4 Fashion in Geomorphology

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ABSTRACT

Geomorphology is a philosophically sedate discipline. It might be argued that this philosophical and methodological quietude represents the progression of Kuhn's 'normal' science. However, this same torpor can be interpreted as indicative of a discipline waiting for a fashion leader. Sperber (1990) adopted the concept of fashion change from the design and arts disciplines to explain one means of controlling the developments and directions of science. He contends that changes in the goals, subjects, methods, philosophies, or practice of science can often be attributed to the emergence of an opinion (or fashion) leader, pointing toward a different path - setting out the new fashion. The fashion process relies upon fashion dudes to advance their disciplines (and their careers). This chapter examines the applicability of the fashion process to geomorphology through an evaluation of eight criteria established by Sperber as common to fashion-dominated disciplines. It is concluded that the fashion process is applicable to geomorphology, and examples of past leaders are presented. Some fashion futures, and their implications, are considered. Whether the fashion process has been a positive or negative force in geomorphology remains to be assessed, but its recognition as an agent of change is critical for a maturing discipline.

INTRODUCTION

> *Fortunately, practice never quite fulfills the expectations of prescription*
> (Leighly 1955, p. 317).

There are more geomorphologists alive today than at any time in the past. More geomorphological research is being conducted, more papers are being published and presented, and there is more general interest in the discipline than in any previous era. It is an exciting, challenging, and promising time for us. It is also a season appropriate for

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some disciplinary introspection - an inventory and appraisal. This is an activity that most of us can (and do) avoid for most of the time, but there are occasions when serious contemplation is called for. This is such an occasion. Recent debates concerning the place and status of geomorphology as a science betray a fundamental insecurity regarding the intellectual viability of our discipline. These debates also provide the opportunity to move away from the orthodoxy of tradition toward the establishment of a vibrant geomorphology; an opportunity that requires critical introspection.

The theme of this volume is 'the scientific basis of geomorphology', as designated by Bruce Rhoads and Colin Thorn to include consideration of the historical, methodological, and philosophical issues surrounding our discipline. This chapter will touch on aspects of all three of these realms as it reconsiders the developmental history of geomorphology through lenses ground with Sperber's (1990) concept of 'science as a fashion process'. The discussion is structured around five themes:

1. Pertinent aspects of the relationships between philosophy, science, and geomorphology;
2. Science as a social process;
3. The applicability of Kuhn's model of scientific revolutions to geomorphology;
4. The concept of fashion in science as described by Sperber (1990);
5. The propriety of a model of geomorphology as a discipline developing according to the fashion process.

It is not my purpose to start from first arguments and principles here. Instead I will rely on summary arguments directly related to the contention that geomorphology has been, and remains, a fashion science. The recognition that we, as a scientific community, are influenced by social forces is not new. But the manners through which fashion processes control our disciplinary development are worthy of exploration. Categorical denial of the power of the fashion process in shaping geomorphology is retrogressive.

There are several steps necessary to get to point 5, each of which involves a substantial literature. Examples of these literatures are cited, but the reader who is interested or skeptical will need to read beyond these pages. I have also relied to a substantial degree on direct quotation from pertinent sources. There are two reasons for this - first, I find paraphrasing tedious and unrewarding, with a nontrivial risk of misstating the original author's intent, and second, in most cases the thoughts of the respective authors are best presented in their own words. I have tried diligently to avoid divorcing these quotes from their context. Bryan (1950, p. 206) chided: 'Direct quotation and copious footnotes used merely to convince the reader of the scientific verity of statements should be avoided.' I have avoided footnotes entirely (unless Bryan was really referring to references), and this is not an effort to convince the reader of this piece about its scientific verity.

PHILOSOPHY, SCIENCE, AND GEOMORPHOLOGY

> *The reason for the neglect of philosophical issues by geomorphologists is unclear. Perhaps it reflects a basic sense of philosophical security*
>
> (Rhoads and Thorn 1994, p. 91).

Geomorphology is a philosophically sedate discipline. This is not to suggest that there are not philosophers among us - there are. And it is not to suggest that there are not critical philosophical underpinnings to our discipline - there are. It is to contend that most of us are psychologically unconcerned or academically unprepared to initiate or entertain philosophical debate about the nature of our science. It is to contend that this disconcern represents a surrender of disciplinary responsibility to a small cadre of opinion leaders, and a complacency to the fact that we are governed by trends, or fashions, rather than by logic or theory. It might be argued that philosophical training is superfluous to the education of a geomorphologist, or it is at least unimportant relative to more immediately germane subjects. Philosophical issues remain largely innate in our research, and mainly ignored, except in occasional explanatory retrospection. We can rationalize this fact by agreeing with the statement of Einstein (1936, p. 349) 'It has often been said, and certainly not without justification, that the man of science is a poor philosopher.' Indeed, why should geomorphologists, in their roles as scientists, engage in philosophical considerations anyway? Are we not able to avoid philosophical issues by saying 'not my job'? Is it not sufficient to do 'good science' and get on with it? The answers to these questions, and the motivations behind the questions, are central to an understanding of the internal and external processes that guide geomorphology. In a broader context, the importance of these questions (and some partial answers) was also underscored by Einstein (1936) when he continued his discussion to stress the necessity that physicists not leave the contemplation of the metaphysical foundations of their discipline to philosophers. The involvement of the scientist in disciplinary self-examination is especially critical during times of fundamental uncertainty or change, '… for, he himself knows best, and feels more surely where the shoe pinches' (Einstein, 1936, p. 349). Recent developments suggest that geomorphology may be entering (or perhaps continuing) a period of fundamental unrest. In fact the calling of this symposium seems highly diagnostic of such conditions. Few of us would admit to being part of a disciplinary proletariat that is chained to an intellectual hegemony forged by a few academic power brokers more than a half century ago. But most of us are. And most of us remain curiously silent concerning the scientific and social foundations of the discipline that we embrace.

The recognition that there tends to be a philosophical *laissez-faire* (at least) in geomorphology is not new. Most of us have some personal experience or views along this vein, and these attitudes are often noted in the literature (e.g. Chorley 1978; Schumm 1991; Rhoads and Thorn 1993; Bassett 1994; Sherman 1994). Ironically, of course, virtually every pronouncement on the paucity of philosophical discourse in geomorphology is prologue to such discourse. It is no different here, and it is in this spirit that I offer another perspective on where we stand. My interests, perhaps my temerity, in undertaking this chapter stem from the quasi-coincidences of reading Sperber (1990), Baker and Twidale (1991), Yatsu (1992), and Rhoads and Thorn (1993). This was coupled with teaching a graduate seminar with a prototypical postmodernist, Michael Dear (e.g. Dear and Wassmansdorf 1993), and the preparation of an essay on social relevance and geomorphology (Sherman 1994). These events made me aware of how much is taken for granted concerning the development of our science, especially differences between the explanations of how science should work and the histories of how science does work. This symposium affords an opportunity to offer an interpretation of how our discipline, its ideas, theories, methods, and philosophies, appears to evolve, rather than how it is supposed to behave.

Wittgenstein (1921, 4.111) wrote: 'Philosophy is not one of the natural sciences. (The word "philosophy" must mean something whose place is above or below the natural sciences, not beside them)' and (4.113 and 4.14): 'Philosophy sets limits to the much disputed sphere of natural science. It must set limits to what can be thought; and, in doing so, to what cannot be thought.' If we allow, *prima facie*, that geomorphology is a science and that Wittgenstein was correct, then we are obligated to treat our discipline as a core of science sandwiched by philosophies that must condition our practices. Speculation concerning such science (the task essayed here) must be tied to a speculation concerning the philosophy of that science. We can analogize the Kantian metaphysical dyad of ontology and epistemology (wherein the latter must flow from the former) to represent the bread of our sandwich. The upper crust, ontology, conditions our broader beliefs in the organizational structure that governs the behavior of geomorphological systems. The lower crust, epistemology, conditions our rules for obtaining, evaluating, and accepting knowledge concerning the behavior of geomorphological systems. In reality, of course, our sandwich is crushed to an extent that although we can still recognize a 'top' and 'bottom', the filling leaks here and there, and there is substantial ambiguity about where the metaphysical bread and the scientific filling meet.

