Contributions to the history of geomorphology and Quaternary geology: an introduction

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This Special Publication deals with various aspects of the histories of geomorphology and Quaternary geology in different parts of the world. Geomorphology is the study of landforms and the processes that shape them, past and present. Quaternary geology studies the sediments and associated materials that have come to mantle much of Earth’s surface during the relatively recent Pleistocene and Holocene epochs. Geomorphology, with its concern for Earth’s surface features and processes, deals with information that is much more amenable to observation and measurement than is the case for most geological work. Quaternary geology focuses mostly, but not exclusively, on the Earth’s surficial sedimentary cover, which is usually more accessible than the harder rocks of the deeper past.

Institutionally, geomorphology is usually situated alongside, or within, academic departments of geology or geography. In most English-speaking countries, its links are more likely to be with geography; but in the United States these connections are usually shared between geography and geology, although rarely in the same institution. In leading institutions everywhere, strong links exist between geomorphology and such cognate disciplines as soil science, hydrology, oceanography and civil engineering. Although nominally part of geology, Quaternary geology also has strong links with geography and with those disciplines, such as climatology, botany, zoology and archaeology, concerned with environmental change through the relatively recent past.

Given that geomorphology concerns the study of the Earth’s surface (i.e. landforms, and their origin, evolution and the processes that shape them) and that the uppermost strata are in many cases of Pleistocene and Holocene age, it is unsurprising that this Special Publication should deal ‘promiscuously’ with topics in both geomorphology and Quaternary studies. This particular selection has been developed from a nucleus of papers presented at a conference on the histories of geomorphology and Quaternary geology held in the Baltic States in 2006, where a great deal of what the geologist sees consists of Quaternary sediments. However, much of the Earth’s surface is not formed of these sediments but of older rocks exposed at the surface by erosion and structural displacement. Here, geomorphology can seek answers to questions regarding the past histories of these rocks, their subsequent erosion, and present location and form. Geomorphology also raises questions, and may provide answers, regarding tectonic issues, for example from deformed marine terraces and offset fault systems. In all these instances, the history of geological and geomorphological investigations can serve to illustrate both the progress and pitfalls involved in the scientific understanding of the Earth’s surface and recent geological history.

There are relatively few books but a growing number of research papers on the history of geomorphology. For readers of English, there is a short book by Tinkler (1985) and a collection edited by the same writer (Tinkler 1989), an elegantly written volume on British geomorphology from the sixteenth to the nineteenth century by Davies (1969), and a series of essays by Kennedy (2006). But towering over all other writings are three volumes: those by Chorley et al. (1964) on geomorphology up to the time of the American, William Morris Davis (1850–1934); by Chorley et al. (1973) dealing exclusively with Davis; and by Beckinsale & Chorley (1991) on some aspects of work after Davis. As envisaged by Chorley and Beckinsale, who died in 2002 and 1999, respectively, a fourth volume by other authors is soon to emerge (Burt et al. 2008). A series of essays edited by Stoddart (1997) on Process and Form in Geomorphology (1997) also contains valuable historical material, while papers edited by Walker & Grabau (1993) discuss the development of geomorphology in different countries, of which Australia, China, Estonia, Iceland, Japan, Lithuania, New Zealand, The Netherlands, the USA and the USSR are specifically mentioned in the present volume.

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A framework for geomorphology

Connections between geomorphology and geology go back to the early days of Earth science, but it is to developments in the later eighteenth century that we often attribute the foundations of modern links between the disciplines, notably to scholars such as Giovanni Targioni-Torreggini (1712–1783) in Italy, Jean-Etienne Guettard (1715–1786) in France, Mikhail Lomonosov (1711–1765) in Russia and James Hutton (1726–1797) in Scotland. Hutton gave much thought to extended Earth time, and to the processes whereby soil and rock are eroded from the land to the sea. In 1802, Hutton’s friend and biographer, John Playfair (1748–1810), not only rescued Hutton’s ideas from relative obscurity but contributed original ideas on the nature and behaviour of river systems. However, the intellectual climate of the time worked against the ready acceptance of their views.

