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Shifting paradigms in geomorphology: the fate of research ideas in an educational context

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Abstract

The acceptance of new ideas into the mainstream of geomorphological education is illustrated from the development of theories dealing with Earth history, glaciation, uniform flow, mass movement, continental mobility, cyclic erosion, and drainage networks. The lag between the conception of new ideas and their incorporation into mainstream texts has varied from negligible to more than 200 years. On one hand, despite its then untestable assumptions, the Davisian cycle of erosion gained rapid favor as the dominant paradigm of the early 20th century before it was found wanting. In contrast, concepts of uniform flow and slope stability, confirmed in the 18th century, waited almost 200 years for incorporation into geomorphology texts sensu stricto, although they had long been available in books on hydraulics and soil mechanics. Continental mobilism had a wild ride, culminating in the eventual acceptance of the plate-tectonics paradigm in the later 20th century. Explanations for the fate of these and other ideas are varied. New ideas are often opposed by establishment conservatism, language barriers, the perceived surrealism of new concepts, and simple ignorance. In contrast, new ideas may be accepted, sooner or later, by virtue of simplicity, forceful and well-connected leadership, or the death of opponents. Although mitigated by the information revolution of recent decades, these forces still persist and influence the extension of new ideas into a larger arena.

Keywords: Geomorphology; History of science; Landform evolution; Classical mechanics; Education

> Nullius addictus iurare in verba magistri [In the words of no master am I bound to believe]—Horace

1. Introduction

In the introduction to his stylish book on geomorphology in 1942, O.D. von Engeln of Cornell University presented a portrait of W.M. Davis above a caption entitled "The Master." On the facing page was a portrait of Walther Penck, "The Challenger." This was recognition of geomorphology's then dominant paradigm, Davis' cycle of erosion, and of the controversy raging over whether Davisian peneplanation or Penckian slope retreat was responsible for landscape denudation. These personalities and their ideas have long been buried beneath retrospective critiques and it is not my intention to resurrect them. However, the question may be asked as to how an individual could rise to the exalted status of 'master' in a text that influenced so strongly a future generation

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of geomorphologists. It may have been justified in the intellectual climate of the time, but the very sobriquet and the text that followed either ignored the contributions of others, or justified them before the Davisian altar. This raises further questions. How many other potential 'masters' have been ignored, even ridiculed, in the past, only to have their ideas gain acceptance at a later time? How many potential leaders and their ideas still lie unappreciated for want of a sympathetic audience? How and when do new ideas developed at the research frontier become integrated into mainstream educational texts designed to shape future generations of scientists? Once educated, how do scientists gain acceptance for new ideas among the makers of public policy and the broader public?

The above questions are not unique to geomorphology but they are particularly relevant in the context of this paper. Geomorphology as a research field has had a mixed record of acceptance by educational circles, policy makers, and a wider public. Part of the problem exists because of the seemingly separate goals of research and education, the former concerned with discovering new facts and testing fresh theories, the latter with disseminating acceptable information in digestible form. To some extent, this dichotomy reflects the differing approaches of scientists and educators, the former focused but often remote and argumentative, the latter usually ensconced comfortably in teaching institutions and traditional paradigms, and wary of new ideas, certainly those that appear heretical or revolutionary. Lacking educational acceptance, new geomorphological ideas and approaches have much less opportunity to affect public policy. The latter in turn, subject to so many political, economic, and legal constraints, thus function in a generation gap deprived of fresh approaches to recurrent problems.

Using examples selected from the history of geomorphology, this paper examines the fate of new research concepts in the educational arena, specifically the time lag that may develop between the initiation of new ideas at the research frontier and their incorporation into influential mainstream texts. Theories of Earth history, glaciation, uniform flow, mass movement, and continental mobility, that are widely accepted today, experienced long periods in the wilderness, largely because they were revolutionary or incomprehensible, and thus, initially at least, failed to secure a sympathetic audience in the prevailing academic climate of the time. Quantitative approaches to geomorphology were likewise long shunned as irrelevant or indigestible. Some ideas were simply ignored because they were developed elsewhere in unfamiliar languages, an intellectual nationalism that still lingers. Conversely, fresh ideas were more successful when they emerged from the establishment of the time, or were conveyed in simple terms to an unprejudiced audience, or simply because their opponents died. The paper concludes with an evaluation of why new concepts in geomorphology become acceptable, sooner or later, and of the lessons to be learned for the future. The fate of new ideas is of course influenced by the nature and speed of communication, as exemplified in the contrast between privately published books in times past and modern electronic transmission of research proceedings.

2. The fate of new ideas

The history of science is replete with new ideas and observations that were ignored or rejected by the establishment of the time, sometimes with cruel consequences for their authors. The case of Galileo Galilei is particularly poignant, his espousal of the Copernican model for Earth's place in the solar system being condemned in 17th century Italy and only finally given papal approval in 1984. As the Earth Sciences began to take shape during the Renaissance of the 16th and 17th centuries and the Enlightenment of the 18th century, it seems that few were truly reborn, that even fewer were truly enlightened. Many new ideas were stifled within academia and gained only belated acceptance beyond the academic environment. Geomorphology, which traces its roots to this period of the Renaissance and the Enlightenment, suffered in much the same way.

In essence, geomorphology owes its emergence as an intellectual discipline during the later 19th century to twin foundations laid during the preceding 200 years (Orme, 1989). One foundation lay in the historical approach to Earth Science that emerged during the later 17th and 18th centuries (Davies, 1969). This approach developed slowly during the 19th century, initially against strong opposition from catastrophists, but was eventually given formal expression in the attractive Davisian cycle of erosion. To many geomorphologists in the 20th century, especially those concerned with landform evolution, this was the only true foundation, perhaps because the links were so clearly evident. Thus, where stratigraphy was lacking, geomorphologists sought to provide denudation chronologies for Cenozoic landscapes as logical extensions of pre-Cenozoic stratigraphic records. In various guises, this evolutionary aspect of geomorphology was central to most texts of the time (e.g., von Engeln, 1942). Further, because the accompanying debates were widely available in English, the evolutionary approach spread readily throughout the anglophone world, as exemplified by King (1962), based in South Africa, and Twidale (1976) in Australia. Similar historic/genetic approaches, variously tempered by climatic considerations, also dominated French and German geomorphology for much of the 20th century (e.g., de Martonne, 1909, 1940; Birot, 1960; Büdel, 1963).

Geomorphology's other foundation lay in the emergence and application of classical mechanics among hydraulic engineers in continental Europe, also during the later 17th and 18th centuries These roots, however, were long ignored by most Earth scientists, in part because their links were so scattered and disjoint, in part because of the prolonged dominance of evolutionary paradigms (Orme, 1989). Furthermore, a certain cultural myopia among anglophone scientists, a preference for Earth Science in a familiar language, blinded many to achievements in other languages and other emerging disciplines. Even non-anglophone scholars were drawn to evolutionary concepts, especially the Davisian model. Thus, the approach from classical mechanics, long ignored by all but a few within geomorphology, did not find its way into the mainstream texts and educational curricula of the field until the later 20th century. Among the scholars who did espouse this approach earlier, neither Grove Karl Gilbert nor Albrecht Penck was initially successful in gathering converts.