It is apparent that although most of us are quite content with the filling of our geomorphological sandwich, our diets are not complete without the bread. Further, a well-constructed sandwich keeps our hands clean and enhances the taste (thus our enjoyment) of the filling. Just as this metaphorical sandwich represents an entity, we must also recognize that science, philosophy, and landforms must be conceptually costructured to represent the entity of geomorphology. The distinctions between science and philosophy must be recognized as the organizational (disciplinary) conveniences that they are meant to represent. It is critical to recognize that science and philosophy (just like our sandwich) are human endeavors; created, shaped, and practiced by humans. Past, present, and future philosophies and sciences must be considered through the sociologies that produced them, use them, or desire to change them. It is how we think about and use science and philosophy that is important (Longino 1990). If the subjective foundations of philosophy and science are accepted, then a stage is prepared for the interpretation of scientific development as a social process. And, if we are to consider the development of the discipline of geomorphology as occurring in some manner other than that of the orderly, sterile (mythical?) scientific method, then we must search for, and appraise, alternative models. There are many such models to choose from (including the model of no models!) but only the models of Kuhn (1970) and Sperber (1990) are contrasted here because they seem best to describe the practices and developments of geomorphology. And this comparison first requires some support from a discussion of social influences and controls on science.

SCIENCE AS A SOCIAL PROCESS

> *... authoritarianism, heroism and idolatry are deeply buried in the mental substrata of geomorphologists' communities which strictly prohibit libertarian activities in geomorphology*
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> (Yatsu 1992, p. 115).

Science is a social process. It depends upon contracts, contacts, and communication. Its practice depends upon funding, publication, and communication. It is organized into disciplines, with boundaries defined and drawn through tradition or argument. And within these disciplines we have the 'invisible colleges' where like-minded scientists group together to attack particular problems (Crane 1969, 1972). According to Price and Beaver (1966, p. 10 11) 'The basic phenomenon seems to be that in each of the more actively pursued and highly competitive specialties in the sciences there seems to exist an "in group" . . . Since they constitute a power group . . . they might . . . control the administration of research funds and laboratory space. They may also control personal prestige and the fate of new scientific ideas.. . ' Coinage of the term 'invisible college' is credited to the origins of the Royal Society of London (Price and Beaver 1966). We can recognize that there are invisible colleges, of different degrees of organization and formality, in geomorphology. These groups respond to external and internal social and political pressures and also exert social pressures on their nonmembers. Invisible colleges work as part of the selection process for symposia such as this one. Invisible colleges determine who is funded, who is published, who is in, and who is out. J. T. Jutson, for example, was judged 'out' by his contemporaries (according to Brock and Twidale 1984), and is, therefore, near-forgotten by his discipline. And Davis was judged to be 'in'.

There is nothing inherently wrong with the practice of science being a social process. There is still a requirement that the quality of science be judged according to agreed-upon rules of evidence. Longino (1990) compared the so-called objectivity of science with similar standards of appraisal in the arts and in philosophy. She wrote (Longino 1990, pp. 74-75): 'Objectivity ... is a characteristic of a community's practice of science rather than of an individual's... ' and 'Scientific knowledge is, therefore, social knowledge. It is produced by processes that are intrinsically social, and once a theory, hypothesis, or set of data has been accepted by a community, it becomes a public resource.' Feyerabend (1975, p. 309) was, as we might expect, a little more blunt in this regard: 'Science itself uses the method of ballot, discussion, vote, though without a clear grasp of its mechanism, and in a heavily biased way.' But a key requirement is that the community somehow agrees on standards for the appraisal of work, that there is some method at least to decide which issues are on Feyerabend's ballot. We must also recognize that the community tacitly allows power (responsibility) to concentrate into the hands of a few. According to the findings of Price and Beaver (1966, p. 10 17) ' . . . the research front is dominated by a small core of active workers and a large and weak transient population of the collaborators . . . '.

The manner in which these standards are set, and the criteria that they embody, must reflect the central concepts held by that community, or at least those of the invisible college claiming proprietary rights. Latour and Woolgar (1979) have likened this process to a rhetorical contest, where the audience is most likely to be persuaded by the scientist(s) bringing the greatest resources to bear on the problem. They include the mustering of references as one means of fighting this battle. Certainly, that represents a principal methodological tool in chapters like this one! But the purposes motivating the struggle, providing the impetus for struggle, are often omitted from the literature describing the sociologies of science. A recent review of this literature (Golinski 1990) reveals very little about what motivates scientists to adopt a particular position, or why they might refuse to adopt other positions. It is the issue of motivation, however, that must underlie the explanations of human agencies for disciplinary change. Hull (1988) is bold enough to write that science only works to the degree that it does because scientists are motivated by, and receive, individual credit for their product - knowledge. Because crediting is a social process, and because the judgment of credit due is a public process, we then have a portrait of scientists moved to please the social circle represented by their invisible colleges.

In the pre-Kuhnian era, it was commonly believed, or at least commonly pretended, that science was practiced according to some objective (e.g. Baconian) standards. Logical positivism provided the most common philosophical stance, at least for those that were trying to pay attention. It was sciencey, it was safe, it was acceptable, and it represented dispassionate objectivity. The scientist was emotionless, clean, fair, and motivated by a love of truth and the desire to improve the human condition. Kuhn suggested otherwise, with arguments founded on the subjectively derived rules developed by scientific communities. There is now a large literature deconstructing the practice and aims of science (e.g. Miller 1987; Hull 1988; Chalmers 1990; or Wolpert 1992). Many historians, sociologists, and philosophers concerned with science now subscribe to the notion of science as a relativistic (subjective) process. Although this perspective is popular, it is not universal. Popper, for example, remained unconvinced by much of this. As reported in an interview by Horgan (1992, p. 42), 'Popper is repelled by the currently popular view that science is driven more by politics and social custom than by a rational pursuit for truth. He blames this attitude on a plot by social scientists . . . '. There is not even agreement on how to disagree!

The attack on rationalism reached new levels with the publication of Feyerabend's (1975) *Against Method.* Feyerabend tried to apply concepts of political revolution and anarchism to the pursuit of science, claiming (Feyerabend 1975, p. 23) 'The only principle that does not inhibit progress is: *anything goes.'* It is readily apparent that the invisible colleges, or other forms of social organizations within scientific communities, do not, and cannot, allow anything to go. Although the argument that anything goes can be used to substantiate any particular stance embraced by the leadership of an invisible college, this posture might also rob them of authority and eliminate many of their gatekeeper functions (this may, in fact, pose one of the fundamental dilemmas confronting postmodernists how can they denigrate positivism while advocating a methodological postmodernism?). We must explore models that do describe how change can be accomplished for the development of scientific disciplines. For nearly three decades, the paradigm for scientific development and the evolution of disciplines has been Thomas Kuhn's vision of 'scientific revolution'. Of interest here is the frequent application of Kuhn's model, implicitly or explicitly, to describe progress in geomorphology. If it is correct to propose a geomorphology governed by fashion processes as described by Sperber, then the viability of Kuhn's model must be considered first.

KUHN'S MODEL OF SCIENTIFIC REVOLUTION

... Kuhn tried to say something about the way science is created...

(Thorn 1988, p. 20).

Any modern attempt to explain disciplinary change must at least consider the applicability of Kuhn's model for scientific revolutions. His model is founded first upon the concept that a discipline follows the tenets of 'normal science'. According to Kuhn (1970, p. 10), '. . . normal science means research firmly based upon one or more past scientific achievement, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice'. Normal science is characterized by the establishment of paradigms. Although Kuhn has been criticized for his relatively loose use of this term (e.g. Shapere 1964), he sets forth a basic definition - 'A paradigm is what members of an academic community share, *and*, conversely, a scientific community consists of men who share a paradigm' (Kuhn 1970, p. 176). This is pretty vague, but Kuhn (1970, p. 182) clarifies the term as being equivalent to signifying "disciplinary matrix": "disciplinary" because it refers to the common possession of the practitioners of a particular discipline; "matrix" because it is composed of ordered elements of various sorts, each requiring further specification'. Normal science progresses under the constraint and opportunities presented by a discipline's paradigm. During this progression there will arise 'anomalies', or instances where nature has somehow violated the paradigm-induced expectations that govern normal science' (Kuhn 1970, pp. 52-53). The scientific community will attempt to adjust their paradigm to accommodate the anomaly. However, where accommodations cannot be made (or forced), and where the anomaly (or set of anomalies) is of sufficient importance that it cannot be ignored, there must be a paradigm change. The new paradigm should be capable of assimilating the facts of the old paradigm and its anomalies while allowing considerable freedom for additional discovery.

