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Reflections on the fate of some geomorphological ideas

Reflexiones sobre el destino de algunas ideas geomorfológicas

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ABSTRACT

In geomorphology, as in other sciences, investigation is concerned with the collection and characterisation of data, and the generation and testing of working hypotheses. Considering the analysis of landforms and landscapes, the reasons some explanations have been accepted, others rejected, and yet others refuted but later approved, are examined. In particular, why hypotheses which were considered plausible but were shown to be flawed still received general acclaim, whereas others of obvious merit were ignored, are discussed. The roles of chance and the human factor are also broached.

Key words: acceptance/rejection of ideas; plausibility; human factor; fashion in geomorphology; concepts considered: age of landscape, Channeled Scabland, desert dunes, insolation weathering, lunettes, river velocity, sheet structure, submarine canyons

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INTRODUCTION

Einstein once stated that it is not intellect but character which makes a great scientist. Certainly, high intelligence is no guarantee of moral worth, whereas tenacity, integrity and moral courage are characteristic of much of the best scientific research. Parga Pondal, whom we honour with this volume, displayed such qualities not only in his geological career but also in the much more dangerous arena of life.

In research it is frequently necessary to persist with an idea in the face of ridicule, obloquy and professional ostracism. One may eventually be shown to be in error in whole or in part, but once convinced of an interpretation it is obligatory to defend it vigorously but professionally, tenaciously but not stubbornly, and always with an open mind. Yet to win acceptance for an idea is not always easy and not always based in logic. Contemplating interpretations of landscape of bygone ages one is adversely surprised by the persistence of untenable ideas, yet also pleased with the certain knowledge that there have always been rational beings. Thus, on the one hand, despite the incontrovertible evidence to the contrary, including pictures from space, there are still those who believe in a flat Earth. Again, there are those, a few, who, genuinely believe the biblical account of the age of the Earth, who, despite all the evidence, regard reports of Moon landings as propaganda lies, while complacently accepting biblical allegories as literal truths. On the other hand, two millenia or so ago, in classical Greek and Roman times, there were those who not only argued from observations that the Earth is roughly spheroidal, but who, using this as a basis, had worked out how to measure the size of the planet on which we live.

The ever changing interpretation of various landscape elements well illustrates POPPER's (e.g. 1963) assertion that no scientific theory may ever be regarded as proven, if only because, as CRICK (1988) has pointed out, any theory which fits all the facts must be flawed because some of those facts will prove to be misleading. Also as Kant pointed out two centuries ago, truth is reality as perceived by human eyes and interpreted by human minds; and the interpetation of data, especially field data, is a personal matter, varying with the observer's character, training and experience (READ, 1957, p. i). But not all rational hypotheses find ready acceptance, and the more radical or unconventional the idea the more difficult it has proved to have it aired. Yet other concepts, clearly questionable, immediately receive the status of a theory. What factors influence the reception accorded an interpretation or concept perceived to be new?

RATIONAL BUT FLAWED

Many hypotheses and assertions were plausible in light of the known data at the time they were derived, but were overtaken by new data and either modified or abandoned.

Plausible but placed in perspective

Insolation has long been cited as a cause of rock disintegration. In ancient India and Egypt, the expansion of rock consequent on heating was used as an aid in quarrying. The rock surface was cleared of soil and other overburden and a bonfire lit on the bare rock surface. The heat of the fire caused the near surface rock to expand and arch, making it easier to break the mass into manageable blocks (e.g. WARTH, 1895). The heat of bushfires and that generated in nuclear explosions also causes rocks to flake (e.g. WATANABE *et al.*, 1954). Little wonder that rock weathering has been attributed to heating and cooling.

Two arguments have been put. First, many rocks (e.g. granite) consist of minerals of different composition and colour, which absorb radiation differentially and therefore heat and expand at different rates. At night or in winter they cool. After many alternations of such heating and cooling, diurnal and seasonal, the rock breaks down into fragments: granular disintegration. Second, rocks are poor conductors of heat, so that while rock at and near the surface heats and expands, that just below does not, with the result that eventually the surface layer splits away: flaking or spalling, depending on scale, and more generally, 'exfoliation'.

Such reasoning is convincing, and flakes and slabs of rock as thick as those involved in A-tents or pop-ups, and even those referred to as sheet structure, have been attributed to insolation (e.g. SHALER, 1869; MacMAHON, 1893; SCOTT, 1897, p. 225; PIRSSON & SCHUCHERT, 1915, p. 20; TYRRELL, 1928). Yet, at best, such insolational weathering is of minor importance in nature. The heat generated in fires and explosions is short-lived and localised. Attempts to simulate disintegration and spalling in the laboratory have failed, possibly because the time factor cannot be reproduced, though also because the experiments were carried out in dry conditions (see GRIGGS, 1936). The critical observations placing insolation in perspective are due to BARTON (1916).