2.1. Theories of the Earth

Theories about Earth's origins and development abounded during the 17th and 18th centuries, as reflected in the many books and pamphlets of the period. Their various fates have been much discussed (e.g., Chorley et al., 1964; Davies, 1969; Tinkler, 1985) and are summarized here only to support the present argument. In general, these theories are often grouped under the rubrics of catastrophism and uniformitarianism, although these terms were not formally coined until later (Whewell, 1832).

Other aspects apart, catastrophism is a belief that ascribes the origin of Earth's landforms to one or more themes: more or less instantaneous formation during Creation; formation after Noah's Flood; and earthquake and volcanic activity (Davies, 1969). These ideas were reflected in influential educational texts of the time. In his *Geography Delineated Forth in Two Bookes* (Carpenter, 1625), for example, Nathanael Carpenter (1589–1628) wrote that "mountains, valleyes, and plaines were created in the Earth from the beginning, and few made by the violence of the Deluge." Such views were echoed by Bernhard Varenius (1622–1650) in his *Geographia Generalis* (Varenius, 1650) and given further support in the 1650s by the eminent biblical scholar James Ussher (1581–1665), Archbishop of Armagh. Ussher concluded that the Creation of Heaven and Earth had occurred "upon the entrance of the night preceding" Sunday, October 23, in the year 4004 BC, with 'man' and other creatures appearing on the following Friday. He also calculated that the Flood, that other catastrophic event described early in the Old Testament, had occurred between December 7, 2349 BC and May 6, 2348 BC (Ussher, 1650, 1654, 1658). Although religious dogma had long constrained scientific enquiry, when these dates were inserted into the margin of the new Authorized Version of King James' Bible in 1701, they came to possess an authority similar to the scriptures themselves. Thus, Thomas Burnet (1635–1715) in his *Telluris Theoria Sacra* of 1681 and 1689 (Burnet, 1681, 1689), could reasonably ascribe the Flood to the bursting of a fluid-filled globe which "at one stroke dissolved the frame of the Old World and made us a new one out of its ruins which we now inhabit since the Deluge." Supported by religious beliefs and political establishments, many such ideas dominated the 18th century (Fig. 1).

Uniformitarianism, the simple notion that the present is the key to the past, is commonly linked to the writings of James Hutton (1726–1797), but in many respects his work reflected the emerging concern in the Age of Enlightenment for a more rational

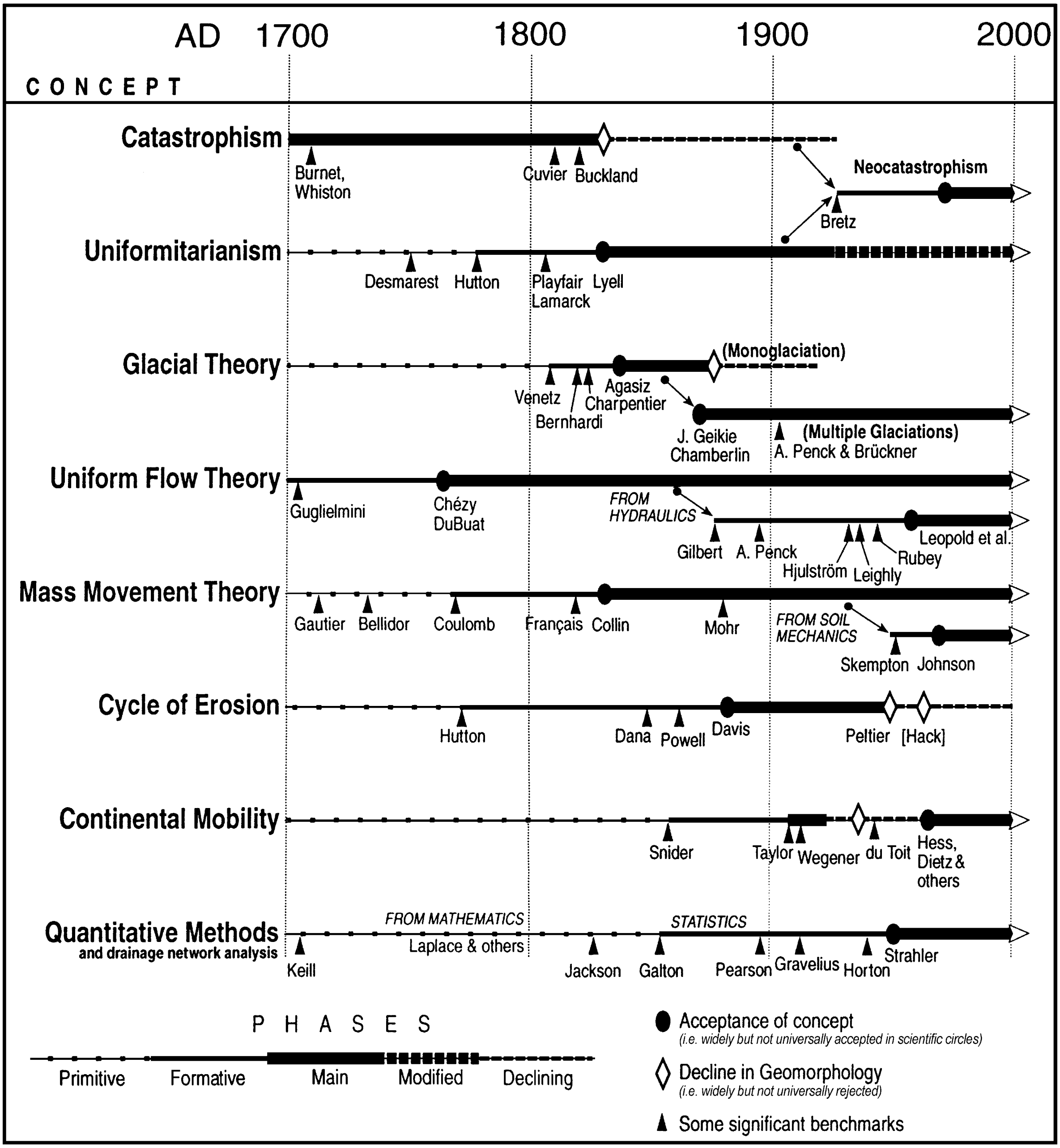

Fig. 1. The rise and fall of selected concepts in geomorphology and related fields.

explanation of Earth history. For example, the primacy of rivers in shaping relief had been recognized earlier by Mikhail Lomonosov (1711–1765), Jean-Etienne Guettard (1715–1786), and Nicholas Desmarest (1725–1815), but their ideas were often clouded by cumbrous prose and suffered, among English speakers, from language barriers. Hutton's writings were cumbrous but the essence of his message, notably his theory of Earth history that found "no vestige of a beginning—no prospect of an end" (Hutton, 1788), was rescued and clarified by his friend, John Playfair (1748–1819), an eminent professor of mathematics in the University of Edinburgh. In the same year, 1802, that Playfair presented his *Illustrations of the Huttonian Theory of the Earth* (Playfair, 1802), Jean-Baptiste Lamarck (1744–1829) was publishing, privately, seemingly quite independently, and in French, similar ideas in his *Hydrogéologie, ou Recherches sur l'Influence Qu'ont les Eaux sur la Surface du Globe Terrestre* (Lamarck, 1802) (Fig. 1).