Following the leads provided by Hutton and Playfair, Charles Lyell (1797–1875) also addressed questions of extended Earth time and of erosion in his well-known and influential three-volume treatise Principles of Geology (Lyell 1830–1833). He emphasized the differential erosive powers that rivers or the sea could have on strata of different hardness, and discussed cases where river systems did not divide simply, like the branches of a tree, but cut through higher ground or occupied the eroded axes of anticlines. The latter phenomenon could be explained by supposing that folding had fractured the rocks at an anticlinal crest so that they became more prone to erosion, with the result that ‘reversal’ of drainage might occur. But Lyell realized that most of the rivers draining the Weald of SE England did not follow the main axis of the Wealden anticline but often cut through the North or South Downs that formed the flanks of the fold. He attributed such anomalous configurations to fractures that cut across the Wealden axis and to the interaction of Earth movements and fluvial erosion. Thus, Lyell invoked geomorphological and tectonic considerations in order to develop a geological history of a region.

A name that often emerges in the present collection of papers is that of W.M. Davis, with his theory of a cycle of erosion that was constructed in part on the work of his compatriots John Wesley Powell (1834–1902), Clarence Edward Dutton (1841–1912) and Grove Karl Gilbert (1843–1918) (Davis 1889, 1899, 1912). And one may reiterate that Davis’s work was considered by Chorley et al. (1973) to be so influential as to warrant an entire volume of their comprehensive historical study of geomorphology.

Davis’s initial cyclic ideas were encapsulated in the hypothesis that, following initial structural uplift, landforms shaped by rivers pass through different stages of development, which he dubbed ‘youth’, ‘maturity’ and ‘old age’, until they are reduced to a nearly level surface or ‘peneplain’. The peneplain, for which he found evidence in the Appalachians, could later be ‘rejuvenated’ by uplift, thereby initiating a new cycle of erosion. This model led to studies of ‘denudation chronology’, or the reconstruction of landscape histories based on the recognition of erosion cycles and peneplains in various stages of development. Without a clear understanding of the processes and time involved, however, ‘reading a landscape’ through the lens of Davisian doctrine, or elucidating its ‘denudation chronology’, became an art form, rather than a rigorous science. Davis’s geomorphic model was essentially qualitative and difficult to test but, as Charles Darwin famously wrote regarding his notion of natural selection, ‘here then I had at last a theory by which to work’ (Darwin 1887, p. 83).

Davis’s ideas were challenged in his own time, particularly by German geomorphologists such as Albrecht Penck (1858–1945), Professor of Physical Geography at the University of Vienna and later of Geography at Berlin, and more particularly his son Walther Penck (1888–1923). Before the World War I, the Pencks and Davis were on good terms, but they subsequently drifted apart, partly owing to world politics and partly owing to Walther’s rejection of the idealized character of Davis’s theory along with disagreements as to the relationship between Earth movements and erosion. The Pencks objected to the notion of discrete upward Earth movements as the cause of topographic rejuvenation and also argued that erosion wears back a surface just as much as down. However, Walther Penck’s proposed model of slope retreat would eventually yield a gently sloping surface resembling a Davisian peneplain (Penck 1924, 1953). Penck also envisaged an empirical relationship between tectonic activity and slope development, owing to the changing rates of river incision as the land itself was raised at varying rates. This idea was rejected vigorously by some in the English-speaking community, with Douglas Johnson (1878–1944), for example, describing it as ‘one of the most fantastic ideas ever introduced into geomorphology’! (Johnson 1940, p. 231).

Ultimately, the differences between Davis and Penck lay in their different objectives and scientific approaches. Davis regarded geomorphology as a branch of geography, with geographic processes furnishing the topography upon which geography ‘resided’. He, together with a number of like-minded geologists, geomorphologists and natural scientists, founded the Association of American Geographers in 1904, in part as a forum for his...
views (Orme 2005). Penck, in contrast, saw the field as being one that could elucidate problems of crustal movements and he was apparently less concerned with process and time (Hubbard 1940). It may be noted, though, that in his old age Davis accepted the idea of parallel slope retreat, such as is usually associated with the name of Walter Penck. Davis’s changed views were given in lectures delivered at the University of Texas in 1929 but were not published until as late as 1980 (King & Schumm 1980).