Barton visited some of the pharaonic monuments in Egypt. Some had fallen on to the desert sands, others into the silts of the Nile valley. In both situations, Barton noted that the upper faces of blocks and columns, which when used to construct the various temples, etc. had been freshly quarried and which had been exposed to the Sun's radiation for 3-4000 years, showed no detectable sign of disintegration or decay. On the undersides, on which dew had formed from time to time, on the blocks buried in desert sand, and especially those buried in flood plain silt, there was clear evidence of alteration.

Such observations do not imply that insolation does not take place, but they show that it acts much more slowly than does moisture attack, even in deserts where insolational heating is maximal and moisture is scarce. Common rock-forming minerals readily react with moisture and in the humid tropics, where the prevalent humidity and high temperatures are conducive to chemical activity, alteration takes place very quickly. CAILLÈRE & HENIN (1950) have shown that in Madagascar, mica in a newly quarried crystalline rock shows signs of decay within a few years, and examination of a granite quarried and used in a building in Rio de Janeiro again revealed evidence of alteration within a decade or so. Feldspars in dated volcanic ash and lava are altered in 10^3 years in the humid tropics, a soil a metre thick has been shown to develop on a dacite in about 5000 years, and ferromagnesian minerals, especially, decay very rapidly in such conditions (e.g. WEYL, 1954; HAY, 1960; TRESCASES, 1975) and detectable rapid rates of weathering are evidenced elsewhere (e.g. BIRKELAND, 1974; THOMAS, 1994). Though the Nile is an exoreic stream flowing through an arid region, in the flood plain the combination of moisture and heat in some measure simulate the humid tropics. Barton's simple but astute observations placed insolational weathering in perspective.

Plausible but incorrect

River velocity provides a splendid example of an apparently logical explanation that has perforce been abandoned. Though, thanks to the efforts of HUTTON (1788, 1795) and GREENWOOD (1859) it was recognised early in the history of geomorphology that the vast majority of valleys have been eroded by the rivers that flow in them, other early ideas of river activity and their consequences were logical, but in error.

Surveys show that the longitudinal profiles of rivers most commonly describe concave-upward courses. Gradients are steepest in the headwater reaches, and become increasingly gentle as the sea is approached. It was not unreasonably assumed that velocity would be greater on steeper than on gentle slopes, and it was concluded that, overall, velocity would decrease from source to sea. This is consistent with the observational evidence of rushing headwater streams in the mountains and apparently sluggish meandering rivers on the plains. Explanations of many river characteristics and associated landforms were based in this concept of downstream decrease in velocity. For example, the deposition of debris in flood plains was said to be due to low velocity rivers not having the energy to transport the available load. The absence of coarse detritus in plains sectors of major rivers like the Congo and Amazon was attributed to there being insufficient energy to carry blocks and boulders.

Velocity is taken as the maximum flow developed (and demonstrated by measurements) in the deepest part of the channel, and about one third of the way down between the water surface and stream bed. There are, as expected, local departures from this generalisation, as for example on river curves where velocity is greater on the outside than on the inside of the bend, and where the river passes through a defile or narrows. The discharge or volume of a stream (Q) is a function of cross section area of channel (A) and velocity (V): $Q=A \times V$. Water cannot be compressed so that a narrowing of channels is accompanied by an increase in velocity; and vice versa. But such variations are local and the overall picture stands.

The concept of downstream decrease in river velocity was based in reasonable deduction. But it stood only until the later '50s when the results of measurements of stream velocities became known. Stream gauges were set up at intervals along various rivers and recordings continued over considerable periods. Contrary to expectations, apart from a short sector in the headwater region, river velocity increases downstream (LEOPOLD, 1953). But how can a river flow more rapidly on gentler than on steeper gradients? The explanation appears to involve channel characteristics, and in particular the ratio of cross-section area to wetted perimeter. In the headwater reaches of a stream, the channel is eroded in bedrock, with many irregularities, and many cobbles, blocks and boulders resting on the stream bed. Thus the wetted perimeter, or length of rock surface in contact with water, is long in relation to the cross-section area of the channel. Downstream, however, on the plains, the discharge is higher and the cross-section area greater; moreover the river channel is in some measure at least, eroded in alluvium, with few irregularities. Thus the ratio of wetted perimeter to cross-section area is low. As some 95% of the potential kinetic energy of rivers is dissipated in frictional losses at the contact of flowing water with bed and banks (RUBEY, 1952), there is a much greater loss of energy in the headwaters than in the plains sectors. Moreover, in rocky mountain channels a great deal of energy is lost as a result of turbulence and eddies caused by obstacles and irregularities.