Acceptance of uniformitarian principles by the educational establishment of the time was a slow, tortuous process, in part because it represented a radical departure from conventional wisdom. Whereas the intellectual climate might favor new ideas, the social and political climate, traumatized by the American and French revolutions and the Napoleonic wars, urged caution. As Lyell later observed, at another time the force and elegance of Playfair's style should have insured acceptance for Huttonian doctrines but catastrophism in its various guises implied religious and social orthodoxy whereas Hutton's "no vestige of a beginning–no prospect of an end" was dangerous heresy. Lamarck's *Hydrogéologie* was conceived as a comprehensive terrestrial physics with a vision of an Earth system in which natural processes produced gradual changes over long periods of time. Falling short of these lofty goals, the book was published privately and its limited circulation destined it for temporary obscurity (Orme, 1989). Thus, eminent catastrophists in powerful positions, such as Jean André de Luc (1727–1817), science advisor to Britain's Queen Charlotte, and Richard Kirwan (1733–1812), President of the Royal Irish Academy, were able to lead spirited attacks on nascent uniformitarianism. For many years after its inception in 1818, the *American Journal of Science* actively fostered catastrophist beliefs. For example, J.W. Wilson wrote in 1821, "Is it not the best theory of the Earth, that the Creator, in the beginning, at least at the general deluge, formed it with all its present grand characteristic features?" (Wilson, 1821). By 1830, however, de Luc and Kirwan were dead, and Georges Cuvier (1769–1832), the eminent Swiss paleontologist, had invoked an extended timescale to accommodate the many faunal 'catastrophes' in the stratigraphic record. The intellectual climate, like the social milieu, had become sufficiently agreeable for Charles Lyell (1797–1875) to present compelling, if rather extreme, support for uniformitarianism in his *Principles of Geology*, volumes that were to have a profound impact within and beyond the Earth Sciences (Lyell, 1830–1833). The subtitle of Lyell's *Principles—Being an Attempt to Explain the Former Changes of the Earth's Surface by Reference to Causes Now in Operation*—is particularly meaningful. However, almost half a century had passed since Hutton first presented his ideas before an academic audience in Edinburgh (Fig. 1). Lyell's uniformitarianism was in turn to prove too rigid, and a qualified form of catastrophism, divorced from its biblical links, re-emerged in the 20th century to explain some of the sudden changes that have punctuated landform evolution. Bretz (1923) invoked 'catastrophic' superfloods released onto the Columbia Plateau by collapsing Pleistocene ice dams to explain the Channeled Scabland of eastern Washington, a concept that was also long viewed with skepticism.

2.2. Diluvial and glacial theories

A further aspect of the catastrophist–uniformitarian conflict was the quest for explanation of the surficial deposits that so often draped over bedrock across much of Europe and elsewhere. To many, certainly to those who had never seen a glacier at work, these deposits were clear vindication of the biblical Flood. Again, persons of established credentials or religious conviction favored catastrophic explanations and delayed acceptance of new ideas. Thus, Horace-Bénédict de Saussure (1740–1799), the eminent French naturalist; William Buckland (1784–1856), professor of mineralogy at Oxford University, Dean of Westminster, and author of *Reliquiae Diluvianae* (Buckland, 1823); and Adam Sedgwick (1785–1873), professor of geology at Cambridge, each supported variants on the diluvial theory. Alpine peasants and mountain guides, who had seen glaciers retreat from Little Ice Age maxima, knew better (Chorley et al., 1964). So too did thoughtful observers like Bernard Kuhn, Ignace Venetz, and Jean de Charpentier in the Alps, Jens Esmark in Norway, and Reinhard Bernhardi in northern Germany, each of whom presented papers in the 1820s and 1830s on the likely role of former glaciers in erosion and sediment transport, and Karl Schimper, the German botanist, who in 1837 coined the term *Eiszeit* (ice age).

Despite accumulating evidence and Playfair's early support, glacial explanations for far-traveled erratics and tills made slow progress against entrenched diluvialism and were confounded by the alternative iceberg theory espoused by Lyell. It fell to Louis Agassiz (1807–1873), a young and exuberant fish paleontologist with impeccable credentials (he had studied with Cuvier and in most respects was a catastrophist), to establish the case for glacial theory (Fig. 1). His address to the Swiss Society of Natural Sciences in Neuchâtel in 1837 and his book *Etudes sur les Glaciers* (Agassiz, 1840), published privately a year ahead of Charpentier's *Essai sur les Glaciers* (Charpentier, 1841), firmly established his leadership in the field, although some of his notions, such as the advance of polar glaciers into the Mediterranean and Amazonia, were soon rejected. But he lost friends such as Charpentier and Schimper in the process, and was notably discouraged by Alexander von Humboldt, while converting Buckland to the cause and causing Lyell to waiver (Imbrie and Imbrie, 1979). Furthermore, his arrival at Harvard University in 1846, and the patronage of wealthy New England industrialist, John Lowell, ensured a wider audience for his views. The case for glacial theory was made not so much on evidence accumulated by careful observers over several decades, but on the credentials of a vigorous young scholar, whom Hallam (1983) has called "the glacial evangelist," whose catastrophism was acceptable to the establishment but who was perhaps less than circumspect in his treatment of erstwhile colleagues and field companions.

Despite Agassiz' advocacy, however, glacial theory made slow progress against entrenched beliefs over the next two decades, and some opposition persisted to the close of the century. The theory was exposed to a broader audience by James Geikie's *The Great Ice Age and its Relationship to the Antiquity of Man*, first published in 1874 (Giekie, 1874), and later by G.F. Wright's *The Ice Age in North America and its Bearing on the Antiquity of Man* in 1889 (Wright, 1889). Both of these popular books, whose titles also reflect the continuing Darwinian controversy over human origins, were to see several editions as evidence for multiple glaciations accumulated. James Geikie's book was especially important because he moved among influential scientists, including T.C. Chamberlin in North America and Otto Torrel in Sweden, who early recognized the evidence for multiple glaciations, effectively retiring Agassiz' monoglacial concepts and Lyell's iceberg origin for drift. Indeed, the first formal publication of North American glacial stages using geographic names was a contribution by Chamberlin to the 3rd edition of Geikie's *The Great Ice Age* in 1894 (White, 1973). By then, broader questions regarding climate change, initiated by John Herschel in 1830 and Joseph Adhémar in 1842, had been given wider currency by James Croll (1821–1890) in his *Climate and Time* (Croll, 1875).

2.3. Uniform flow and related theories

No hiatus in the history of geomorphology has been as long as that which intervened between the confirmation of uniform flow theory during the mid-18th century and its incorporation into mainstream educational texts in geomorphology some 200 years later. The reasons are complex but probably resolve into four main causes—language barriers, the reluctance of theorists to recognize practice, compartmentalization of science, and lack of an evangelist with a receptive audience. The lag is all the more surprising because of the assumptions concerning fluvial processes made, more on faith than evidence, during the Davisian interlude of the earlier twentieth century.