Another major figure in the modern formulation of ideas on landscape evolution was the South African geomorphologist Lester C. King (1907–1989). Imbued with Davisian ideas and the triad of process, time and structure, as a graduate student of Charles Cotton (1885–1970) in New Zealand, King nevertheless went on to challenge much of Davisian theory. While still invoking the cyclic concept, like Penck he emphasized the importance of surficial processes, particularly in relation to the role of scarp retreat and pediment formation, and the considerable antiquity (e.g. Cretaceous) of some erosion surfaces. Given the structure of his adopted homeland in Africa, with its extensive flat-lying strata and thus many potential cap rocks, it is not surprising that King interpreted landscapes primarily in terms of scarp recession with consistency of slope form and inclination in any area and structural setting indicating parallel retreat. He thought that steep slopes are shaped by gravity and turbulent water flow (e.g. in gullyng), whereas pediments, the typical landform of erosional plains, are the result of surficial water flow (sheet wash), capable of transporting sediment and ‘smoothing’ the bedrock (King 1953). Pediments or pediplains persisted until another cycle of river incision or change in base level occurs, causing further slope retreat.

Thus, although King concluded that the evolution of landscapes by the action of running water would occur everywhere, except in glacial and desert areas, his ideas stemmed from observations in a semi-arid South Africa with limited river action, where weathering and rockfall predominate, and where scarp retreat, which occurs everywhere, is closely linked to pedimentation, which is of limited importance. King’s recognition of a Mesozoic (or Gondwana) surface on the Drakensberg gave support to the idea that not all landforms are necessarily Late Cenozoic in age, as postulated in other theories of landscape evolution (e.g. Hack 1960). Mesozoic or Early Tertiary palaeosurface remnants have been identified in many other cratonic and old orogenic areas (e.g. China and Australia; see articles by Branagan (2008) and Zhang (2008), respectively, in this Special Publication), and their persistence raises fundamental questions about the complex interaction of surface-shaping processes such as erosion, the effects of climate change, tectonic uplift and deformation, etc., the duration of erosion ‘cycles’, or rock composition and structure.

Davis’s erosion model was imbued with ideas drawn from Darwinian biology and his interests in entomology, and his diction was full of evolutionary metaphors. He was also interested in the pragmatic philosophy of Charles Peirce, as has been remarked by Baker (1996). By contrast, an awareness of recent developments in thermodynamics manifested itself in Gilbert’s geomorphology through notions of dynamic equilibrium, grade and feedback loops. Gilbert’s concept of ‘negative feedback’ in stream systems leading to ‘graded rivers’ occurred some 7 years before Henri Le Chatelier (1850–1936) enunciated his well-known principle as a general feature of chemical systems. Gilbert wrote:

Let us suppose that a stream endowed with a constant volume of water is at some point continuously supplied with as great a load as it is capable of carrying. For so great a distance as its velocity remains the same, it will neither corrad (downward) nor deposit, but will leave the grade of its bed unchanged. But if in its progress it reaches a place where a greater declivity of bed gives a diminished velocity, its capacity for transportation will become less than the load and part of the load will be deposited. Or if in its progress it reaches a place where a greater declivity of bed gives an increased velocity, the capacity for transportation will become greater than the load and there will be corrasion of the bed. In this way a stream which has a supply of debris equal to its capacity, tends to build up the gentler slopes of its bed and cut away the steeper. It tends to establish a single uniform grade.

(Gilbert 1877, p. 112)

In the same publication, Gilbert also enunciated ‘laws’ for the formation of uniform slopes, structure and divides, and the concept of planation. In vegetated areas, he believed that the ‘law of divides’ was likely to prevail; in arid regions, he favoured the ‘law of structure’. Thus, the ‘laws’ were not universal, in the style of Newton’s laws. Nevertheless, Gilbert’s work marked a significant advance in the search for geomorphological principles and thereby a step towards the establishment of geomorphology as a physical science (rather than an historical ‘art’!). By contrast, Davis’s ‘evolutionary geomorphology’, although attractive to his contemporaries and through much of the first half of the twentieth century, has now been largely or wholly superseded.