[Incidentally, such frictional loss of energy is the basis of a method, derived not from hydraulic theory but from trial and error, commonly used by farmers to impede or repair gully erosion. Dumping many large blocks and boulders in the channel, first, increases the wetted perimeter and thus reduces stream energy and velocity, and, second, provides the basis of a crude dam behind which sediment is trapped and the channel aggraded.]

It says much for the ingenuity of geomorphologists that the same features that were explained in terms of downstream diminution of flow are just as plausibly interpreted within the new framework of stream activity. Many flood plains, it is now realised, are the result of point bar deposition during lateral planation by rivers (see e.g. WOLMAN & LEOPOLD, 1957; LEOPOLD *et al.*, 1964) and the character of load in plains streams reflects the weathering and attrition of debris and the consequent non-availability or supply of coarse fragments. Such intellectual agility also highlights the fact that Earth scientists in general, and geomorphologists in particular, are good at explaining what has happened, but not so impressive when endeavouring to predict events.

Plausible but questionable

Some interpretations are plausible but owe something to the reputation or distinction of the proposer, as well as to its inherent qualities. The offloading hypothesis of sheet fractures provides such an example. Arcuate fractures are characteristic of massive rocks such as granite, and have long been noted in the literature. Though some early writers (e.g. MERRILL, 1897; DALE, 1923; BAIN, 1931) attributed them to torsional and compressional stresses, they have for many years been interpreted as due to offloading or pressure release consequent on erosional unloading; so much so that in text books of geology and geomorphology, as well as in research papers, they are commonly referred to as offloading or pressure release joints, or some similar term. The theory behind this nomenclature is due to GILBERT (1904) and is compelling.

Imagine a mass of granite exposed at the surface. That it is exposed implies the erosion of several kilometres of superincumbent rocks, for granite is emplaced deep in the crust in an environment characterised by high ambient temperatures and lithostatic (hydrostatic) pressures. The granite cooled at depth through heat loss to the surrounding rock, but as the overlying crust thinned through erosion, pressures diminished and the rock tended to expand radially, normal with the cooling surface, i.e. essentially the land surface. Many rock masses tend to be rounded by weathering, and particularly by moisture attack in the subsurface. It was proposed that expansive or tensional radial stresses would be manifested in sets of fractures disposed tangential to the stresses and parallel to the land surface, and that these are the well-known offloading joints. So persuasive is this explanation that other possible interpretations of the fractures are pre-empted by their being widely referred to as offloading joints, etc. That sets of sheeting fractures in some places parallel recently eroded surfaces, such as cirque headwalls and valley side slopes, is cited as evidence of rupture due to erosional offloading (e.g. LEWIS, 1954; KIERSCH, 1964).

Whether joints or faults, all fractures are a manifestation of pressure release in the sense that they presumably disappear in depth as a result of high lithostatic pressures, but much evidence and argument is inconsistent with pressure release, and can best be explained in terms of a compressive environment (VIDAL ROMANI *et al*., 1995; TWIDALE *et al*., 1996). For example, at several sites sheet fractures describe synforms within domical hills, a relationship impossible of explanation by offloading. Sheet fractures are typical of bornhardts or domical hills, many of which are lithologically identical with the rock beneath the surrounding plains, and which appear to be developed in rock characterised by low fracture density, presumably as a result of compressive stress. The development of fracture sets parallel to recently formed surfaces can be understood in terms of realignment of compressive strain in parallel to the changing geometry of the land surface. Fractures analogous to sheet fractures can be produced experimentally by squeezing partly confined blocks (HOLZHAUSEN, 1989). And so on - there is much evidence to support the suggestion that sheeting is associated with compression, and much field evidence incompatible with the pressure release hypothesis. Gilbert himself recognised that sheeting in the eastern United States was possibly related to compressive environments (DALE, 1923, p. 29).

[Yet when as a young man in the mid '60s and visiting a university not far from Dartmoor, I suggested to my host professor that I thought the sheet structures I had observed on nearby granite massifs could not satisfactorily be explained in terms of erosional offloading, I was ridiculed: 'What on earth are you thinking of - of course offloading joints are due to offloading!'; and referees reviewing (and recommending rejection of) a paper presenting a similar conclusion in the early '90s offered the same circular argument.]

MENTAL BARRIERS

Some rational explanations based in field evidence have not been accepted because of an inability or refusal to accept the magnitude of natural forces and the infinite immensity of time.

