques. This brings us back to elitism. The demarcationist perspective, on the other hand, demands the recognition of universal criteria for judging science. But we do not need to know what those criteria are. In fact, we are just required to believe or disbelieve their existence. Philosophers of science stand ready to do the judging for us. We are required to believe that the products of our knowledge may exist independently of the producers of that knowledge. This is the most noble (because of its invocation of universal truths) and intellectually challenging (because universal truths are the standards) of the three traditions. Popper (1974) places his demarcation proposal, roughened around the edges, at the center of his philosophy. But despite Popper’s disdain for belief philosophers, there remains the expectation that we will choose to believe in the existence of world 3. This leaves us to consider the elitist perspective for geomorphology.

At first glance, Kaplan’s description of the autonomy of science and Lakatos’s description of elitism have the appearance of being democratic to practitioners within the realms of a discipline. Only the “outsiders” are excluded from a meaningful position. As with the skeptical position, however, methodological allegiances must develop, and this must lead to tensions within a particular scientific community. I have argued elsewhere (Sherman 1996) that the discipline of geomorphology develops through a fashion process similar to that described by Sperber (1990). The fashion process is an extreme manifestation of the elitist tradition, wherein a few (the fashion leaders) exert substantial power over a discipline. Fashion leaders are selected based upon the popularity of their research agenda rather than the quality of their scholarship, and they, or their delegates, are the judges and jury for maintaining disciplinary standards. The fashion model also includes defending a disciplinary status quo against the attacks of outsiders. It differs from the broader elitist agenda in that the ultimate lines of defense are drawn around the fashion leaders rather than the discipline. The power structure of fashion is explicitly removed from the periphery of a discipline, and manifested at the core.

From the simple triad of Lakatos, we may make a choice about where we stand as geomorphologists, or as physical geographers, or geographers, or as humans. This choice poses the hypotheticals that should govern our methodological approaches. For example, if you believe that there are no absolute truths, then you might embrace either the skeptical or elitist tradition. You could also be a pragmatist (most of us are, really). If you believe in absolute, world 3 truth, then you must be a demarcationist, however you choose to manifest that tradition. Each of these traditions carries the rich subtext of “isms,” and every piece of our research, past, present, and future, could be tagged with one of them. If an impartial jury were to be found and charged with the task of performing this tagging, an appraisal of their results would indicate which methodology we practice. I think that most of us (at least those of us who care) would be surprised at the differences between what we believe and what we do. Even larger differences might characterize the gaps between what we do and what we teach. I would feign the hypothesis that despite the increased attention paid to theoretical, philosophical, and methodological issues in geomorphology, for the most part, it does not matter. We just do what we want to do in journey-like fashion. And this hypothesis should be fairly easy to falsify. Ask yourself and your colleagues about how they pose research questions and design experiments, either for the field or for the laboratory. Ask if there is careful
consideration of the methodological tradition that is to be applied. Ask if the research design would be different if another tradition were to be adopted. Ask yourself if you believe their answers. Twidale would be leery. He claimed (1983:55) “that the so-called methods are in reality haphazard, intuitive or even serendipitous. Moreover, when scientists have attempted to record the sequence of events leading to discovery, they describe what they think ought to have been done rather than what was indeed the case.” My opinion is that our practice is little, if at all, different from that described by Twidale, and that these discussions change little. History supports the latter contention.

There have been no revolutionary changes in geomorphology since Davis set loose his vision of the Geographical Cycle (Davis 1899). And this is absolutely not revolutionary in the sense of Kuhn (1970). The evolution of geomorphology, especially in a methodological context, has been a slow drift from fashion to fashion. The quantitative revolution and the dynamical/process revolution in geomorphology were not revolutionary, at least in the sense that they were not triumphant. Further, there is no apparent indication that any revolutionary work is presently afoot. There are no great debates taking place in geomorphology. For the most part, we have our heads down and we are focused on personal research programs. The methodological and philosophical quiescence we enjoy stems largely from our comfort with what we are doing. The larger issues are decided in physics and biology (for example), and we seem to be content to wait for the results to filter down so that we can apply them in our research programs.