But Gilbert’s concept of ‘grade’ also presents problems. It is supposedly a situation of balance between the transport of material in a river and the widening or deepening of the river bed by corrasion. According to Davis, for a ‘mature’ river ‘a balanced condition is brought about by changes in the capacity of a river to do work, and in the
quantity of work that the river has to do’ (Davis 1902, p. 86). This assumes that for a given rate of river flow, there is a limit to the load that it can carry, and that the energy available can be used for either transport or corrasion. But these cannot just be summed, so that for a given stream flow if there is an increased load there is an equivalent decrease in corrasion. But this is simply not the case: halving the load does not double the corrasion (Wooldridge 1953, p. 168).

Walther Penck’s interest in the relationships between Earth movements and landforms was shared by the French geomorphologist Henri Baulig (1877–1962), who was a student of Davis for 6 years at Harvard. Baulig’s main area of research was France’s Massif Central for which he tried to synthesize the ideas of Davis and those of the notable Austrian geologist Eduard Suess (1831–1914) (Baulig 1928). During the later nineteenth century, Suess had sought a global understanding of geological phenomena in terms of the increasingly questionable notion of a cooling and contracting Earth, which led to lateral compressive forces that produced great orogenies. With each large-scale collapse of Earth’s crust, Suess believed that there was a concomitant global lowering of sea level as well as elevation of mountain ranges. Worldwide erosion and sedimentation would follow, and the ocean basins would receive sediment, leading to global marine transgressions. These would supposedly account for the correlations that might be made worldwide for different parts of the stratigraphic column. In 1888, the global changes in sea level, arising from spasmodic tectonic episodes, were called ‘eustatic movements’ by Suess (English translation 1906, p. 538); and, thus, there emerged the concept of global ‘eustasy’, based on intelligible (albeit mistaken) explanatory principles. Suess’s ideas were attractive in Baulig’s earlier years as the basis of a general geological theory and it is therefore unsurprising that Baulig sought to link them to his geomorphological studies.

In considering the relative levels of land and sea (globally), one could consider epeirogenic movements, isostasy and eustasy (the latter being due to epeirogeny/diastrophism or the waxing or waning of glaciation, which could also generate isostatic responses). And if a land surface is reduced by erosion there will also be an isostatic response. Despite these complexities, Baulig favoured global eustasy as the main source of the formation of planar erosion surfaces. This opened the prospect of worldwide temporal correlation of penepia:ns:

[Regions, widely-spaced and totally independent from a structural viewpoint, show perfectly clearly an exactly similar geomorphological development since the Upper Pleistocene. This similarity, in the present state of ideas and knowledge, admits of only one explanation: that it is eustasy pure and simple. (Baulig 1928, p. 543; translation from Beckinsale & Chorley 1991, p. 268)

Of course, it was easy to conflate or confuse glacial and Suessian eustasy. Nevertheless, Baulig reiterated his ideas in 1935, extending his claims of uniformity of marine terraces at distant locations back into the Pliocene (Baulig 1935). But this line of inquiry led to confusion as much as to understanding.

The search for guiding principles, or ‘laws’ as they were (or are) sometimes mistakenly called, has been a recurrent feature of the history of geomorphology. As early as 1802, Playfair enunciated a general principle that, despite many exceptions, became known as ‘Playfair’s Law’, thus:

Every river appears to consist of a main trunk, fed from a variety of branches, each running in a valley proportional to its size, and all together forming a system of vallies, communicating with one another, and having such a nice adjustment of their declivities, that none of them join the principal valley, either on too high or too low a level; a circumstance which would be infinitely improbable, if each of these valleys were not the work of the stream that flows in it. (Playfair 1802, p. 102)

As the field of geomorphology developed, the search for so-called laws among drainage networks continued to interest scholars. For example, is there any pattern, any law-like behaviour in such networks? Can a mathematical model of stream branching be discerned? Very early on, Leonardo da Vinci (1452–1519) had noted the similarity of branching in trees and stream systems (Shepherd & Ellis 1977). Later, following the physician James Keill’s (1673–1719) (1708) early anatomical work on arterial trees, known to Hutton and Playfair, Julian Jackson (1790–1853) (1833) addressed the notion of ‘stream order’ in 1834, and Harry Gravelius (1861–1938) of the Dresden Technical Institute later expanded on these ideas (Gravelius 1914). The largest or stem stream was designated as being of Order 1; the first tributary was of Order 2; and so on back to the unbranched ‘fingertip’ tributaries.