Uniform flow occurs within stream channels when frictional resisting forces are equal and opposite to the gravitational force impelling water downslope. The concept was first seriously discussed in a major geomorphology text by Leopold et al. (1964), in their now classic *Fluvial Processes in Geomorphology*. By then, the concept was more than 200 years old (Fig. 1).

The history of human relations with water is replete with an awareness of fluid dynamics. Irrigation ditches were being constructed in Mesopotamia long before the date proposed by Ussher for the Creation. The subsequent success of Egyptian irrigation works, Persian qanats, Chinese flood-control projects, and Indian water-supply systems all indicate an empirical appreciation of hydraulics extending back several thousand years. Greek scientists and Roman engineers revealed similar understanding, even if the corpus of their scientific theory remains elusive. Much later, during the Renaissance, the formulation of mechanics as a physical science reflected the contributions of Leonardo da Vinci (1452–1519) on hydrodynamics, Bernard Palissy (1510–1589) on the hydrologic cycle, Simon Stevin (1548–1620) on hydrostatics, and Benedetto Castelli (1577–1644) and Blaise Pascal (1623–1662) on fluid dynamics.

Nevertheless, many of these contributions were scattered and misleading. Leonardo was a prolific writer but rarely published, such that his ideas mostly emerged later through edited and variable translations. Palissy's (1580) *Discours*, published in French rather than Latin, then still the medium of scientific communication, was only rescued from obscurity nearly a century later by Pierre Perrault (1611–1680), one of the founders of modern hydrology (e.g., Biswas, 1970). And Castelli (1628), often termed the founder of the Italian school of hydraulics by virtue of his 1628 treatise *Della Misura dell'Acque Correnti*, believed that stream velocity was directly proportional to water depth.

The above works did however mark a transition from vaguely theoretical to observational methods of research. Knowledge was further advanced by Edmé Mariotte (1620–1684), who used interconnected weighted floats to demonstrate the vertical velocity profile in streams, and by Domenico Guglielmini (1655–1710) whose keen field observations, as reflected in his *Aquarum Fluentium Mensura Nova Methoda Inquisita* (Guglielmini, 1690) and *Della Natura dei Fiumi* (Guglielmini, 1697), must place him among the direct lineal precursors of modern fluvial geomorphology (Orme, 1989). Guglielmini understood the variation of stream velocity with depth and slope, the nature of streamflow acceleration and opposing bed resistance, the relationship between channel geometry and sediment scour and fill, and particle-size reduction in a downstream direction. These ideas were further refined as the 18th century progressed, notably by Bernoulli (1738) and Brahms (1753).

Confirmation of uniform flow theory is generally credited to Antoine de Chézy (1718–1798) and Pierre DuBuat (1738–1809) (Fig. 1). Chézy spent most of his working life as an engineer with the Ecole des Ponts et Chaussées in France and in 1768 was entrusted with the design of a canal to bring water from the River Yvette to Paris. Lacking proven methodology to ensure optimal flow conditions, he developed his own flow formula and tested it with experiments in the Courpalet Canal and River Seine. The result has come down to us as the well-known formula $V = C\sqrt{RS}$ (Fig. 2). This formula was well established by 1775, but its analysis was omitted from Director Per-

Antoine de Chézy (1719-1798)	$V = C\sqrt{RS}$ (C = 272 for French units; 57.3 for English units)	1775
Pierre DuBuat (1738-1809)	$V = \dfrac{48.85\sqrt{R} - 0.80}{\sqrt{1/S} - 1n\sqrt{1/S + 1.6}} - 0.05\sqrt{R}$ (pouces/s)	1779
J. Eytelwein (1764-1848)	$V = 50.9\sqrt{RS}$	1796
Philippe Gauckler (1826-1905)	$V = \lambda_2 R^{2/3} S^{1/2}$ for $S > 0.0007$	1867
Robert Manning (1816-1897)	$V = 62\ S^{1/2} (R^{1/2} + R/7 - 0.05)$ (ft/s) $V = 34\ S^{1/2} (R^{1/2} + R/4 - 0.07)$ (m/s)	1889
"Manning Formula"	$V = K\ \dfrac{S^{1/2} R^{2/3}}{n}$ (K = 1 for metric units; 1.486 for English units)	TODAY

where:
V = mean stream velocity
S = slope
R = hydraulic radius
C, λ = factors of flow resistance
n = coefficient of roughness

Fig. 2. The development of uniform flow formulae for open channels.

ronet's report on the Canal de l'Yvette. It was not until the American engineer Clemens Herschel found it more than a century later among the files of the Ecole des Ponts et Chaussées that Chézy's contribution became more widely recognized (Herschel, 1897).

Meanwhile, apparently unaware of Chézy's work, DuBuat (1779) presented his own experimental work on uniform flow in his influential *Principes d'Hydraulique* in 1779 (Graf, 1971). But his formula was cumbersome and was much modified by later workers, notably the German engineers Reinhard Woltman (1754–1837) and Johann Eytelwein (1764–1848), and ultimately by Robert Manning (1816–1897) in Ireland (Woltman, 1790; Eytelwein, 1801; Manning, 1891) (Fig. 2). Although Manning viewed it as oversimplified, it is his formula that was widely adopted in engineering practice and with which most modern geomorphologists are familiar.

Some 200 years elapsed between confirmation of the uniform flow theory and its admission into geomorphology texts, although it had long been available in engineering hydrology works (e.g., Beardmore, 1862) and was reflected in comprehensive studies of the Mississippi River published in 1861 (Humphreys and Abbott, 1861). Albrecht Penck (1858–1945) had incorporated the Chézy equation, together with a critical shear–stress formula for the transport of fluvial gravels, into his *Morphologie der Erdoberfläche* in 1894 (Penck, 1894), but according to Anhert (1998), this work went largely unnoticed and unused for the next half century. Uniform flow and equilibrium concepts were also fundamental to the work of Grove Karl Gilbert (1843–1918) on the entrainment and transport of debris by rivers, but again their entry into the educational mainstream was long delayed (Gilbert, 1914, 1917).

Why did these concepts make such a belated entry into geomorphology texts? For the most part, scholars concerned with such weighty matters as the origin of Earth's landforms could hardly worry about the behavior of water in French canals. In addition, as science matured during the 19th century, a rift developed between theory and practice that was to affect the subsequent dissemination of ideas. Classical mechanics based on inviscid fluids appealed to theorists but was not readily usable by practicing engineers who needed designs for viscous fluids. Mathematicians and physicists developed theoretical relationships that often could not be used by engineers, while engineers developed empirical solutions that could rarely be applied beyond the limited range of problems for which they were devised (Biswas, 1970; Orme, 1989).