An Ussherian view of landforms: the antiquity of landforms

Consider for example the question of palaeosurfaces. James Hutton lived two centuries ago and he is widely regarded as the father of modern geology. It has been said that he '... discovered time' (EISELEY, 1961 p. 65). Amongst his many astute and penetrating visions of the Earth was the grand design whereby the destruction of old landscapes provided the materials for the construction of the new. He saw this recycling process as having operated in all past and future time, and he concluded with the famous assertion that he could see in the landscape no vestige of a beginning and no prospect of an end (HUTTON, 1788, 1795). Another of Hutton's great themes revolved around the constant wearing away of the land surface, and many later workers, either tacitly or explicitly, made the same point, with the implication that the Earth's surface is mostly very young, and with the exception of exhumed forms, nowhere older than Tertiary, i.e. nowhere more than about 60 millions of years old (WOOLDRIDGE, 1951, p. 23; LINTON, 1957; THORNBURY, 1954; BROWN, 1980). This theme of constant change finds clear expression in the various models of landscape evolution applied to the analysis of landscape.

W.M. Davis, for example, is well known for his concept of the geographical (or, more properly, the geomorphological) cycle, concerned with the evolution of landscapes through time and with the development of the peneplain (DAVIS, 1899). According to Davis, rivers and streams in the form of wash and rills extend to every last part of the land surface:

'*a river is ... a moving mixture of water and waste in variable proportions, but mostly water; ... one may fairly extend the "river" over all its basin and up to its very divides. Ordinarily treated, the river is like the veins of a leaf; broadly viewed it is like the entire leaf.*' (DAVIS, 1909, p. 267)

The implication is that the entire land surface undergoes constant change, erosion on hills and higher plains, and as a corollary, deposition in valleys; but all is changed in some degree or other, and no landscape can long survive attack by external agencies. Other models of landscape evolution, such as scarp retreat (KING, 1942), or steady state development (HACK & GOODLETT, 1960; HACK, 1960), both of which are applicable in particular structural settings, carried the same inbuilt age limitation. Even in favourable circumstances no surface ought in geologic or stratigraphic terms to be older than Oligocene, i.e. no more than some 30-35 millions of years old (see TWIDALE, 1976).

Yet for the past 70 years various investigators have been driven by stratigraphic and topographic evidence to the conclusion that facets of the contemporary landscape date back 70-100 million years or more, to the later Mesozoic (HOSSFELD, 1926; CRAFT, 1932; HILLS, 1934). Subsequent work has substantiated the suggestions of these pioneer workers, though the extent of old palaeosurfaces and the certainty attaching to their great ages has been made possible, on the one hand by advances in regional stratigraphy and on the other by radiometric dating. For example, the summit surface of the Gawler Ranges is an Early Cretaceous feature (some 120-130 millions of years old; CAMPBELL & TWIDALE, 1991). That of the Hamersley Ranges is of Eocene age and at least 60 millions of years old, the ribbed crestal bevel of Ayers Rock is at least Maastrichtian in age, and Kakadu (Arnhem Land) and the Arcoona Plateau were in existence in the Early Cretaceous (TWIDALE, 1994). The ancestors of many modern rivers can be traced back far into geological history, in some instances at least 120 million years and in others a mere 60 million (TWIDALE, 1997). Remnants of ancient landscapes are preserved in other parts of the world (e.g. KING, 1942, 1950; MICHEL, 1978; DEMANGEOT, 1978; BRICEÑO & SCHUBERT, 1990; TWIDALE & VIDAL ROMANI, 1994; TWIDALE, 1994). Hutton’s concept of constant change and Davis' of ubiquitous erosion are not universally applicable.

Concurrently with the great age of some landscape elements being established, circumstances conducive to their persistence were also noted (e.g. KNOPF, 1924; CRICKMAY, 1932, 1974, 1976; HORTON, 1945; TWIDALE, 1976). Certainly, and contrary to the Davisian teaching cited earlier, by the mid 'thirties there were suspicions that erosion was not everywhere equally distributed over the land surface. Concerned to explain the survival of bevels high in the Appalachian topography, KNOPF (1924, p. 637) described some high level remnants as 'out of reach of erosion', implying that erosion, in this instance by rivers and streams, was not ubiquitous even at the local scale. That divides are relatively immune to erosion find support in HORTON's (1945) concept of a headwater zone of nil erosion. Meanwhile, however, Knopf’s implication of unequal activity had been taken up and developed by CRICKMAY (1932, 1976) and later by TWIDALE (1991). The concept of unequal activity focuses on the ubiquity and effectiveness of water as an agent of weathering, and on rivers as reinforcement or positive feedback systems. If, for whatever reasons, rivers erode their channels but there is little or no lowering of divides, then the interfluves will be preserved, particularly if the scarps defining the uplands are also essentially stable.