This nomenclature (or taxonomy) prevailed in Europe for a considerable time. But the US Geological Survey hydrologist Robert Horton (1875–1945) reversed the terminology so that ‘un-branched tributaries are of 1st order; streams that receive 1st-order tributaries, but these only, are of the 2nd order; third order streams receive 2nd- or 1st- and 2nd-order tributaries; and so on, until, finally, the main stream is of the highest order and characterizes the order of the drainage basin’ (i.e. the highest order stream extends from source to outlet) (Horton 1945, p. 277). Subsequently, the American geologist Arthur Strahler (1918–2002) proposed an alternative system of
stream ordering designed to give an idea of the relative power of the different waterways in a drainage system (the higher the order the higher the power) (Strahler 1952).

However, the Horton and Strahler schemes were misleading, and mathematically cumbersome, in that they ignored downstream links with streams of lower order. Later schemes by Adrian Scheidegger (b. 1925) (1965) and Ronald Shreve (1966) resolved this problem. Although Horton’s early system allowed some interesting ‘laws’ to be defined and for drainage densities to be calculated, by the 1970s it had come to be realized that such relationships were in large measure a consequence of the ordering systems used and the topological randomness of such networks. Nevertheless, stream-ordering systems continue to be used for ranking purposes by drainage basin specialists.

Among Strahler’s students at Columbia University, Stanley Schumm (b. 1927), who spent most of his career with the US Geological Survey and at Colorado State University, and Mark Melton (b. 1930) were particularly prominent. Melton did his PhD at Columbia and moved from there to the University of Chicago, where he was given to understand that strongly mathematical and statistical work would be appreciated. Schumm (1956) measured and analysed both the surface and subsurface processes involved in slope development in order to provide a theoretical analysis of fluvial erosion. Melton’s mathematically sophisticated work used a systems approach and ergodic reasoning for the analysis of geomorphological problems (Melton 1958). Perhaps unsurprisingly, he demonstrated that channel frequency was a function of drainage density. In these and other ways, Playfair’s early insight on the form and interrelationships of drainage networks was given mathematical expression during the so-called quantitative revolution in geomorphology during the mid-twentieth century.

Prior to the ‘quantitative revolution’, the time-dependent models of Davis and Penck had incorporated into geomorphology the notion of uniformitarianism: the assumption of gradual change through time based on the principle that ‘the present is the key to the past’ (Geikie 1962, p. 290). In contrast, the earlier work of Gilbert, based on the dynamic interactions of landform processes, was more suited to a time-independent approach, although he did not develop a comprehensive model. And in 1960 such a model, based on the study of humid temperate drainage basins, was proposed by John T. Hack (1913–1991) of the US Geological Survey. Hack’s model revealed conflicts with erosion-cycle concepts and presented time-independent equilibrium as an alternative to the Davisian system. Instead of attributing it to age, landscape variability was considered to result from interacting contemporary processes wherein a state of balance, or dynamic equilibrium, existed between fluctuating inputs and outputs of material and energy. According to Hack, landforms were open systems so that similar landforms could have different origins. For example, accordant summit heights, invoked by Davis as evidence of former peneplains, may originate in rocks with similar hardness, structure and drainage density. Where rocks differ in resistance, there is the possibility of different levels of accordant summits, which are not necessarily explained in terms of multiple erosion cycles. Implicit in Hack’s work was the assumption that there is a uniform lowering of the landscape with little obvious change in rate and process unless there is a change in climate, tectonism or geology. Thus, Hack’s model was in broad agreement with Penck and, provided that uplift was slow enough to balance the rate of erosion, a steady-state relief would result. Sudden uplift would produce transitional relief, with relict landforms disappearing as a new equilibrium state was approached.

The advantages of this dynamic equilibrium approach to landscape evolution was that it was not constrained by, or dependent on, a Davisian stage, and it provided a convenient entry point (current conditions) for understanding the system because the past is usually poorly known. The idea of dynamic equilibrium relies on the notion that landscape systems near equilibrium change slowly (time-dependent) and those that are far from equilibrium change rapidly (time-independent). The concept thus unites two viewpoints. But debate continues, with arguments that equilibrium probably never exists in the multivariant, often chaotic and non-linear nature of Earth processes, and that they more probably reflect disequilibrium (e.g. Phillips 1999).