G.K. Gilbert was an exception. Fully aware of the work of the mathematicians and physicists of his time, he sought to weld the physical sciences with geology in his field observations and experimental studies (Baker and Pyne, 1978; Chorley and Beckinsale, 1980). As his biographer, Stephen Pyne, has so aptly stated:

> "...as he [Gilbert] argued by his own example, no topic was so trivial or refractory that it could not be expressed according to the laws and logic of physics, and no physical law was so inviolate that it could exist meaningfully outside of a specific context in the facts of physical geology" (Pyne, 1980, p.134).

Gilbert read deeply into the scientific literature of his age, including foreign sources. His experimental work was accorded mathematical precision, and his quest for fundamental physical laws and rational explanations of observed relationships unending. But, excepting his junior colleagues in the United States Geological Survey by whom he was much loved, Gilbert had no students, no educational platform from which to galvanize a fresh generation of geomorphologists (Pyne, 1980). He did write a successful high-school text, *An Introduction to Physical Geography* with Albert Perry Brigham in 1902 (Gilbert and Brigham, 1902) but, though drawing heavily on the federal western surveys and offering a rich bibliography, including works by W.M. Davis, the book did not truly reflect the research frontier with which he was so involved. His quiet unassuming personality also mitigated against evangelism. As Pyne has observed, Gilbert's scientific temperament was classical and conservative, rather than romantic or revolutionary. His achievements were lauded in his own lifetime and were incorporated after his death into mainstream texts in engineering hydraulics (e.g., Rouse, 1938).

Thus, while the scientific lineage of Guglielmini, Chézy, and Gilbert was being advanced by a few, such as geographer Leighly (1934) and geologist Rubey

(1938) in North America, and Hjulstrom (1935) and Bagnold (1941) elsewhere, most geomorphologists of the earlier 20th century were oblivious to, or at least ignored, the fundamental mechanics of their science developed so long ago. Widely used geomorphology texts of the period, such as von Engeln's *Geomorphology* (1942), while rich in genetic inferences for landform evolution, were largely silent on the mechanics of geomorphic process.

2.4. Mass movement theories

In similar vein, the inclusion in geomorphology texts of process-oriented approaches to mass movement was also delayed, although such information had long been available in engineering texts concerned with soil mechanics (Fig. 1). Eventually, Johnson recognized the need for "applying mechanics to the solution of geological problems" in his eclectic book on *Physical Processes in Geology* (Johnson, 1970), but texts such as those by Carson and Kirkby (1972) and Young (1972), important milestones in modern hillslope geomorphology, continued to initiate their discussion with reference to qualitative Davisian and Penckian concepts.

The foundations of modern studies of slope stability were laid down in Coulomb's Statics Memoir in 1773 (Coulomb, 1776). Charles Augustin Coulomb (1736–1806) was a military engineer, with a better grasp of mathematics than most, who achieved distinction from his studies of electricity, magnetism, and torsion (Gillmor, 1971). With the French predilection for war during the preceding century, there had been many distinguished military engineers, such as Sebastian Vauban (1633–1707) and Bernard Bellidor (1671–1761), but also several notable engineering failures based on the faulty designs of earthworks. Slope stability problems were treated empirically in terms of idealized geometries, with a belief that failures invariably occurred at the angle of repose, and with little concern for soil properties.

In contrast, Coulomb recognized that the angle of repose of a free-standing bank of homogenous earth was not the same as the angle of the rupture plane and that, assuming retarding forces attributable to cohesion and friction, failure could occur along any one of several planes. The modern expression of Coulomb's equation is generally given as $S = c + \sigma \tan\phi$ where S is shearing resistance, c is non-directional cohesion per unit area, σ is effective normal stress on the slide plane, and ϕ is the angle of internal friction. He also showed how infiltrating water could reduce the angle of internal friction and under more buoyant conditions lead to failure at lower values of ϕ.

Coulomb's contribution to soil mechanics was not immediately acknowledged (Fig. 1). The cumbrous algebraic expression that he initially proposed and his reasoning that slip planes are commonly steeper than containing natural slopes discouraged easy acceptance of his work. However, as the 19th century progressed, support from the Director of the Ecole des Ponts et Chaussées, and widespread observations and experimental testing, especially by canal and railway engineers, verified and refined Coulomb's conclusions (e.g., Collin, 1846; Mohr, 1871, 1872). Early in the 20th century, the science of soil mechanics was given formal status by the writings of Terzaghi, Fellenius, and Krey (e.g., Terzaghi, 1925; Skempton, 1979). But not until the 1960s did these explanations for mass movement enter a major text in geomorphology (Leopold et al., 1964), and a further decade elapsed before such information became common fodder for geomorphology students (Fig. 1). Instead, in the continuing debate over landscape denudation, most texts continued to contrast Davisian downwearing and Penckian backwearing of slopes, with nary a slope measurement and only lip service given to the internal mechanics of landslides and debris flows.

2.5. Tectonic theories—stabilism versus mobilism

Few theories in the Earth Sciences have attracted such excitement as the attempts to explain Earth's primary and secondary relief features with reference to a mobile crust. Today, we may reflect on the inescapable logic of plate tectonics, on the wisdom of our immediate predecessors in accepting the theory and suggesting what appear to be rational explanations. But it has not always been so. Alfred Wegener (1880–1930), the German scientist most commonly linked with plate tectonics through its lineal predecessor, continental drift, was ridiculed during his own lifetime and did not live long enough to see his ideas vindicated.

In mobilist theory, Wegener had several antecedents. In his book *La Création et ses Mystères*

Dévoilés (Snider, 1858) for example, catastrophist Antonio Snider invoked the fissuring of the Atlantic on the sixth day of Creation to explain the similarity in fossil plants within the Carboniferous coal deposits of Europe and North America. His ideas were soon viewed as too outrageous to merit serious attention (Holmes, 1944). Later, Taylor (1910) invoked the concept of crustal creep from high to low latitudes to explain the distribution of mountain ranges, but his ideas were rejected largely because his mechanism, tidal forcing related to Earth's capture of the Moon in the Cretaceous, was untenable. In any case, these and other ideas, plausible or otherwise, were accorded short shrift in the prevailing stabilist paradigm of the late 19th and early 20th centuries. After all, were not Earth's continents, ocean basins, and their major relief features admirably explained by the model of a cooling and contracting Earth so convincingly advocated by J.D. Dana (1813–1895) in North America and Edward Suess (1831–1914) in Europe, and elucidated in Lowthian Green's tetrahedral hypothesis (Green, 1875)?

Thus, when astronomist and meteorologist Wegener published brief papers on continental drift in 1912 and a subsequent book on *Die Entstehung der Kontinente und Ozeane* in 1915 (Wegener, 1915), the intellectual climate in general was unlikely to be favorable. In Germany, however, mobilist concepts were somewhat more familiar from the works of Wettstein, Colberg, and Kreichgauer (Hallam, 1983). Furthermore, Wegener was well connected, having married the daughter of the distinguished meteorologist, Wladimir Köppen, whom he later succeeded as Director of the Hamburg Marine Observatory, and was thus assured of some sympathy for his views (Jacobshagen, 1980). This he received from such notables as the Swiss structural geologist Emile Argand (1879–1940), a founder of the nappe theory of the Alps, and later from Arthur Holmes, then of Durham University, whose convection-current model offered a mechanism for continental drift, and from Alex. du Toit, the South African geologist, who was well placed to provide supporting evidence from Gondwana. In other respects, however, Wegener's ideas were openly ridiculed, notably at a New York symposium in 1926 (van Waterschoot van der Gracht, 1928), and rejected by such luminaries as the British geophysicist Harold Jeffreys and the Americans, structural geologist Bailey Willis and paleontologist George Gaylord Simpson.