Kuhn's concept of paradigm change and scientific revolution was directed to the development of mature disciplines, and geomorphology may not fit this description. However, geomorphologists have not been shy in their attempts to interpret the development of geomorphology from a Kuhnian perspective. For example, it is commonly accepted that the paradigm governing the discipline through the early decades of the twentieth century was the Davisian model of the geographical cycle (e.g. James 1972, p. 350; Sack 1992; Strahler 1992; Rhoads and Thorn 1993, among many others). Certainly this was the first general landscape model to receive widespread, international acceptance (e.g. Chorley et al. 1973; Beckensale 1976; Tinkler 1985) and it provided the touchstone for geomorphological development and debate for decades. It is also commonly accepted that this paradigm was rejected sometime around the middle of the century and replaced by process (or quantitative/dynamic/systematic) geomorphology (e.g. Vitek and Ritter 1989; Sack 1992; Strahler 1992). This is a North American perspective, as other regions embraced, at least temporarily, what Beckensale and Chorley (1991) have categorized as 'historical geomorphology' and 'regional geomorphology'. The latter was defined to include morphoclimatic geomorphology. Others can and do argue that there are other, mainstream, paradigms adopted by geomorphologists. In the modern era we can most easily see paradigmatic regimes categorized as 'scientific' (i.e. nomothetic or process-based) or 'nonscientific' (i.e. idiographic or historical), as discussed by Jennings (1973), Chorley (1978), Baker and Twidale (1991), Schumm (1991), or Yatsu (1992), among many. There is relatively little 'crossover' between the geomorphological communities practicing in these divergent camps, and there may occur substantial tension between these, and other, camps as each makes proprietary claims on the discipline. We see aspect of this type of struggle in the continuing efforts of geologists to disenfranchise geomorphologists trained in the process-oriented geographical tradition (e.g. Baker and Twidale 1991). Bauer (Chapter 16, this volume) addresses this issue in a carefully considered essay. This is a disciplinary power struggle in the most basic sense, clothed in a guise of intellectualism. It could be considered to be a paradigmatic struggle according to the Kuhnian model, if that model actually described the way geomorphology works.

THE FAILURE OF THE KUHNIAN MODEL IN GEOMORPHOLOGY

> *It now seems certain that the philosophical vacuum created by the disenchantment with Davisian geomorphology was not replaced by another general theory of landscape development*
>
> (Ritter 1988, p. 165).

Kuhn's model of scientific revolution through paradigm failure cannot be applied to geomorphology as a coherent discipline, and an alternative explanation for disciplinary evolution must be sought. My argument to support this contention is relatively simple and is posed through a consideration of geomorphology's prototypical paradigm, Davis's geographic cycle, under the light of Kuhn's protocol. The most rational conclusion that can be obtained is that the geographical cycle was not a paradigm and geomorphology is not a 'normal science'.

We must recognize that geomorphology did not exist as a distinct intellectual enterprise prior to the latter half of the nineteenth century (if it has even attained such a status today). What we now recognize as the roots of our discipline were grounded in physics, chemistry, theology, engineering, geography, geology, and medicine, among other disciplines (Chorley et al. 1964; or Bauer, Chapter 16 this volume). When Davis began the proclamations of his model toward the end of the nineteenth century (e.g. Davis 1889), geomorphology had no paradigms that would distinguish it from other disciplines. It had no foundational claims for recognition. Therefore it could not have been a 'normal science'. In Chapter 11 of his book, *The Route to Normal Science*, Kuhn (1970) notes that normal science is usually preceded by a period of fact-collecting. Preparadigms arise through the efforts to organize these facts and to build theories to accommodate them. Without a paradigmatic context, it is difficult to evaluate the worth of facts, and it is difficult to prescribe a particular research agenda to seek more facts. Therefore preparadigms are usually founded upon existing facts or those easily obtained. 'The resulting pool of facts contains those accessible to casual observations. . . ' (Kuhn 1970, p. 15). The development of the model of the geographical cycle was perforce predicated upon explaining just such a pool of facts. Further, in the absence of a belief system by which to address the pool of facts, such a system ' . . must be externally supplied, perhaps by a current metaphysic, by another science, or by personal or historical accident' (Kuhn 1970, p. 17). We can presume that Davis's body of belief derived from geology and biology. We see in his theory a preparadigm that comprises Chamberlin's notions of 'old' and 'new' valleys in the Driftless Area (according to Peattie, 1950) and Darwinian evolution. Thus Davis derived a theoretical framework for explanation of landscape evolution that accommodated the facts at hand rather neatly. Divergent views, or alternative preparadigms (and there were several), were largely ignored or swept away as the geographical cycle and its champion won new adherents.

The new discipline of geomorphology was dominated by the Davisian theory, and in that sense the theory might be considered a paradigm according to Kuhn's definitions. Indeed, the acceptance of the model was so pervasive that the geomorphological community might have earned the application of Kuhn's (1970, p. 22) words, 'They had . . . achieved a paradigm that proved able to guide the whole group's research. Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field a science.' Of course we do have the advantage of hindsight with which to judge the success of the geographical cycle as a paradigm for geomorphology.

The key evidence against the Davisian model as a paradigm for geomorphology, and thus geomorphology as a normal science, is based on Kuhn's discussion of *The Response to Crisis* (his Chapter VIII). There are two key statements in this chapter that underpin the argument made here. First, Kuhn (1970, p. 77) states that '. . . once it has achieved the status of a paradigm, a scientific theory is declared invalid only if an alternative candidate is available to take its place'. Second, he continues (on that same page) with a second circumstance of scientific revolution, 'The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature *and* with each other.' Note especially Kuhn's use of 'only if' in the first statement, and 'always' in his second. The contentious discussions over the content and practice of geomorphology, continuing to this symposium, are ample evidence that our discipline is not presently united by a paradigm, or even a set of paradigms. Nor has it been for more than half a century (e.g. Mosley and Zimpfer 1976). And certainly no serious student of the recent history of geomorphology would argue that the geographical cycle was refuted, in the manner required by Kuhn, in favor of an alternative paradigm. St Onge (1981) complained in fact that *vis-a-vis* a general theory (paradigm), geomorphology was in a conceptual vacuum.

There were several contemporary challenges to the viability of the geographical cycle, but these had minimal impact (e.g. discussion by Bishop 1980; or Tinkler 1985). Penck's (1924) alternative slope model, perhaps the most serious competitor, would not qualify as the alternative paradigm. Although its worldview was quite different from that of Davis, Penck's model did not receive the acceptance necessary to reach paradigm status. Through the middle part of this century, many geomorphologists were content to discard much or all of both models and muddle along by retreating to the safer havens offered by the paradigms of geology or geography. For example, at the Association of American Geographers Symposium on Penck's Contribution to Geomorphology, published in volume 30 of the *Annals,* little credence was paid to the viability of Penck's model, especially as a successor to the Davisian. John Leighly (1940, p. 225) notes that 'The great forward step beyond Davis that Penck took is not to be comprehended as a system that replaces even a part of Davis's work. . .'. In that same volume, Douglas Johnson (1940, p. 23 1) called Penck's slope model '. . . one of the most fantastic errors ever introduced into geomorphology'. And Kirk Bryan (1940) approved of neither approach as a general theory.

A decade after the Penck Symposium, the Association of American Geographers held a symposium to commemorate the 100th anniversary of Davis's birth, again marked by a special issue of the *Annals.* Once more, investigation of that literature fails to reveal a scientific revolution. O.D. von Engeln (1950, p. 177) notes that 'The geographers, though not disowning Davis, on the other hand, have more and more abandoned his ways and approaches.' Baulig (1950, p. 195) concludes his essay with 'Whatever success may attend the study of processes, the interpretation of forms will remain the ultimate aim of geomorphology. To this end, the Davisian method has not, thus far, been superseded.' Even the quantitative slope studies of the young lion, Strahler, could not produce anomalous results. In summarizing his own results, Strahler (1950, p. 212) allows that 'This suggests strongly that slopes recline in angle with time, unless constantly refreshed by stream corrosion. Davis's concept of reclining slopes in the erosion cycle may thus to some extent be confirmed.' Gregory (1985) discusses extensions, alternatives, and additions to the Davisian model, but not substitutes. There are many other accounts to confirm that the geographical cycle was never refuted, but rather ' . . . many of Davis's ideas simply hang in suspended animation; they are largely in disfavor, but have never actually been addressed comprehensively, let alone disproven' (Thorn 1988, p. 132).