The concept of time is important to geomorphology, but it was not until the twentieth century that the traditional preoccupation with time-dependent landscape evolution could be tested. The long-established foundations of the geological timescale, such as the principles of stratigraphy, could not readily be applied to landforms undergoing denudation but for which there were no residual deposits. For relatively short-term geomorphic events, time might be measured directly during a particular event or period of measurement, or by reference to records over periods of recorded time. By contrast, for studies of landscape change over longer periods, say from thousands to millions of years, some means of establishing the time frame is necessary and it is only within the past 100 years or so, and often more recently, that such methods have become available. They include absolute
A framework for Quaternary geology

The Quaternary is the shortest and most recent of the geological periods recognized in Earth history, defined here as the last 2.6 Ma. It is, perhaps, the most important period of time because, despite its brevity relative to earlier periods, its materials cover much of the present landscape and provide soils and resources for agriculture and other human activities. It has been a period of pronounced climate change with all that implies for Earth’s land surface, hydrosphere and biosphere. It has also witnessed the later evolution of hominids and the emergence of *Homo sapiens*. In short, it is a period of Earth time of great intrinsic and practical interest. It is also, as we shall show, the focus of much controversy.

Recognition of the peculiar properties of the Quaternary Period was slow to emerge, and debate continues as to the precise nature of the changes and forces involved. A major issue for geoscientists in the early–middle nineteenth century was the origin of the extensive surficial deposits, from clay to boulders, found across northern Eurasia and North America. The deposits were mostly poorly consolidated, unsorted, poorly structured and devoid of guide fossils. Following the dominant biblical beliefs of the time, these deposits had often been ascribed to materials deposited by the Noachian Deluge. Later, several catastrophic episodes were thought to have interrupted geological history from time to time, as supposed by Georges Cuvier (1769–1832) (1813). The deposits supposedly derived from the Deluge were termed ‘Diluvium’ and were distinguished from ‘Alluvium’, which was still to be seen being laid down by rivers (Buckland 1819, pp. 532–533).

Roderick Murchison (1839, vol. 1, p. 509) preferred the term ‘drift’ to Diluvium, as that word did not have any connotations of the ‘Deluge of Holy Writ’ and might be applied to deposits of similar character from different locations and of different ages, many of them attributable to marine currents. The term ‘drift’ caught on and, despite its ‘archaic’ implications, has survived to the present in many areas. In describing his observations on coastal exposures in Norfolk in 1839, Lyell (1840, p. 176) deployed Murchison’s term ‘drift’ as a substitute for ‘Diluvium’ and added the suggestion that erratic boulders and the like were emplaced as drop-stones by floating icebergs at a time of reduced global temperature, rather than by exceptionally violent marine currents. Thus, where Lyell found such materials onshore, the land surface must have been lower than at present. The ‘iceberg theory’ accorded with Lyell’s objections to catastrophic floods as geological agents but it initiated the unhelpful notion of ‘glacial...
submergence’: the idea that epochs of cold in regions presently mantled in drift deposits coincided with marine transgressions. Yet, the involvement of the sea in drift deposits seemed helpful in seeking to explain the occurrence of stratified layers in some ‘tills’ (the Scottish term that Lyell also employed).

The ‘glacial theory’ (or ‘land-ice theory’) emerged through the work of Ignace Venetz (1788–1859) and Jean de Charpentier (1786–1855), among others, in the European Alps, and was popularized by Louis Agassiz (1807–1873) with his publication of *Etudes sur les glaciers* (1840). It was Agassiz who brought the Swiss land-ice theory to Britain at the Glasgow meeting of the British Association in 1840; and then to America with his appointment to Harvard University in 1846. Yet, for a time the unhelpful ‘glacial submergence’ theory remained popular, at least in insular Britain, as it gave an attractive explanation of the presence of erratics over much of the low-lying ground of northern Europe. In his *Antiquity of Man*, Lyell (1863) seemingly accepted the ‘land-ice theory’ but he later reverted to his ‘iceberg theory’. Meanwhile, stimulated by writings on climate change by the astronomer John Herschel (1792–1871) (1830) and Joseph Adhémar (1797–1862) (1842), the Scotsman James Croll (1821–1862) developed the then remarkable explanation for glacial epochs in terms of an astronomical theory (Croll 1867) – a forerunner of the early twentieth-century work of the Serbian mathematician Milutin Milanković (1879–1958) (synthesized in 1941 and republished in English in 1998).