In 1937, du Toit dedicated his book, *Our Wandering Continents: An Hypothesis of Continental Drifting*, to the memory of Alfred Wegener "for his distinguished services in connection with the geological interpretation of our Earth." In his preface, du Toit emphasized that, whether or not his explanations for continental drift were valid, he felt that "a great and fundamental truth is embodied in this revolutionary hypothesis" (du Toit, 1937, p. vii). A few years later, in 1944, Arthur Holmes offered a concluding chapter on continental drift in his *Principles of Physical Geology*, a text that served British geomorphology exceedingly well for many years. That chapter's final section, on the search for a mechanism, reviewed the problem that had long confounded those who wished to believe in continental drift and, while presenting his own convection current hypothesis, he admitted "that purely speculative ideas of this kind, specially invented to match the requirements, can have no scientific value until they acquire support from independent evidence" (Holmes, 1944, p. 508).

However, most other geomorphology texts of the period either summarily dismissed or ignored the continental drift hypothesis in their explanations of first-order relief features. In 1939, one sentence in A.K. Lobeck's text *Geomorphology* (Lobeck, 1939) was sufficient to present and dismiss Wegener's concept as unsubstantiated. In the same year, Philip Worcester's *Textbook of Geomorphology* offered three brief paragraphs on continental drift as one of several hypotheses that had been invoked to explain the origin of first-order relief. He concluded that "There are perplexing problems of geology, paleontology and climatology that yield rather readily to the hypothesis of continental drift. However, the hypothesis violates many geologic principles that have been established during the last century" (Worcester, 1939, p. 22). In 1942, O.D. von Engeln's *Geomorphology* offered a single paragraph on continental drift, augmented by two illustrations from du Toit's book. While recognizing the need for an acceptable mechanism, he did suggest that if present lateral displacement of continents could be demonstrated then "drifting in the past could be reasonably inferred" (von Engeln, 1942, p. 31). Despite some ambivalence, he concluded that

"Relief features of the first order appear to have a high degree of permanence and to be the product of forces acting unremittingly in the same direction over extremely long periods of time" (von Engeln, 1942, p. 36). Even as late as 1969, when the 9th edition of Fritz Machatschek's *Geomorphology*, a standard text in Germany for more than a generation (Machatschek, 1969), was translated posthumously into English, Wegener's hypothesis was noted respectfully but arguments for and against it were not discussed.

Some 40 years after Wegener conceived of continental drift, the concept was revived and extended during two remarkable decades of geophysical, oceanographic, and paleomagnetic research between 1950 and 1970 (Fig. 1). The tale has been much told and will not be repeated here (e.g., Hallam, 1973). In short, the intellectual climate, exhausted, rearranged, and stimulated by World War II, now found evidence to vindicate the essence of Wegener's mobilist theory, and subsequent decades have seen a massive expansion of research in global tectonics and its inclusion, to a greater or lesser extent, in modern geomorphology texts. In the English language, although absent from the 2nd edition of William D. Thornbury's *Principles of Geomorphology* (1969) and from Robert V. Ruhe's *Geomorphology* (Ruhe, 1975), plate-tectonic concepts were incorporated by H.F. Garner into *The Origin of Landscapes* (Garner, 1974), and, depending on a book's emphasis, are treated in most contemporary texts (e.g., Ritter et al., 2002; Bloom, 1998; Summerfield, 1999).

2.6. Davisian theory

Whereas many bold theories have struggled for acceptance by a skeptical Earth Science community, the concept produced by William Morris Davis regarding the cyclic response of landforms achieved rapid and widespread, if not universal, acclamation (Fig. 1). In essence, the Davisian model, presented in many papers beginning in 1884 (Davis, 1884, 1899), explained landforms in terms of structure, process, and stage. It assumed rapid uplift followed by prolonged structural quiescence during which geomorphic processes, assumed rather than measured, denuded the landscape over a time interval that was equated with life in terms of youth, maturity, and old age. This was an evolutionary model that placed emphasis on inevitable, continuous, and irreversible processes of change through time although, over time, a new cycle could be initiated by renewed structural uplift or climate change. If one accepted the basic premise, the Davisian model was alarmingly simple, couched in terms which most students could readily understand. Furthermore, his professorial appointment at Harvard University, his founding role in the Association of American Geographers, and his visiting appointments and travels overseas, provided Davis with platforms from which to proselytize, to project his boundless enthusiasm and strong will (Chorley et al., 1973).

As a consequence, the Davisian model became exceedingly popular and generated three generations of disciples whose influence pervaded geomorphology and its texts during the first half of the twentieth century, and often beyond. The works by Worcester (1939), Lobeck (1939), and Von Engeln (1942), noted above, were a triad of influential texts by true believers. Furthermore, whereas the cyclic model focused initially on so-called 'normal' or fluvial landscapes, it was sooner or later extended into such disparate landscapes as coasts (Johnson, 1919), karst (Sanders, 1921, after Cvijic), and periglacial environments (Peltier, 1950).

The anglophone world beyond North America was similarly enamored with the Davisian model, notably in Britain where a generation of students labored on denudation chronologies under the influence of Wooldridge and Linton (1939, 1955), and in New Zealand where Charles Cotton (1885–1970) applied the system, surprisingly, to an area of tectonic instability (Cotton, 1922).

Beyond anglophone audiences, Davisian ideas received less adulation, even hostility in Germany from such worthies as Passarge, Hettner, and Davis' erstwhile friend Albrecht Penck and his son Walther (1888–1923), but variations on the theme long dominated and constrained the practice of geomorphology in Europe. In France, for example, such variations were reflected in works ranging from those of de Martonne (1909) to those of Birot (1960).

Apart from German opposition, much of which came to focus on Penckian alternatives, major cracks in the Davisian paradigm began to appear among anglophone followers towards the middle of the 20th century. Some, such as Kirk Bryan (1940), who wrote critically of the mild intoxication of Davis' limpid prose, may never have been convinced, while the resurrection of mechanics and equilibrium concepts in the process studies of Leopold and others (e.g., Leopold and Maddock, 1953) and the landscape interpretations of Hack (1960), the introduction of more quantitative approaches to geomorphology by Horton (1945) and Strahler (1952, 1954), and improved understanding of Earth time, all sounded the death knell of the Davisian model that was so firmly rung by Chorley et al. (1964, 1973). Leighly had earlier emphasized a critical weakness in the Davisian method by stating that "Davis's great mistake was the assumption that we knew the processes involved in the development of land forms, We don't; and until we do we shall be ignorant of the general course of their development" (Leighly, 1940, p. 225). Much the same could be said about Davis' understanding of tectonics. Even so, works in the Davisian mold continued to appear, for example Small's *Study of Landforms* (1972), but most modern texts, reflecting current understanding of tectonics and process, now place the cycle of erosion in its historical context or at most as an end member in a spectrum of possible landforming scenarios (Ritter et al., 2002; Bloom, 1998; Summerfield, 1999).