It might be thought that the advent of physical or numerical dating might resolve such difficulties: not at all. A dramatic example concerns the revision due to radiometric dating of the landscapes of Kangaroo Island, in South Australia. SPRIGG *et al*. (1954) established that the lateritised surface predated basalts extruded near the present northeast coast. At the time they worked there were few radiometric dates, and they reasonably linked the basalts to the stratigraphically dated later Cainozoic volcanicity of the Mt

Gambier area and adjacent parts of western Victoria. In these terms the laterite predated the Quaternary, which appeared consistent with stratigraphically dated laterites in northern Australia. The stratigraphic relationship is correct, but the temporal placement of the chronology was drastically changed with the discovery that the basalt is Middle Jurassic, not Quaternary, in age (WELLMAN, 1971). In these terms the laterite still predates the basalt, but is younger than the Permian glacigene rocks on which it is in places developed. Regional stratigraphic considerations suggest a Triassic age for the duricrust and the surface on which it developed (DAILY *et al*., 1974). Antiquity of this order is confirmed by oxygen isotope analysis of clays from the lateritic profiles (BIRD & CHIVAS, 1988, 1993).

[When an attempt was made to publish this evidence, argument and conclusion (DAILY *et al*., 1974) it was rejected by the first journal to which the manuscript was submitted, one referee uncompromisingly stating that the conclusion was 'impossible'. However incredible it may have seemed at the time, the conclusion was surely worthy of an airing and eventually more liberal views prevailed. Unfortunately, institutional disbelief resulted in many official maps carrying legends which indicate laterites at least 150 million years too young and based in a tectonically unlikely concept for another 20 years or so.]

The survival of land surfaces for some hundreds of millions of years was and still is regarded by many as unreasonable or impossible (e.g. BOURMAN, 1989, p. 47, 1995, p. 16, to take a recent example). The concept of constant and widespread change was, after all based in observations, reasonable perceptions and common sense: how could a landscape constantly subjected to the elements not be worn down? Rational arguments were, however undermined by the development of a different way of viewing processes and landscape, though these new ideas were not given serious consideration for many years. On the contrary, both Crickmay and his concept of unequal activity, were subjected to ridicule (TWIDALE, 1993).

Quantification to the rescue

The forces of Nature are commonly underestimated. In geomorphology, the impacts of some events on the landscape were either not recognised or not believed because the investigators could not - or would not - accept the implied magnitude of the forces at work. The importance of floods in shaping the landscape was recognised early, but the possible magnitude of natural floods was not. Quantified relationships were established between stream discharge and velocity on the one hand, and such features as channel width, meander geometry and depositional features such as bars and ripples, on the other (e.g. TRICART, 1961; LEOPOLD *et al*., 1964; MALDE, 1968). Only then did the origin and significance of certain naturally-occurring riverine forms become comprehensible. The impacts of very large if localised floods, either natural (e.g. BAKER *et al*., 1988; GARCIA RUIZ *et al*., 1996) or associated with burst dams (e.g. TRICART, 1960; KIERSCH, 1964) reinforced these conclusions.

Even so, while the impacts of the largest floods did not defy comprehension, their implied magnitude was so enormous as to be unacceptable. The basaltic Columbia Plateau of eastern Washington, in the northwestern U.S.A., is occupied by the Channeled Scablands, a region characterised by interlaced broad channels, interrupted by gigantic waterfalls, with enormous bars and ripples in the channel floors and huge blocks of country rock strewn over the landscape. Various explanations have been offered for this stupendous landscape (BAKER, 1973). Suffice it to say that BRETZ (1923, 1933; BRETZ *et al*., 1956) attributed all the recent erosional and depositional features to a single catastrophic flood, but this was questioned and alternatives suggested, because a source for the implied huge quantities of water was not identified; notwithstanding that PARDEE (1910) had earlier described evidence of the glacier-dammed 'Lake Missoula'. This was the huge body of water which was released on the melting of the ice barrier (see also BAKER, 1995). The earlier systematic work relating the size of dunes, ripples, transported blocks and channels to stream discharge allowed the volumes of water involved in the shaping of the Channeled Scablands to be computed (BAKER, 1973). They are an order of magnitude greater than even the highest floods recorded on modern rivers. The erosion of immense channels and waterfalls, the deposition of huge bars and ripples, the transport and deposition of the huge blocks of basalt, are commensurate with the huge discharges implied. The reshaping of the Scabland topography was accomplished about 14,000 years ago, probably in a few hours, during catastrophic flooding due to the breaking of the glacial barrier which retained Lake Missoula.

[Bretz's initial interpretation of the Channeled Scabland was correct but it met with strong institutional resistance (see e.g. BAKER, 1978) and it was not until 50 years later, when the processes had been quantified and a source of the huge volumes of floodwaters belatedly recognised, that it was accepted. Fortunately Bretz lived long enough to see his explanation vindicated. A combination of quantified processes and stratigraphic knowledge convinced the scientific world.]