So if geomorphology has any claim to disciplinary status, we are stuck when we try to apply Kuhn's model. In appraising our present situation we are left to accept one of two options: (1) to be a normal science we need a paradigm. Davis's model has been purported to be such a paradigm. This paradigm has not been simultaneously replaced in a scientific revolution. Therefore modem geomorphology would still operate under the aegis of the geographical cycle as a marcescent paradigm. The only rational alternative is that (2) geomorphology does not fit the model for normal science. From this stance, it can be argued that geomorphology has never been governed by a distinct paradigm, although it has been influenced substantially by the elbows of geological and geographical paradigms. Davis's model, and the general models of Penck (1924) or King (1953), and the general methodologies of Quaternary, process, and climatic geomorphologies (among others) all represent competing preparadigms. This option is reinforced by the frequent mention of the geomorphologists who were contemporaries of Davis but who did not accept the general or specific applicability of the geographical cycle (see discussions in Chorley et al. 1973). Sack (1992, p. 254) noted that there were several ' ... investigators who worked outside the dominant geomorphic paradigm during the Davisian era...'. According to Kuhn's definition of paradigm, quoted above, these investigators could not have been members of the geomorphological community if they did not accept the geomorphological paradigm. This conclusion is unambiguous. If we accept the paradigmatic concept for geomorphology, we must then argue that Gilbert and Bryan and Salisbury, and many other scientists pursuing similar research agenda, were not geomorphologists. This is an unreasonable corollary.

It is more reasonable, and certainly more satisfying intellectually, to conclude that the second option is the more viable explanation. The postmodern scramble of competing worldviews may represent nothing more than preparadigms struggling for domination. It is to be expected and encouraged as diagnostic of a healthy and vigorous discipline, but it should not be accepted without a critical awareness of what the associated social processes are. This perspective also leaves us without Kuhn's model for scientific revolution as a framework for describing the development of geomorphology. There are other models that might be applicable to fill this void. But the purpose here is to assess geomorphology in the context of Sperber's model for disciplinary development as a result of fashion processes.

THE FASHION MODEL

The game of science is, in principle, without end

(Popper 1934, p. 53).

Irwin Sperber (1990) attempted to explain episodes of irregular or irrational behavior in the scientific community by explicitly acknowledging the importance of a discipline's social structure and the vulnerability of that structure to the play of fashion. This approach grows out of the general body of literature produced by historians, sociologists, and philosophers of science that recognizes the importance of social controls on the development of disciplines (e.g. Livingstone 1984). Sperber acknowledges more directly the influences of Georg Simmel and Alfred Kroeber, social scientists who published some of the initial explorations of the interplay of fashion with modem society. In his book, Sperber addresses a fundamental question: 'How do the rules of Popper's game of science change?' His argument is based on the notion that the fashion process is pervasive but unacknowledged (even denied) in the scientific community, that the process is a dominant factor in the development of many scholarly disciplines. The fashion process operates in a scientific community to cause '. . . styles or models of scientific thought {to} rise to and fall from prominence just as shifting hemlines and chrome fenders. . .' (Sperber 1990, p. ix). He defines the fashion process as

> . . . a form of collective behavior marked by a series of normative preoccupations: keeping in step with the times, with the latest of developments; following the examples of prestigious opinion leaders who 'keep their ears to the ground' and articulate the shared and implicit sentiments of the public; admiring proposals for adoption when they are in good taste and new, discarding them when they are in bad taste and old; dismissing the weight of tradition while rediscovering and repackaging old proposals as though they were unprecedented, exciting, and modern; ignoring or downgrading explicit criteria by which competing proposals can be evaluated (Sperber 1990, p. x).

Several aspects of this definition appear immediately pertinent to the discipline of geomorphology as it constitutes a scientific community as defined by Sperber (1990, p. 6): '. . . any discipline, profession, institution, or network of organizations regarded as both responsible for producing and conveying ideas about a given order of social or physical phenomena and the source of those particular ideas defined as *authentic*, *certified*, and *valid* descriptions or explanations of such phenomenon'. Sperber claims that the adoption of various modes of scientific inquiry and the recognition of disciplinary leaders both replicate the processes associated with the rise and fall of ideas and leaders in the fashion and design industries. The disciplinary leaders are also variously described as fashion leaders, fashion dudes, or opinion leaders. His formal definition (Sperber 1990, p. 6) '. . . is any scholar whose work is regarded as highly influential and prestigious by his or her peers, reasonably representative of the most important and best research in a given area of study, and generally successful in "staying on top" (at least in the short run) despite the entry of rival designers into the selection process'. He then argues vigorously that the fashion process is an apt model for the behavior of many scientific communities. His particular concern is with the progressions of social science, but his model can be, and should be, held up against the natural sciences for appraisal because 'the delights of nature and the play of fashion in science seem to be "made" for each other' (Sperber 1990, p. 249).

The fashion process plays to the socially acceptable side of a scientific community, especially that of its leadership, a side that longs for status, popularity, conformity, and stability. The fashion process also degrades the importance of logical progress in the development of knowledge unless that mode of the development is in vogue. Most importantly, the fashion process recognizes that scientists are people, and that science is a social activity, and that any social community will have leaders. Even though the term 'fashion process' may have negative connotations for many of us, it is not intrinsically a bad thing. The value of the fashion process depends upon the selection and action of particular opinion leaders. The operation of the fashion process in a discipline may either allow - indeed encourage - the adoption of radical new approaches, or it may provide the appearance of change through a continuous reshuffle of existing fashions while minimizing true change. The process can be used as a funnel, a sledgehammer, a door, or a window. The recognition of the fashion process by a disciplinary mainstream, however, does provide a mechanism to maximize the potential benefits by increasing the participation in decision-making.

Throughout his book, Sperber posits his arguments as direct refutation of several other models of scientific development, especially Kuhn's descriptions of revolutions as the means by which science progresses. Indeed Kuhn, according to Sperber (e.g. Sperber 1990, pp. 209-210), was also involved as part of a fashion process, and he became a fashion leader because his work spoke to the desires of both his academic and the popular community. The logical, scholarly components of his work were of less direct impact in fostering an acceptance of his model, than was his timely critique of the way science works. The approaches of Kuhn and Sperber are not entirely contradictory, as they both rely upon a recognition of the critical roles played by social processes in directing scientific communities. Their models differ instead through the treatment of the motivations in the communities that result in change. Kuhn's model suggests that scientific communities change their paradigms when they cannot avoid doing so (this is a very simplified statement). Sperber's model suggests that scientific communities change their focus when it becomes fashionable, through any number of reasons.

It was argued above that the Kuhnian model is not generically applicable to geomorphology as a scientific community. It is now argued that Sperber's fashion model is applicable to, and important for, the development of geomorphology. We must recognize and embrace the importance of collective behavior in setting the geomorphological agenda. If we acknowledge the influences of the fashion process and the roles of opinion leaders in directing our discipline, we appropriate the ability to control the process in a critical and beneficial manner. This will, in turn, empower our community in the development of a vibrant and progressive science. Denial of the fashion process leaves our community vulnerable to the repressive aspects of fashion.

GEOMORPHOLOGY AND THE FASHION MODEL

> *Geomorphic fashions have swung from the qualitative statements of early observers to the highly mathematical treatments. . . and back again to inductive studies derived from field measurement*
>
> (Carson and Kirkby, 1972).

The term 'fashion' can be found frequently in the literature of geomorphology. It is usually employed in a manner that is at least mildly derogatory. Use of the word in a discipline's literature, however, does not provide a sufficient basis to conclude that the fashion model is operative. A more rigorous standard must be applicable. To meet this need, Sperber has defined eight criteria that are common to (and indicative of) scientific communities governed (at least partly) by fashion processes. An appreciation and evaluation of each criterion are fundamental to any attempt to link the fashion process to a science, and every criterion must be satisfied in order to conclude that a discipline is governed by the play of fashion. This is desirable information because the recognition of the potential roles of fashion in the development of a scientific discipline, and a subsequent acknowledgement of their importance, reflects a laudatory professional self-awareness. Sperber claims that this appraisal can lead to an objective decision concerning the discipline's status *vis-a-vis* the fashion process. The intent in this section of the chapter is to assess the applicability of each of these criteria to geomorphology in the context of the discipline's development and present status. If a relevant example can be described for each criterion (and I have tried to provide multiple examples for each), then we may conclude that the fashion process represents, at a minimum, a suitable hypothesis for exploring the way the discipline evolves. The criteria, as presented here, are paraphrased and abridged from Sperber's original.