2.7. Recent theory and practice: the case of drainage-network analysis

Recent events are always more difficult to place in an historical context, essentially because they are relatively new and cannot be viewed with the objective retrospection that comes with time. Nor is it the purpose of this paper to provide such an assessment. However, the conditions that affected the fate of earlier theories have continued to flourish over the recent past. Modern students might reasonably ask why, with many statistical procedures already provided before 1900 by Francis Galton, Karl Pearson, and others, the introduction of quantitative methods to geomorphology was so long delayed (Fig. 1). The explanation for this lag can only be partly attributed to the Davisian school because there were other conservative forces at work. Morisawa (1988), discussing the role of the *Geological Society of America Bulletin* in fostering quantitative geomorphology, could have added that Robert Horton's innovative approach to drainage-basin analysis languished for a decade or more in search of a wider audience before its eventual publication in 1945.

The growth of a more quantitative geomorphology in the 1950s was due in part to Arthur Strahler's refinement of Horton's approach to drainage networks and the former's encouragement of rigorous statistical testing among his students at Columbia University, students who in turn carried the message far and wide (Strahler, 1950, 1952, 1954, 1980). But, again, the study of network-ordering systems was by no means new. For example, Woldenberg (1997) has shown how James Keill, M.D. (1673–1719) introduced geometric progression scaling laws into his work on the anatomy and physiology of arterial trees as early as 1708. Keill's specific contributions to arterial networks were known to James Hutton, who had also studied medicine at Leiden, and who compared river networks to venous trees, which return blood to the heart. There had been other stream-ordering systems, notably those of Jackson (1834) and Gravelius (1914), but not until the publication of Horton's paper in 1945 and its subsequent refinement by Strahler were such ideas incorporated into modern geomorphology (Goudie, 1978; Jarvis and Woldenberg, 1984; Woldenberg, 1997).

Later, however, enthusiasm for analyzing drainage networks waned, at least temporarily, after Shreve (1966, 1967) pointed out that the so-called 'laws' developed by Horton, Strahler, and others were only to be expected from topologically random distributions. Shreve's random topology model was in turn challenged, for example by Abrahams and Mark (1986), and the field of network analysis experienced a resurgence. In recent years, analytical concepts pioneered by geomorphologists have been espoused by anatomists, physicists, engineers, and others concerned with fractal trees and other organizational attributes of natural and artificial systems, both deterministic and random, and stream ordering systems continue to be used for ranking purposes by watershed specialists (e.g., Rodriguez-Iturbe and Rinaldo, 1997).

The fluctuating fortunes of drainage network analysis are reflected in geomorphology texts published in English subsequent to the initial work of Horton and the Strahler school. The path-breaking book by Leopold et al. (1964) devoted an entire chapter to drainage basin morphometry, but later editions of existing texts ignored the concept. Thus, Thornbury's 2nd edition *of Principles of Geomorphology* (Thornbury, 1969), while discussing Horton's overland flow model and stream texture and Strahler's earlier work, makes no reference to network analysis. The English translation of Machatschek's (1969) *Geomorphology* also remained silent on the issue, although this was no reflection on the original author who had died in 1957. In contrast, in new texts published within the next few years, Small (1972) recognized the utility of morphometric techniques, Garner integrated Hortonian concepts into his text (1974), and Ruhe (1975) devoted an entire chapter to drainage nets and basins, including the so-called 'laws', but did not mention Shreve's work. The first editions of most contemporary texts also provided measured evaluations of the analytical techniques introduced by Horton, Strahler, and Shreve (e.g., Ritter, 1978; Bloom, 1978; Summerfield, 1991), and these have continued in recent editions (Ritter et al., 2002; Bloom, 1998; Summerfield, 1999).

3. Lessons learned

In the several examples presented above, the fate of new ideas and fresh research in a broader educational milieu appears to reflect a number of constraints and opportunities indicative of the intellectual climate of the time. The lag time between new concepts and their incorporation into the mainstream educational process is conditioned by such variables as establishment response, simplicity and testability of ideas, language barriers, personalities, and survivability. From this record, a number of lessons may be learned that may prepare present and future generations of scholars in their quest for the acceptance of new ideas.

3.1. Beware the establishment!

The history of the Earth Sciences shows how many innovative ideas have been rejected by the academic environment of the time, itself commonly a reflection of the contemporary religious, political, and social milieu. Uniformitarianism had a prolonged struggle against the entrenched catastrophism of the late 18th and early 19th centuries. Continental drift was eventually rejected by the geological establishment of the earlier 20th century. Conversely, despite inherent weaknesses, Davis' cyclic erosion scheme had prolonged success in anglophone circles of the early 20th century, just as the climatic geomorphologies of Julius Büdel and Jean Tricart had a lengthy run in mid-20th century Europe, essentially because they became the establishment. Likewise, the glacial theory gained credibility because Agassiz had excellent links to the establishment in Europe and later patronage in North America. For similar reasons, Wegener's ideas on continental drift were given a fair hearing in central Europe, though often ridiculed in North America.

Such examples raise the issue of the establishment's responsibility to foster alternative dialogues, rather than to retreat into the security of accepted doctrine. In a modern context, the establishment can be interpreted to include not only senior academicians but also journal editors, funding agencies, principal investigators, and faculty review committees. It is incumbent upon learned societies and scientific journals to foster dialogue between opposing views (e.g., Birkeland, 1998, on Schaffer) and to maintain an open mind at the research frontier (unlike the *American Journal of Science* in its early years). For example, the symposia staged by the American Association of Petroleum Geologists in 1926 (van Waterschoot van der Gracht, 1928) and the Royal Society in 1964 (Blackett et al., 1965) represent opposite ends of the spectrum in the debate on continental drift, but these occurred 38 years apart. And funding agencies must of course sponsor alternative approaches to perplexing problems and searching tests of conflicting hypotheses.

A feature of modern geomorphology, with its penchant for research teams funded by the establishment, is the tendency for most students to replicate the work of their *magistri*, in part because they are beholding to advisors and their funding agencies for support. This may create a comfortable milieu for the student but it presents the danger of stifling originality, of assigning a student to a single piece in a jigsaw puzzle of broader knowledge. Advisors and principal investigators have a responsibility to ensure an independent spirit of enquiry among their students. Despite the increasing trend towards team research in geomorphology, there must always be opportunities for individual thinkers freely to express their ideas.

3.2. Keep it simple but test it!

The success of the Davisian model was due in part to its compelling simplicity, in part to its evolutionary implications that fit well into contemporary Darwinian notions of organization in nature and progressive change through time (Stoddart, 1966). Such was not the case with 18th century concepts of uniform flow and slope stability which were given cumbrous mathematical expressions unlikely to be used by any but the practicing engineer. Nor was it true of the quantitative revolution that invaded geomorphology in the 1950s, into a community unprepared for the language of statistics. As late as 1969, William Thornbury in the 2nd edition of his *Principles of Geomorphology*, a firm favorite with instructors over two decades, admitted that:

> "Some readers may be disappointed that there is no treatment of Quantitative Geomorphology. The main reason for this omission is that I did not feel competent to do justice to it" (Thornbury, 1969, p. vii).