CONVERGENCE

An untenable idea or explanation is one that is widely considered not to be supported by the available evidence. But just as truth is rarely pure and never simple, so Nature is commonly complex, and it is not unusual for similar forms to evolve in different ways - they are said to be convergent.

Blinkered vision

Submarine canyons are huge chasms which resemble terrestrial gorges and score the continental slope and shelf in all parts of the world. Some are more than a kilometre deep. They have been known for many years, and various explanations have been offered for them. For example, SHEPARD (1948) argued that they are terrestrial gorges that have been drowned, but this suggestion is at odds with the evidence and has, rightly, been rejected as a general explanation. The weight of evidence supports the suggestion that they are due to turbidity currents, huge submarine rivers of sediment carrying water (DALY, 1934; KUENEN, 1950; HEEZEN & EWING, 1952; HEEZEN, 1956).

But there are exceptions. Some 6 million years ago, during the later Pliocene, what is now the Mediterranean Sea was a deep trough occupied by a desert (HSÜ, 1972). Rivers draining northward from what is now Libya poured over the edge of the African continent and eroded huge chasms, which were drowned and converted to submarine canyons when the Straits of Gibraltar were breached and the sea flooded the basin. They are typical Shepardian canyons, and Hsü records that a terrestrial origin was suggested for the Libyan features, but the interpretation was rejected as untenable and the paper never published because of the widespread general evidence favouring turbidity currents. The fact that near its mouth the Nile had excavated a gorge at least 300 m (and maybe as much as 1500 m) deep in relation to the same low baselevel provided by the desiccated Mediteranean basin, was ignored by referees and editors, as was the possibility of convergent evolution or equifinality. The probable truth was denied.

CHANCE

Chance has been described as a nickname for providence. If so, providence has been unkind to some investigators who had the misfortune to investigate examples of a particular landform which have proved to be genetically atypical: their observations and reasoning were sound, but were applied to what were proved to be unusual examples.

The wrong place

Lunettes are small crescentic fixed dunes located on the lee sides of lakes and lake beds. Though similar forms had been earlier recorded from Texas (e.g. COFFEY, 1909), HILLS (1940a) described and named lunettes from northwestern Victoria. Some bordering Lake Eyre, in central Australia, are up to 50 m high (DULHUNTY, 1983), though most stand of the order of 10 m above the level of the adjacent lake depression and plains. Hills attributed the fine-grained lunettes with which he was concerned to atmospheric dust being moistened, coagulated and deposited on passing over the lake: what became known as the 'wet' theory. A few years later this explanation was challenged by STEPHENS & CROCKER (1946) who pointed out, amongst other things, that many lunettes are built of sand, which could not have been transported in suspension. They argued that wind action on bare dry alluvial flats in internal drainage basins would cause both the scouring of lake basins and the transport of debris to the edge of the basin where it would be trapped by vegetation: the so called 'dry' theory. BOWLER (1968) reported gravelly debris from within lunettes, which he considered to be relic Late Pleistocene forms, and in the same year CAMPBELL (1968) reported similarities in mineralogy and texture between lake bed deposits and adjacent lunettes, and the presence of rudimentary cross-bedding. She also pointed out that in terms of the dry theory, lunettes ought to be located on the lee sides of basins in respect of summer or dry season winds whereas many are oriented with respect to wet season winds. It was also difficult to understand how the material constituting sandy or silty lunettes bordering salinas could have been eroded by the wind from a source area covered by a layer of salt. To accommodate this and other field evidence, she proposed that lunettes are comparable to coastal foredunes. In the wet season waves (even in the shallow water implied by the topography of the basins) carry sediment to the lee shore where it is deposited in beaches and whence it is carried by the wind a short distance and trapped by vegetation, forming the mounds known as lunettes. [She later found that the essential evidence for this mechanism had been noted about a century previously by WOODS (1863, p. 28-29)!]

While these investigations were in hand, Hills defended the wet theory with some vigour, but faced with the evidence and arguments graciously gave way and acknowledged that the material of which lunettes are constructed derived from the lake basins and also attributed their transport to saltation (HILLS, 1975, p. 147). Had Hills not chanced to work on what it transpired were rather atypical silty lunettes in northwestern Victoria, he would, almost certainly, have reached different conclusions concerning the forms in general.