Criterion 1

The most recognized research of a discipline's leadership may be seriously flawed in logic, evidence, or conclusions.

Can such an example be identified for geomorphology? It is often difficult to point to an individual publication as representing a scientist's 'most recognized work'. It must also be a subjective appraisal, and therefore open to debate. To simplify (but not necessarily relax) Sperber's criterion, it will be defined here as representing an oft cited or influential, substantive piece of work by a recognized leader (or leaders) of the community of geomorphologists. And from this basis many examples can be found.

It is prudent to avoid the litany of now humorous explanations of landform development that were offered prior to the twentieth century. The reader is directed to accounts in Chorley et al. (1964), Davies (1969), or Tinkler (1985) for compendia of examples. But there remain several notable examples from more recent literature. First and foremost can be offered Davis's suite of publications on the geographical cycle.

There is little debate that the major, internal aspects of Davis's model are logically consistent. Indeed Bishop (1980) has made a credible argument that Davis constructed a system that was internally consistent to the degree that it is not falsifiable in Popper's sense. Because his system was deductively contrived (e.g. Thorn 1988), such internal consistency should not be surprising. This reflects nothing more than the inherent emptiness of deduction (e.g. Reichenbach 1951). However, logical consistency does not necessarily imply scientific credibility. Deduction based upon faulty premises must lead to faulty conclusions. Bishop (1980) concludes that Davis's model probably is not a scientific theory because it cannot be falsified. One might argue that this conclusion is valid only to the extent that Popper's view of science is valid. However, it may be more powerfully argued that the Davisian model is not scientific because, as posed by Davis, it cannot lead to new knowledge. Therefore, because the prototypical geomorphological theory (albeit a theory in current disrepute) is not scientific, geomorphology, at least near the turn of this century, satisfies Sperber's first criterion. But the efforts of Davis do not stand alone. Lustig (1967, p. 5), discussing desert geomorphology, states ' . . . much of the literature . . . as well as current research effort, is inextricably enmeshed in basic concepts of debatable validity. . .'. And our arid lands colleagues are not unusual in being faced with this situation.

The rate of production of illogical, nonscientific literature by leaders of the community of geomorphologists seems to be accelerating. To mention a few examples, and without detailing particular correspondence relative to criterion 1, we can consider the subset of fashionist propaganda embodied in the prescriptive lists of concepts, canons, principles, error or propositions, and rules, constraints or cautions, as published by Strahler (1952), King (1953), Thornbury (1954), Chorley (1978), Schumm (1985, 1991), Scheidegger (1987), Brunsden (1990), and Baker and Twidale (1991). These targets are easy because many of these authors making fashion statements were writing in essay modes, and it is important to note that they were not necessarily concerned with logic, evidence, science, or quality of conclusions. However, all of these examples represent larger bodies of research and the writings are diagnostic of the geomorphological fashions embraced by their respective authors. The authors making fashion statements must be held to high standards of accountability because their works (beliefs) are expounded at conferences, appear in journals and books aimed at broad audiences, and their publications all address the metaphysics, methods, or goals of geomorphology. All of these publications aim to control the practice of geomorphology through the explication of beliefs concerning what geomorphology does or what it should look like. Because few of these publications are based upon logical consideration of all pertinent evidence, they must be held as evidence validating criterion I for geomorphology. For the reader concerned with more substantive examples of persistent error impacting conclusions, the case of frequent and continuing misused regression analyses of velocity profiles (discussed in Bauer et al. 1992) serves as one specific example.

Criterion 2

These flaws stem unintentionally from an embrace of a fashion stance empowered by the social context of the discipline. Sperber (1990, p. 7) notes, as examples of such stances, '. . . what is new or modern is necessarily best', or 'the scholar who works alone is irresponsible and his findings are suspect', or 'good taste, congeniality, and moderation are preferable to heated debate, political conflict and disruption of the professional status quo'.

There are several examples where we can see clear evidence of fashion stances in the community of geomorphologists. The focus here will be on the specific cases arising from the notion that '. . . what is new or modern is necessarily best' (Sperber 1990, p. 7). The importance of this attitude in geomorphology is best exemplified by Jennings' (1973) classic, *"'Any millenniums today, lady?" The geomorphic bandwaggon parade.'* This chapter alone substantiates the role of fashion in the practice of geomorphology. In Jennings' (1973, p. 115) own words, '. . . I propose to muse about fashions in geomorphology on a thread of personal experience.' And then Jennings dictated his list of the fashions that had influenced the development of geomorphology. He singles out for mention general systems theory, climatic geomorphology, a revitalized historical geomorphology exploiting breakthroughs in dating methods, the 'new morphometry', process geomorphology, quantification, and a new structural geomorphology. Jennings (1973, p. 128) semi-concludes with '...new ideas and approaches must always be welcomed, though it does not follow that everybody should busy themselves with them'. This is a critical notion. The fashion process embraces what is new for the sake of its newness. As scholars, we have a responsibility to be able to critically evaluate, accept, or discard what is new. This can be accomplished within the context of a fashion model, but it cannot be done with innocence.

Other examples of 'if it's new it's good' are found abundantly, if implicitly, in efforts to import innovation to geomorphology. In the last three (or so) decades we have seen claims for entropy (Leopold and Langbein 1962), general systems theory (Chorley 1962), probability theory (Scheidegger and Langbein 1966), allometry (Bull 1975), catastrophe theory (Graf 1979), chaos, or nonlinear dynamics (Phillips 1993), and fractal physiography (Outcalt et al. 1994). Each of these concepts has seen a brief flurry of attention, and most are mentioned in recent reviews of developments in geomorphology. But none have prospered as general approaches after the novelty wore off (although it is too early to extend this verdict to nonlinear dynamics and fractal physiography). And it is critical to note that the ephemeral attention paid each of these 'fashion flashes' reflects but little upon the potential utility of a given theory - just its popularity. Discussion of the implications of different temporal scales of fashion churning in geomorphology is beyond the scope of this chapter. However, the occurrence of these flashes validates Sperber's second criterion for geomorphology.

Criterion 3

There are many models competing for recognition, and the scientific community recognizes successful models through the perspectives of criteria 1 and 2, and through the results of a disciplinary 'popularity contest'.

It is possible to identify a population of models competing for domination of our discipline, and the competition does resemble a popularity contest run according to the rules of criteria 1 and 2. A straightforward demonstration of the existence of many geomorphological models can be found in the compendium published by the Japanese Geomorphological Union (1989) describing the history of geomorphology in 32 countries. The reader of these accounts will be struck by similarities and differences in the development of national geomorphologies. That there is not one common history, and that there persist different geographical patterns of present-day approaches is confirmatory of the competing models condition. More specific evidence for this competition can be found in the discussions of Butzer (1973) and the polemics of Baker and Twidale (1991). After his discussion of four major research streams in contemporary geomorphology, Butzer (1973, p. 41) notes '... it becomes apparent that geomorphology has unusual methodological problems' but continues (p. 43) 'Diversity is a source of strength but it can only be so when pluralism is accepted and tolerated.' This open-handed stance toward competing models is an interesting contrast to the closed-mindedness of Baker and Twidale's (1991, p. 84) plaint concerning '...the current infatuation with theory in Geomorphology'. Further, I would offer Baker and Twidale (1991) and Rhoads and Thorn

(1993) in their (continuing?) debate over the role of theory in our discipline as examples of geomorphologists pitting wits in a contest that can only be resolved as a popularity contest because their arguments are incommensurate from any other perspective. This example alone is ample confirmation of criterion 3 for geomorphology.

Criterion 4

The process of selection among competing models is governed more by matters of taste and consensus than matters of logic or empiricism.

It is possible to identify evidence indicating that competing geomorphological models are selected by a fashion process rather than through rigorous considerations of their logical or empirical qualities. The annals of geomorphology are ripe with such examples, many of them found in the 'prehistory' of geomorphology. For example, Tinkler (1985, p. 50) describes hostile reactions to Hutton's work stemming from the misled beliefs that surface erosion cannot produce substantial landform change. Empiricism certainly would have to (and, in fact, does) weigh in for Hutton. Tinkler (1985, p. 230) also makes the eloquent point: 'It is not always the case that the most visible authors in an era are those who, potentially and from the point of view of posterity, ought have had the most influence.' And Higgins (1975) argues that criterion 4 processes (not using this terminology, of course) contributed substantially to the widespread and rapid acceptance of the geographical cycle (see also Thorn's 1988 discussion).