In short, we do not have a paradigm acting as a disciplinary focus (e.g., Sherman 1996), and little has been done to resolve the internal sniping about methodology and practice that Butzer (1973) rebuked more than a quarter of a century ago, and that has been revisited by Yatsu (1992). We are hypocritical in making claims for the scientific method, or positivism, or realism, or constructivism as governors of our practice. Without a governing paradigm, there is no necessity for a governing methodology. We adopt the approach that best suits a specific problem. Unless geomorphologists as a class are different from other scientists, we do not do what we say we do. With perhaps a few exceptions (just in case the pure do exist!), we do what works for us in a manner that maximizes some return that we value. This sounds utilitarian, but it is realized largely at an individual level first, and at a social level (e.g., invisible college level) incidentally. We have chosen to be geomorphologists because we derive some personal benefit from the related activities (e.g., Sherman 1994).

Conclusions

I would like to be an elitist—presuming that the elite would have me—because if you are a keeper of the standards, it is relatively easy to maintain your seat on the bus. And, of course, it is a much better-sounding name than demarcationist. Philosophically, though, I cannot advocate the elitist or skeptical positions, or the closely related pragmatic approach. I believe that only judges from outside the discipline, e.g., philosophers of science, or even a lay public, could maintain the semblance of objectivity necessary for rational appraisal of our methodologies in geomorphology. But that presumes somehow that they would not actually care about what they found.

The philosophers of science do not seem to be watching us very closely right now, and vice versa. Geomorphology has developed, and continues to change, according to a fashion process only. This is a strong version of the elitist model, and it affects the way we all recognize, perceive, and react to research questions. It is inherent in the use of the word “discipline” to describe the community of geomorphologists. The term fashion sounds disreputable in the context of our science, and it suggests a shallowness of change, where new methodologies are embraced only as a façade. We can try to substitute paradigm for fashion; it certainly sounds more scientific. But paradigms are just terminologically wrapped fashions, after all.

The prescription of methodology by a practicing geomorphologist is both an audacity and a disfigurement. It is a trademark of the fashion leader. It requires a strong conviction that there is indeed one best way to approach the study of landforms. I offer, in contrast, no prescription here, preferring the anarchical vision of the skeptic. Intellectually, the demarcationist tradition provides the highest standards for scholarly ambitions and responsibilities. But neither describes my scientific methodology. It is disappointing to point toward fashion, or elitism, as
our governing methodological tradition, but it is what remains. And I am reluctant to prescribe that.

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References


“Dilettantism in Hydrology: Transition or Destiny?” (Klemeš 1986). In this article, Vit Klemeš, a respected engineering hydrologist with a reputation for insightful, if not incisive, critiques of the discipline, argued that hydrology has been slow to emerge as a science in its own right and instead has an identity “only as an appendage of hydraulic engineering, geography, geology, etc.” (1986:177S). Because of this, Klemeš claimed, “the perspectives of hydrologists tend to be heavily biased in the direction of their nonhydrologic primary disciplines, and their hydrologic backgrounds have wide gaps which breed a large variety of misconceptions” (1986:177S). “Hydrologists,” charges Klemeš, “do not seem to be able to break free from the grip of their primary disciplines . . . and, as a result, ‘. . . for hydrology as a whole, we are dilettantes who ‘toy with the subject or study it lightly’’” (1986:178S).

I believe Klemeš’s charge of dilettantism is especially relevant to this forum and the questions raised by Bauer, Veblen and Winkler because issues of methodology were at the crux of his critique. Using the metaphor of Bauer et al., dilettantism can be ascribed to those who are too comfortable in their favorite pair of disciplinary-based “methodological sneakers” to equip themselves for the “cross-training” needed to address the increasingly complex scientific questions that are emerging in hydrology. For example, Klemeš questions how much research interest “is really in the science of hydrology, in learning how it works,” as opposed to an interest in “elaborating some pet concept from one’s primary discipline which seems capable of performing a hydrologic trick” (1986:177S). These “pet concepts” are described in terms of favored mathematical tools, models, and other methods of analysis. Klemeš’s charge of dilettantism further implies that there may be a much deeper problem in