Correct but atypical

The structure of desert dunes of central Australia provides another example of unlucky choice of investigation site. MADIGAN (e.g. 1936, 1946) described the longitudinal dunes of the Simpson Desert, central Australia, as mobile constructional forms shaped by the wind. When, however, Donald KING (1956, 1960) investigated dunes adjacent to Lake Eyre he found they consisted of a veneer of sand over an elongate ridge eroded in lacustrine strata: they were not depositional forms but stationary wind-rift dunes. King's data were accurate, and his reasoning impeccable, but he extrapolated his findings not only to the entire Simpson Desert dunefield but to all the Australian dune deserts. In this he was wrong, for bulldozed sections of numerous dunes later showed beyond doubt that they are built of sand, and that they have been constructed by the wind (e.g. WOPFNER & TWIDALE, 1967). King's error was to extrapolate without checking, which is an obvious thing to do, but is in some circumstances - like those in which King found himself - logistically difficult to accomplish.

THE HUMAN FACTOR

Human traits impinge on the question of scientific discovery in various ways. The ingenuity of the human mind has provided inestimable benefits: *Quod homines, tot sententiae*: as many minds as men - show ten geologists a problem and at least ten possible explanations will be suggested. And this is admirable, for the greater the choice of possible explanation, the greater the chance of the truth eventually emerging; if indeed it can be recognised! But human characteristics and failings have also introduced problems.

The value of hindsight

The first stage in the evaluation of a concept or idea is for it to be recognised for what it purports to be. Some have been introduced as incidental or casual asides which in retrospect can be seen to be meaningful and which were grasped and developed by later workers. Thus the potential import of KNOPF's (1924, p. 637) comment that some high level remnants in the northern Appalachians are 'out of reach of erosion' has been alluded to in an earlier section of this essay. Its possible significance was not appreciated for many years, until Crickmay, who was for a year at Yale at the same time as Knopf, developed and used the idea as a the basis for his concept of Unequal Activity (CRICKMAY, 1932, 1974,1976).

Parallel scarp retreat has long been appreciated (e.g. FISHER, 1866) but its early application as an explanation of inselbergs is due to HOLMES (1918, p. 93) who devoted a single sentence to the possibility in a paper on the geology of Mozambique which occupied sixty-six closely printed pages. Scarp recession was later invoked as an explanation for landscape evolution in King's model of retreat and pedimentation (KING, 1942, 1953). He also applied the mechanism to bornhardt-inselbergs which he interpreted as the last remnants remaining after long distance scarp retreat (KING, 1949) and his model has found wide acceptance (e.g. OLLIER & TUDDENHAM, 1962; SELBY, 1977), though it leaves much field evidence unexplained and it is incompatible with some evidence and argument (e.g. TWIDALE, 1982).

HOLMES (1918, p. 93) also referred to scarps which had been worn back by intense localised attack at their base, thus presaging the basal sapping of cliffs described by PEEL, (1941, p. 16-18), and which in turn bears on the origin of the piedmont angle (TWIDALE, 1967), admittedly with an emphasis on surficial rather than subsurface weathering, but nevertheless germane to the problem. [It should be added that Peel who reviewed that paper was too modest to mention the matter in his report to the journal editor!]

Of course the authors mentioned may not themselves have appreciated the significance of their incidental observations and comments, but that is unlikely; they simply did not emphasise or highlight what were, in the contexts in which they were working at the time, incidental, almost extrinsic, observations and inferences.

Out of sight, out of mind

Conversely, some concepts have been recycled and rediscovered many times over. Take for instance the two-stage development of granitic boulders. That they originate by fracture-controlled weathering in the shallow subsurface was appreciated at least as early as 1791 (HASSENFRATZ, 1791), and this was noted by HUTTON (1795, p. 174) in his widely read treatise; but the idea was evidently lost and rediscovered many times during the next 150 years (TWIDALE, 1978). Moreover, and arising in part from an ambiguity which developed concerning the meaning of the term 'tor' - some took it to mean a small steep sided tower-like form, 'about the size of a house' (JONES, 1859; LINTON, 1952), while others, particularly those working in the southern continents, took it to mean a boulder (e.g. WILLIAMS, 1936, HILLS, 1940 b, p. 26-28; COTTON, 1948, p. 30; MABBUTT, 1952; THOMAS, 1965), while others used it in both senses (LINTON, 1952, 1955). Indeed, the two-stage concept was extended to larger residuals, namely what are now referred to as bornhardts and castle koppies (FALCONER, 1911, p. 145-247) and later to tors (LINTON, e.g. 1952, 1955), without reference to the key earlier publications.