But it is more instructive to consider this criterion in the light of more modem evidence. And this is also abundant. Twidale (1977, p. 85) considers this issue in some detail and offers evidence to support his contentions that ' . . regrettably, too many geomorphological explanations and hypotheses which either in brief retrospect or even at the time of their announcement appeared untenable, have nevertheless achieved considerable acceptance' and '... some of the answers produced in geomorphology are unacceptable, not because they have been shown to be wrong or unfashionable but because they are so obviously lacking in logic and testing'. These statements are intriguing. Twidale says that acceptability of an answer should be based more upon its logic and testing than upon the degree to which it might be correct. These are curious words from a geomorphologist who later argues for the importance of serendipity and outrageous hypotheses to the development of geomorphology, who espouses a 'maverick geomorphology', and who clearly recognizes 'The distinction between justification and process of discovery' (Baker and Twidale 1991, p. 87). Despite Twidale's earlier opposition to fashion in geomorphology, his later writings indicate a substantial embrace of that very approach.

A more specific example concerns the continued use of Bagnold's (1936) model for aeolian sand transport despite there being little empirical evidence to indicate that it is superior to any of a number of competing models. Indeed, most objective comparisons of aeolian sand transport models, based upon field experiments, find that other models work better (e.g. Berg 1983 or Sarre 1988). The persistent use of Bagnold's model is a reflection of taste and consensus substituting for logic and empiricism, although the model itself may indeed be worthy. We do not know, but it does not seem to matter. The embrace of Bagnold's model is diagnostic of fashion operating in our discipline, and substantiation of criterion 4 in geomorphology.

Criterion 5

Leading scholars in a scientific community are not recognized primarily because of their scholarship, but instead because their work epitomizes the fashion tastes of their discipline and because of the resulting popularity of their research results and perspectives. Rival work is not objectively evaluated against that of the leaders.

It has been argued and demonstrated that the community of geomorphologists recognizes some of its disciplinary leaders more because of their fashion statements than because of their scholarship. This is partially the basis of the evaluation of criterion 1, above. Further, Yatsu. (1992) has argued strongly that this has too often been the case for geomorphology, and his essay alone provides a basis for accepting this criterion. He is amusingly critical of many of geomorphology's leaders, and allows special attention to Chorley, claiming (Yatsu 1992, p. 101): 'It seems to the present author that Chorley is not a geomorphologist at all, but probably an exponent of the enlightenment.' Yatsu continues by noting Chorley's misapprehension of the differences between closed and isolated systems. Despite these (and other) flaws, there can be little doubt that Chorley enjoyed a leadership position in geomorphology for more than a decade.

Other examples are abundant, and probably comprise much of the generic material used in geomorphological thought (concepts/principles/theory/methods) courses. Certainly more geomorphologists have read and appreciated Strahler's (1952) process manifesto than any of his landform studies. Strahler (1992) claims that it is his most frequently cited publication (although the work of Bodman 1991, suggests that this is not so recently). I presume that a similar state maintains for Baker (although I am less convinced about Twidale), and Rhoads, Jennings, and Dury. Twidale (1977, p. 93) explicitly acknowledges his perception of the reality of criterion 5 conditions when he discusses science as a game '. . . in which, unfortunately, personalities, plausibility, and intelligibility are ephemerally as important as the fundamental worth of the ideas propounded'. Whether it is unfortunate or ephemeral or not, it is true in instances common enough to validate criterion 5.

Criterion 6

The disciplinary status quo, or orthodoxy, is defended by the scientific community against unwashed rabble, and this defense may include personal attacks on the outsiders who are 'out of step' with the governing fashion.

There is evidence for such a geomorphological orthodoxy being protected by the community (especially through the actions of the fashion leaders) using these defenses. This is perhaps why Rhoads and Thorn (1994, p. 100) felt constrained to write '. . . philosophical introspection provides an excellent antidote to scientists who wish to divide their colleagues into winners and losers on the basis of methodological preferences'. This may explain why Baker and Twidale (1991) decided that it was appropriate to include gratuitous attacks on human geography in their plea for a geomorphology that is connected with nature. These cases represent both indirect and direct evidence that validates the application of criterion 6 to geomorphology. The first case is a response to the problem, the second is an example of the problem. But issues concerning disciplinary turf protection are not a modern development. They have colored debate in geomorphology for more than a century. Even Davis was moved to complain to W. Penck (in a 1921 letter, published in Chorley et al. 1973, pp. 547-551) about the conservative nature of their discipline's earlier leadership: 'They objected, as many still object, to the use of explanatory methods of description, because of their danger, because of their use of deduction, etc. etc. In a word, they wished to remain purely observational, purely inductive. Indeed many were so prejudiced against deduction that they decried it in others, even when they used it themselves.'

From the many references we have that relate to Davis's harsh treatment of his contemporaries (e.g. the numerous examples provided by Chorley et al. 1973) we might conclude that he prided himself as a keeper of the geomorphological flame of righteousness. Thus Jones, in the 1950 Association of American Geographers Davis Symposium, after dismissing the validity of the geographical cycle, claims that he (Jones 1950, p. 179) was still ' . . . a great admirer of Davis for other of his ideas, not the least of which was heckling presenters of papers at AAG meetings'. Of course Davis was not spared contemporary or posthumous payment in kind. Daly, for exarnple (as quoted in Chorley et al. 1973), made the exclamation concerning some of Davis's work, 'Excellent illustrations, but not a word of truth in it.' Strahler (1950) also had some none too gentle criticisms.

There are numerous examples to substantiate the operation of criterion 6 in geomorphology. I would like to address briefly one specific orthodoxy that is protected by at least part of the geomorphological community. This orthodoxy can only be supported as a fashion, because it fails in aspects of logic, science, and practice. This is the myth of multiple working hypotheses; the notion that a 'good' earth scientist must hold simultaneous, parallel beliefs concerning the explanation of the topic of investigation. The original concept is usually credited to Chamberlin (1897), although Baker and Pyne (1978) assert that the idea was really Gilbert's, and there are many modem practitioners advocating that this method be used by geomorphologists (e.g. Haines-Young and Petch 1983; Schumm 1991). This myth could have served as a case study for Brush's (1974) essay, 'Should the history of science be rated X?' The concept of multiple working hypotheses is commonly taught to earth science students (the primary target audience of Schumm's 1991 book, for example). Yet it is not really a viable methodology and cannot be so. Hull (1988), for example, notes that there are an infinite number of hypotheses available to explain any natural phenomenon, and that the only way to winnow such a spectrum is to rely on one's vision of the relationships of the problem at hand. Although there may be several options remaining after such a process, it is difficult to see the simultaneous evaluation of parallel possibilities. This myth has been critiqued in the essay of Johnson (1990), but it is still advocated as the 'right' approach. It is difficult to interpret whether Baker and Twidale (1991) are supporting or rebuking the concept. But if they are supporting it, then their advocacy of pursuing the 'outrageous hypothesis' (and, of course, there must be more than one 'outrageous hypothesis' per problem) creates a scenario that would grind scientific activity to a halt if taken literally.

The gap between what is practiced and what is taught is extremely large in this instance, and the divergence goes right back to the beginnings of the mythology. Gilbert, claimed as the champion of the method of multiple working hypotheses, did not always practice that method in his own research. An example from Baker and Pyne's (1978, p. 102) essay on Gilbert illustrates this point (and, in fact, a host of others about the practice of science): 'As he approached the Henry Mountains on August 23, he had already formulated a conception of their structure. His field notebook for that day contains the following entry:

"My idea of yesterday in regard to H.M. are confirmed by this view . . . ". In short, Gilbert had conceived the structure of the mountains before he ever actually visited the scene.'

Criterion 7

The orthodoxy, despite any appearance of invincibility, represents a fashion that has replaced an earlier model, and is, in turn, subject to future replacement. Present and past fashions are subject to rediscovery as new and daring fashions in the future.

There is every reason to believe that our present orthodoxy is subject to replacement as part of a fashion cycle, although the issue is a little sticky vis-a-vis geomorphology, because it avoids the preliminary question of whether there is a dominant orthodoxy. For the moment, and for the sake of this discussion, let us presume both that there is such an orthodoxy, and that it is process geomorphology. I do not hold this belief, but it is commonly subscribed to by many. For example, we must believe that it is indicated as so by the dedication of the fourth (final?) volume of the series *The History of the Study of Landforms, or the Development of Geomorphology* to tracking the development of process geomorphology, primarily subsequent to World War 11 (promised in Beckensale and Chorley 1991). Vitek and Ritter (1989) and Strahler (1992) claim that it is so. Kennedy (1992) seems to suggest that at least many take it as so. Sack (1992) says that it is so. Chorley (1978) says that it is so and Phillips (1992) writes like he believes it. And I must presume that Baker and Twidale (1991) believe it or else they would not have bothered with their essay. Yatsu (1992) may (p. 109 and p. 112) or may not (P. 105) think that process geomorphology is the reigning orthodoxy.