This was a disarmingly honest admission but, perhaps needless to say, the book soon faded from the educational scene.

While simplicity is to be commended, new ideas must be testable. The Davisian model may have been intuitively compelling but it was untestable in the prevailing scientific climate of the earlier 20th century, essentially because there were no precise techniques available to gauge the absolute timing and frequency of tectonic uplift and denudational processes. Peneplain seekers and denudation chronologists of the time thus were free to speculate on the model's application to their regions. However, as radiometric techniques matured after mid-century, so the limitations of the model were soon revealed. Likewise, lacking accurate chronologies and a full understanding of weathering rates and climate change, climatic geomorphologists could readily speculate on long-term relationships between climate and landforms.

3.3. Read widely and think originally!

At the present time, when there are so many practicing geomorphologists, it is instructive to note that in the past many new ideas emerged from individuals who were reading widely and thinking deeply at the margins of conventional wisdom. At best, uniformitarianism and glacial theory were marginal to mainstream scientific thought of their time (Imbrie and Imbrie, 1979; Orme, 1989). Though long ignored, Gilbert's ideas were firmly based on his wide reading of contemporary advances in classical mechanics, especially thermodynamics (Chorley and Beckinsale, 1980). More recently, in the 1950s and 1960s, systems theory produced a flurry of intellectual activity not because the concept was new—it was firmly based in the thermodynamics of the previous century—but because astute thinkers, such as Arthur Strahler and Richard Chorley, recognized its relevance to geomorphology (Strahler, 1950, 1980; Chorley, 1962; Chorley and Kennedy, 1971).

Until quite recently, the emergence of national schools of geomorphological thought was linked to limitations imposed by language and communication barriers. Such schools developed not so much because of intrinsic merit or overt nationalism, but because scientists in a particular country were more comfortable with their own language and environment, and less familiar with work in other countries and other languages. That so much seminal work germane to fluvial geomorphology was published in Italian, German, and French during the 18th century goes far towards explaining its tardy acceptance in the English-speaking arena. In the Europe of the earlier 20th century, distinctive British, French, German, Polish, and Russian schools of geomorphology functioned more or less in isolation, dominated by strong personalities whose ideas were not well understood beyond national borders. Even within the established anglophone world of Australia, Britain, New Zealand, North America, and South Africa, where language was not a barrier, distinctive schools of geomorphology emerged, in part because of different environmental challenges, in part because ideas developed in relative isolation. This is not to say that there were no exchanges across language barriers or between continents—Davis' ideas were known in Europe from his own efforts and those of translators (e.g., Davis, 1912), just as Penck's views became better known to anglophone audiences long after his death in 1923 (e.g., Penck, 1953). But imperfect translations and rare appearances mitigate against ready acceptance of ideas, especially where intellectual environments are innately skeptical of foreign notions.

The situation has of course changed rapidly over recent decades, as travel has become easier, electronic communication has exploded, and English has become the lingua franca of so much scientific discourse. Nevertheless, for geomorphology to prosper in the future, an understanding of ideas published in other languages and other places remains an essential ingredient of the field. It is salutary to note that Gilbert's non-linear view of geomorphology, long ignored at home, appealed to some of his contemporaries overseas, notably Philippson in Germany, and de la Noë and de Margerie in France (Chorley and Beckinsale, 1980).

3.4. Live long and prosper!

Max Planck's much-quoted aphorism may be applied to geomorphology, namely that "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Planck, 1949). Uniformitarianism and glacial theory eventually succeeded because their most strident opponents passed from the scene and young scientists were more receptive, although the lag time was often considerable—Charles Lyell, the high priest of uniformitarianism, was born in 1797, the year in which James Hutton died. Longevity and the strength to last the course are among the important attributes of the sponsors of new ideas. Walther Penck, who died at the early age of 35 in 1923, and Wegener who died on his third Greenland expedition in 1930, were not so blessed.

Another feature of human nature is that youthful innovators often become conservative dogmatists with age. For example, despite his early espousal of Huttonian concepts, Lyell's inflexibility on uniformitarian issues caused him to carry wavering support for the iceberg origin of glacial deposits to his death in 1875. Harold Jeffreys, a youthful pioneer of geophysics, remained strongly opposed to Wegener's mobilist notions. And opposition to a more quantitative geomorphology in the mid-20th century was led by aging establishment figures who didn't understand and didn't want to know. Eventually, however, resistance fades from the scene, new personalities emerge, and new ideas take root among students who are often unaware of previous struggles.

4. Conclusion

Returning to the premise of geomorphology's twin foundations, it is evident that the historical approach eventually prospered in the educational arena because it was a logical extension of the evolutionary thinking that had dominated science and intrigued the general public for more than a century. Hutton and Playfair influenced Lyell, Lyell influenced Darwin, and Darwin influenced Davis who in turn spawned three generations of disciples. Paraphrasing Greene (1982), each of these individuals served as a torchbearer in an historical relay race. Inevitably perhaps, the geomorphology it fostered, essentially a study of the history of landforms based on a preconceived notion, became sterile. However, with the advent of improved dating techniques and a better appreciation of geomorphic processes, the historical approach was refashioned during the later 20th century into comprehensive models of landscape change which incorporate both tectonic and climatic forcing (e.g., Bloom, 1998; Orme, 2002).

In contrast, during the prolonged ascendancy of the historical method, the mechanistic approach seemed to have no relevance to the discipline. Scientists, fervently pursuing the details of Earth history, mostly failed to apply concepts of mechanics to the interpretation of surface processes and landforms. This was due in large measure to the origin of such concepts among practical engineers and, in the anglophone world, by their primary availability mainly in foreign languages (Orme, 1989). That such concepts re-emerged during the later 20th century, and have since been integrated into mainstream geomorphology, reflected the links that were eventually forged with the hydraulics texts and practices of earlier times. At the same time, as Ritter (1988) has emphasized, a reawakening of interest in Gilbert's concepts of dynamic equilibrium, borrowed from classical thermodynamics, was reflected in Hack's non-cyclic interpretation of Appalachian landscapes (Hack, 1960) and in re-evaluation of the time factor in geomorphology (e.g., Schumm and Lichty, 1965).

The foregoing paper has sought to address, by example, some of the questions raised in the opening paragraphs concerning the fate of geomorphological ideas in an educational context. The path to leadership in the field is indeed a rocky one. Many eventual masters were ignored, and some ridiculed, during their lifetime; many did not live to see their ideas vindicated or incorporated into mainstream education; and there are presumably future leaders ('master' is anachronistic) who are currently striving for recognition of ideas for which the establishment is unprepared. Furthermore, while scientists argue at length about theory, engineers have been applying basic principles to practical problems. Therein lies the particular challenge for present and future generations of geomorphologists, namely, to combine modern theory with useful practice in the real world of environmental management and public policy.

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