DISCUSSION

The examples discussed raise questions as to why some explanations of landscape have found ready and enduring acceptance while others have received stubborn rejection. Are we too much influenced by what we believe, and not willing enough to consider the unlikely or even the impossible, even in the face of what we like to call common sense? Are we Anselm, and believe in order to understand, or Abelard and understand in order to believe? It is easy to be adversely critical in retrospect and it is necessary to bear in mind first that events now past were once in the distant future, and second that the interpretations now in vogue are all subject to modification or rejection in due course. But for science to advance it is sometimes necessary to entertain the absurd, for as Thomas Huxley remarked, the 'silly' question is frequently the first intimation of a new development. Outrageous hypotheses (DAVIS, 1926) have been instrumental in initiating radical reinterpretations: what is now taken as proved (*pro tempore*) was once only imagined. Many of the absurdities will prove to be just that, but occasionally one incredible idea will lead to a total revision of understanding. Only by generating such seemingly ridiculous explanations, and accepting the obloquy that unfortunately so often accompanies them, will science in general, and geomorphology in particular, advance.

In some instances, even at the time problems were defined and resolved, contrary evidence and viable alternative explanations were available. For example, it was known from quarrying experience in the middle of the last century and earlier that rocks expanded on being unconfined, and rock bursts were a recognised hazard, yet they were inconsistent with the tensional environment implied by erosional offloading. The concept of insolation-induced weathering is revived from time to time and persists in texts despite Barton's observations and various experimental work which cast doubt on it. Bornhardt-inselbergs were long regarded as desert and savanna forms yet some of the earliest accounts of the forms derived from the humid tropics (e.g. DARWIN, 1846). Why are contrary evidence and argument neglected, and other, demonstrably dubious, explanations preferred? Why, a century or so ago, was the complex overthrust and nappe structure of the

European Alps (see e.g. COLLET, 1927) accepted, despite the lack of an energy source, whereas inability to identify a viable means of moving sialic (continental) masses was held against Wegener's theory of continental drift (WEGENER, 1924; also TARLING & TARLING, 1971)?

Is there, despite its basic innovative purpose, an innate conservatism in science? Or is it, as SHERMAN (1996) has articulated, that there are leaders - or 'dudes' in the modern jargon - whose position, intellect or personality has led to their ideas being slavishly followed. From time to time overarching concepts justifiably gain wide consideration, leading to them and their implications being applied because they are fashionable, and not because they are apposite in terms of the evidence and argument. The peneplain concept provides a clear example of both of these last two factors, with Davis' disciples uncritically applying the great man's interpretation. Personal factors can also lead to dispensations being allowed some but not others.

Such matters, involving what might mildly be termed academic misjudgements or even discourtesies, surely ought to be discussed. Editorial attitudes are critical in such situations, for surely objective, sober, professional comments and responses ought not only to be allowed but to be welcomed? Scientific advance involves amiacable disputation. Yet it is not always so, for some editors are unwilling to allow legitimate disputes involving dubious or inconsistent practices to be aired: the litigious society in which we live has much to answer for.

There is an obverse side to Sherman's suggestion of dudes or fashions, namely that a lack of constraints may partly account for the many new ideas generated by those working in geographically and academically remote regions of the world. In addition to being stimulated by strange landscapes and scientifically virgin lands, the great German explorers of Africa and the brilliant geologists who investigated the American West, workers like Jutson and Hills in Australia, King and du Toit in South Africa, and Logan and Scrivenor in Malaya, may have benefited from being outside the scientific mainstream, beyond the influence of academic hierarchies and systems, and thus being less shackled, more daring, in their thinking.

The difficulties inherent in recognising the truth carry interesting implications for our systems of appointment, promotion and awards, for natural selection implies that the merits of a particular concept will not become clear for some time. An idea may justifiably be hailed as clever or elegant but intellectual sophistication is not necessarily coincident with truth. Eventually the problem will resolve itself with the effluxion of time, as personal factors go to the grave with their initiators, as data becomes more accurate and complete, and by a process of natural selection. Eventually someone will investigate the history of an idea, or review the work of some worker long since dead, unravel the sequence of events and feel sufficiently wedded to the perceived truth to publish, and, with the benefit of hindsight, be right. In varying degrees this has happened already with Hassenfratz and Holmes, Crickmay, Peel and Bretz. Truth is the daughter of time.

CONCLUSION

The distinguished British historian A.J.P. Taylor has pointed out that all advance comes from nonconformity. But society does not like mavericks, renegades and heretics - indeed anyone who asks uncomfortable questions, or challenges the conventional wisdom, and tenacity and a thick skin are as useful in science as in life. Parga Pondal was a rebel in his time. In modern parlance, he was seriously and variously politically incorrect. The Spain of his day was not a democracy in which it was safe to espouse unpopular causes. But he eventually not only overcame adversity but in some ways benefited from it. He was living proof of Balzac's assertion that there is no such thing as a great talent without great will power. He maintained his sense of integrity, and thus his ability to be comfortable with himself when, in an immediate sense at any rate, it would have been advantageous, and certainly easier, to run with the crowd. He did not, and we admire, respect and thank him for it.

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