If this supposition is accepted (and it costs little to do so), then we see that in almost every example that discusses the ascension of the process balloon there is a direct acknowledgement of its replacement of the geographical cycle, or historical approaches (e.g. Carson and Kirkby 1972). Similarly the last decade has seen increased criticism aimed at the practice, if not the aims, of a process-based science as the core of our discipline (e.g. Baker and Twidale 1991). And given the fact that most geomorphologists do not in fact practice a genuine form of process geomorphology, this domination must be considered nothing more than (perhaps) wishful thinking or popular mythology. Certainly the bulk of recent publications, as reported by Marston (1989) do not support the process fashion (was Marston's listing of *Earth Surface Forms and Processes* in his Table I Freudian?), and there are increasing numbers of articles that criticize the utility of process studies, both from a scientific and social perspective. There are even more words that seem to imply a psychic yearning for the gentler, simpler times when historical and regional studies were the orthodoxy. Certainly Baker and Twidale appear to long for those days when storytelling sufficed for science. They have forgotten, or have failed to realize, that science must be founded upon theory. We can pay heed to a reminder from a historian of science, Miller (1987, p. 381): 'In modern physical science, there is not a single empirical principle which is firmly believed by all without crucial reliance on a theoretical underpinning.'

It is extremely revealing that in the closing sections of Baker and Twidale (1991, pp. 93-96) entitled 'Possible futures' and 'Proposed action' the authors are caught taking a hard look backward. The geomorphologists that they cite in those pages are, with one exception, long dead. The earth science citations they chose averaged more than 70 years of age in 1991 (and this excludes the 1864 reference to Marsh). And in these sections we read phrases such as 'In returning to its common-sense roots, geomorphology . . . '. Wolpert (1992, pp. xi-xii) argues convincingly that science is unnatural, 'Scientific ideas are, with rare exceptions, counter-intuitive ... doing science requires a conscious awareness of the pitfalls of "natural" thinking. For common sense is prone to error when applied to problems requiring rigorous and quantitative thinking; lay theories are highly unreliable.' We have seen the play of fashion as indicated by criterion 7, and it is us.

Criterion 8

The disciplinary proletariat are under constant pressure to conform to the fashion dicta of opinion leaders, even as those fashions change.

There is evidence to support the premise that the 'working stiffs' of the geomorphological community are under constant pressure to conform to changing fashions as dictated by our leadership, although once again it is difficult to demonstrate this through direct example because seldom will we read an 'or else' statement. But one exemplar is an older case, as described by Butzer (1973, p. 42), 'The academic intolerance of Davis and his followers between World War I and II has become proverbial and can still be readily savored in the editorial policies implicit in the defunct *Journal of Geomorphology.'* There are, of course, few greater strangleholds on a discipline than to control the editorial reins of a central journal. This condition remains as an often unspoken, but frequently felt, pressure to conform. Similar, but more insidious pressures come from the panels that meet to evaluate and fund (or not fund) research proposals. For how could these groups comprise any who do not satisfy some measure of orthodoxy? The publication and granting processes must be controlled largely by members of our own invisible colleges because in these cases our community would not (could not) tolerate judgment from outsiders. Further, the repetitive publications telling us what we should study, and how, are all applying pressure, especially if they have passed through the process of affirmation known as refereeing. If the new 'Ten Commandments' are published in *Zeitschrift fur Geomorphologie,* does that not indicate that the hierarchy has stamped their approval on the product? Finally, there is an orthodoxy represented in the collective opinions of the senior faculty (and other professionals) who are called upon to evaluate junior colleagues for tenure and promotion. Therefore, although generational tensions must and should exist, these tensions must also be modulated by issues of taste and consensus - witness the plight of Bretz. Certainly there has been, and continues to be, pressure to conform to an orthodoxy. Or to change it.

I have little doubt that geomorphology behaves as a fashion science according to the criteria proposed by Sperber. The examples provided here may not be the most compelling that exist for our science, but this evaluation requires only adequate examples, and these we have. Geomorphology is a fashion science.

FASHIONS FOR A DEAD MILLENIUM

An investigator may be likened to a hunter in desperate search for food. He is prepared to shoot whatever appears

(Conant 1967, p. 312)

Fashion Dudes in Geomorphology

Sperber (1990, p. 220) defines the fashion, or opinion leaders of a discipline as ' . . . not merely exemplars and trend setters: these opinion leaders are in reality the collectively perceived heroes of their day'. As a graduate student, especially, I held Strahler, Chorley, Leopold, Bagnold, and Scheidegger in esteem to the extent that they represented the (process-oriented) geomorphological heroes that I aimed to emulate (aside from my graduate advisor and dissertation committee members, of course). I am hard-pressed to nominate one candidate for our present fashion hero. The lack of a clear leader is tied to the lack of a dominant fashion as we turn toward the next millennium. Our present state of methodological and theoretical tension dictates that there are multiple, competing fashions, each with a leadership in quest of power over the direction for future geomorphologies. None has dominion now. But we can identify some of the fashion leaders (and near leaders), and their research streams, that have set the stage for the present era, and we can speculate on directions for the pursuit of our science. The list presented here is merely one subset from geomorphology's pantheon. The purpose is to illustrate fashion dudes, and then ferret their commonalities. A recognition of common traits of some of the past masters should illuminate the characteristics to be seen in present contenders.

Davis as the prototype geomorphological fashion dude

It is easy to support an argument for William Morris Davis as the prototype fashion dude for geomorphology. The literature (including the biography by Chorley et al. 1973) discussing the man and his science is replete with mention of attributes suited to a fashion leader. He was a propagandist and heckler. He was a prolific writer and public speaker, and he was a powerful political advocate for his own ideas and for the stature of our discipline. He founded and led the invisible college of his time, and then took it semi-public as manifested in the Association of American Geographers. His leadership position also transferred status to a series of disciples, acolytes, and partial believers: Baulig, Wooldridge, Cotton, Linton, and Johnson among several others. Davis was a one-man invisible college (for a while), and remains the standard against which fashion competitors must be measured.

Gilbert as an underappreciated fashion dude

There is little doubt that Gilbert was and is highly regarded for his contributions to geomorphological science. In many instances his approaches to problems were decades (or more) ahead of those used by his contemporaries. He remains a true, albeit dusty, hero of geomorphology, and he has been usurped by recent generations as a philosophical and methodological ancestor. But although he was a respected member of his scientific community during his professional career, he was not a fashion leader of his time. There are numerous explanations for why this might have been (e.g. Sack 1991), but they are unimportant here. The pertinent issue is that Gilbert failed to establish geomorphological fashions among his peers. His work was admired, but not immediately emulated. If it had been, we might presume that there would have been much less of Davis and his disciples to occupy our attention.

Strahler as a faded dude

Strahler accomplished an exquisite, fashion power-move. As a relative newcomer to the discipline of geomorphology, and with a fashion vacuum in the discipline, he took it upon himself to outline a plan for the restructuring of the science in a manner sure to upset the complacency of the postwar community. He used polemic and exhortation as policy tools to promote his vision (e.g. Strahler 1950, 1952). He obtained and maintained a leadership position largely as a result of the stimulation generated by his 1952 call to arms, through the success of a cadre of his students, and, perhaps most significantly, through his recognition as 'the man who wrote the book'. His retreat from the research frontier created a power void, in the invisible college, an opportunity for new leadership. Strahler's long absence from the cutting edge precludes his status as a current fashion leader. He can, however, be cast in the role of an esteemed forerunner to a modern leader. Strahler was especially fortunate that Richard Chorley went to Columbia, and was willing to take up the mantle of leadership.

Chorley as a science dude

From the process geomorphology perspective on our history, Chorley followed Strahler (perhaps to an extent by pushing him) at the fashion helm of our discipline. He recognized that changes in geomorphology seemed to occur very slowly relative to related disciplines, and that there was much to be gained by vigorously expropriating theory and methods. Chorley has been often criticized for his multiple approaches to the practice of geomorphology, but his versatility was one key to his fashion success. In this sense he epitomizes one aspect of the successful opinion leader - he keeps just ahead of the pack. When the fashion becomes accessible to the masses, the fashion must change. But Chorley made the bold moves, he argued strongly and convincingly for changes aimed to bring respectability to geomorphology, and he published extensively on issues associated with a broad spectrum of geomorphological topics. Finally, his contributions to the *History of the Study of Landforms* (not to minimize the roles of Beckensale and Dunn) guaranteed his position as one of our fashion leaders. But it may well be argued that Chorley's day is past. And, just as we might see Chorley as descended from Strahler, we might ask if the succession now continues through a subsequent anointing of Goudie.

Some common threads

These four brief sketches are not meant to summarize the careers of these geomorphologists. Neither is this short list meant to be representative of the history of our discipline, except from a process-oriented position. But these cases are sufficient to demonstrate some of the threads common to fashion leaders in any aspect of geomorphology, and to contrast the fashion leaders (Davis, Strahler, and Chorley) with the one non-leader exarnple (Gilbert). There are at least six characteristics that are shared by these (and perhaps all) geomorphological fashion leaders. First, all of the successful leaders wrote voluminously, and their most recognized works appealed to a large cross-section of their scientific community, rather than just one or two narrow subdisciplines. It was therefore possible for a large number of their contemporary colleagues to participate in consensus building for the new orthodoxy. Second, there is a strong indication that all of our successful fashion leaders have written text (or text-like) books, that have been widely used to educate nascent geomorphologists. This is the case with all of the examples noted above. Third, forceful presentations at professional meetings, promoting the new fashion and often denigrating opposing viewpoints, seem to be requisite. Fourth, long-term positions in academia seem to be a desirable foundation for fashion setting. Fifth, all of the fashion leaders seem to possess an academic or professional heritage that placed them at or near the center of the leadership of an existing invisible college. Sixth, all of the fashion dudes seem to obtain recognition relatively early in their careers, and are widely acclaimed by their peers, either directly through the bestowal of awards, or indirectly through citations in the literature. We can recognize all six of these traits as common to Davis, Strahler, and Chorley, but also recognize that Gilbert's profile differs substantially. At least four of these characteristics (i.e. the second, third, fourth, and fifth) do not seem to apply to Gilbert's career. Ability is never sufficient (and perhaps only partly necessary in some instances) to become a fashion leader! This summary avoids the potentially critical roles of nationality and national influences because I remain uncertain concerning their importance. There is no doubt that there are national and local fashion leaders, but it is unclear whether they comprise a pool of candidates for the broader geomorphological population, or part of a fashion pyramid.

Consideration of these points might also provide explanations of why it is difficult to decipher the present fashion dude(s). For example, although contemporary geomorphology boasts several prolific writers, most of them confine the bulk of their productivity to a preferred research subdiscipline. A more central (popular) position is desirable for a fashion dude, because it must be extremely difficult to move the discipline from the edges under conditions where most colleagues will not read or hear fashion statements. Another example is that few of the present generation of geomorphologists receive general recognition from the discipline as being fashion leaders. This is implied in the examination of citation patterns published by Bodman (1991) that suggest our discipline is dominated by an entrenched seniority. However, my interpretation is that these leaders are really place holders, as there is no agreement about where we are, or where we (as a discipline) are going. Notable in Bodman's list is the rapidly rising influence of Goudie (again, based upon the citation indicator).

Looking for leadership

There is a struggle going on for the leadership of geomorphology. The struggle is about power, prestige, and control. Geomorphologists are pitting wits, spending resources, risking egos, and buying ballots in attempts to set the agenda for geomorphology in the next millennium. The leaders of our invisible college are reviewing candidates constantly. The tempo of debate and discussion concerning philosophies, methodologies, and the scientific nature of geomorphology is accelerating. This level of interest and attention is diagnostic either of a discipline experiencing healthy growing pains or of a discipline in need of rejuvenated leadership. Given the nature of much of the recent rhetoric, I fear our condition is the latter. Clearly we are not in the throes of scientific revolution. That term is too grandiose a description for our paradigmless discipline. We are instead in the preliminary stages of reconstructing geomorphology for the next generation. It is tempting to ignore the struggle using our own research agenda as justification, or to sit back and enjoy the spectacle and cheer for an underdog in order to prolong the fight. But we are witnessing continuing struggles between factions of particularism and pluralism; between science and history; between geography, engineering, and geology; between the past and the future. It is a contest between rival invisible colleges, and there will be winners and losers. We can accept the fashions that we are presented, or we can be critics and contenders; assessing, constructing, and refusing to accept a shoddy product. We do not have the luxury of watching the procession unless we are willing to bet that the 'good guys' will win, or unless we are willing to concede that we are part of a disciplinary proletariat lined up and waiting to be told what the next fashion is. We need to work to make sure that geomorphology, in a vital and relevant form, wins. And because there is this struggle, there is a correspondingly long list of those who are intentional or accidental competitors for status as our contemporary fashion leaders. The list will shorten substantially only after the initial sparring has selected some finalists.

One reason for this attempt to establish the importance of fashion in shaping the development of geomorphology is that such a disciplinary self-awareness will allow us to appropriate the fashion process in a critical and therefore useful manner. For although we cannot all be fashion dudes, we all have the option (perhaps the obligation) to be fashion critics. In the preceding section, I have outlined some of the characteristics that we might recognize in a leader, and we can employ these traits in an examination of conditions conducive to the establishment of a new fashion leader. The first goal of this process might be to reduce the present number of competing fashions because this will reduce the length of our list of potential fashion contenders. Although this may not be a desirable process from a disciplinary perspective, it is eminently fashionable! Appraisals of potential semifinalists would be based upon current or potential stature. Alternatively, we might justify the search for a larger population of fashions from which to choose. This can be rationalized through an argument that none of the present contenders are truly worthy.

It must be assumed that the successful opinion leaders will be able to transcend their subdisciplines, either by intentionally writing for a larger audience or because their specific research leads to a discovery that is widely applicable. Examples of broad topical areas that seem to be of modem geomorphological interest and where many subdisciplines can participate include applied geomorphology (e.g. Nordstrom and Renwick 1984 or Sherman 1989), natural hazards (e.g. Gares et al. 1994), public policy (e.g. Graf, Chapter 18 this volume), or environmentalism (e.g. the new text series being edited by Goudie and Viles).

We might also agree on several assumptions about what character traits and professional attributes are desirable (acceptable) for election as a fashion leader. The contenders must have reasonable scientific credentials, they must be in mainstream or near mainstream branches of geomorphology (I do not know if we would line up behind a karst geomorphologist - maybe). They must be somewhat politicized and 'publicly active'. They must be willing to stick their necks out to compete for a leadership role, because it is presumed that no one can become and remain a leader unintentionally. They must be generationally young enough to at least give the appearance of being (post)modern and progressive, a substantive fashion requirement. Finalists in the fashion competition will probably embrace one or more aspects of public policy/social relevance as a campaign theme.

The fashion dude for geomorphology at the dawn of the twenty-first century must be clever and wise enough to manage the power and authority necessary for the position. This person must be willing to serve as the president or director of our invisible college. He or she will probably need to write and speak concerning important issues of methodology and philosophy, and be willing to take unpopular stances and convince our community that the position is fashionable (because it is important). The new leader will be a public face for geomorphology and a defender of the discipline. Our leader will not hide behind the anonymity of review panels and editorial boards, although he or she may manipulate those bodies. That is part of the inevitable role of the invisible college. The leader will not hesitate to establish and maintain rules, and change rules as the moment (fashion) demands.

The operation of the fashion process in geomorphology frees us to embrace new theory, method, philosophy. We are able to skip Baconian generations in advancing our discipline, because the fashion model encourages creative ahistoricism. We can enjoy a more critical perspective on debate and power struggles. The sociologists, historians, and philosophers of science, are necessarily preoccupied with the past, at least in terms of the evidence and events that govern their interpretations of science. We cannot rely upon these scholars for the appraisals of geomorphology past, or for predictions of future geomorphologies. We need to be in an informed position to make conscious decisions about the future of our science. We can recognize that the quest for power or prestige moves geomorphology as much as (or more than) some scientific process. We can identify potential leaders and appraise them according to guidelines of our own fashion. It is destructive and foolish to deny the role of fashion in geomorphology, or to pretend that it is unimportant. Geomorphology as a science and as a discipline can benefit from the innovative qualities of the fashion process, if we choose to participate. The decision is ours. Geomorphology belongs to us.

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