Despite, or perhaps because of, Agassiz’s advocacy of an extreme monoglacial ‘land-ice theory’ and Lyell’s focus on his ‘iceberg theory’, the concept of widespread continental glaciation made slow progress in the mid-nineteenth century, and some opposition persisted to the close of the century (Orme 2002). Eventually, the publication of *The Great Ice Age* by James Geikie (1839–1915) (1874) and *The Ice Age in North America* by George Frederick Wright (1838–1921) (1889) did much to confirm the theory. Geikie’s book was especially influential because he moved among influential scientists, including Thomas C. Chamberlin (1843–1928) in North America and Otto Torell (1828–1900) in Sweden, who early recognized the evidence for multiple glaciations. By then, evidence for extensive non-glacial deposits of Quaternary age, such as loess and pluvial lake deposits, was also emerging.

The term ‘Quaternary’ (‘*Quaternaire*’) was first proposed by Jules Desnoyers (1801–1887) (1829) as an ‘extra’ to the Primary, Secondary and Tertiary subdivisions of the stratigraphic column that had been proposed in Italy in the eighteenth century by Giovanni Arduino (1760). The term ‘Pleistocene’ (‘*Pléistocène* tirée du grec pleistos, *plus* kainos, *recent*’) was introduced by Lyell in 1839 in the Appendix to the French edition of his *Elements of Geology* (1839, p. 622), as an alternative name for his previous term ‘Newer Pliocene’, proposed in his *Principles of Geology* (1833) on palaeontological grounds. (We thank Dr G. Gohau for checking this reference in a Paris library.) Lyell (1833, vol. 3, p. 61) referred to post-Tertiary sediments as ‘Recent’ and stated that ‘some authors’ used the term for ‘formations which have originated during the human epoch’; but he did not favour that definition (Lyell 1833, vol. 3, pp. 52–53). The name ‘Holocene’ (= ‘wholly recent’) was subsequently suggested by Paul Gervais (1867, vol. 2, p. 32), and the term ‘Holocene’ as a synonym for ‘Recent’ was ratified at the Third International Geological Congress in London in 1885. For Gervais, the Quaternary was made up of the Pleistocene and the Holocene. The latter was estimated as being some 8–10 ka in duration. Moreover, with the general acceptance of the idea of a ‘Great Ice Age’, the term Pleistocene came to be used to represent the period of time when glaciation was widespread in the northern hemisphere (as suggested by Edward Forbes (1846, p. 403)). Lyell, however, did not use the term ‘Quaternary’.

So where should the base of the Pleistocene be located, and how should it be related to the Quaternary? Maurice Gignoux (1910) suggested that the base of the Quaternary should be defined by a site in Calabria in southern Italy, where sediment containing cold-water fossils (especially *Cyprina islandica*) was seen to overlie sediments containing fossils indicative of a relatively warm climate. This event was not well suited for international correlation but was nevertheless accepted at the Eighteenth International Geological Congress in London in 1948 (King & Oakley 1950). Later, Hays & Berggren (1971) showed that the Calabrian deposit coincided quite closely with the top of the so-called ‘Olduvai Normal Event’, a short episode within the Matuyama Reversed Epoch of the geomagnetic polarity timescale, which occurred about 1.8 Ma. This geomagnetic marker offered the possibility of unambiguous worldwide correlation, and hence became widely accepted (Häq et al. 1977). A proposal for a Global Stratotype Section and Point (GSSP) for the boundary at Vrica in Calabria was ratified by the International Commission on Stratigraphy in 1983 and at the Moscow International Geological Congress in 1984 (Aguirre & Passini 1985) (initially it was set at 1.64 Ma and later charged to 1.81 Ma).
decision, as there was substantial evidence of earlier glaciations in other parts of the world (see, for example, Milanovsky 2008, published in this Special Publication). Moreover, the term ‘Quaternary’ was considered by many to be outmoded, being out of line with other stratigraphic terminology, given that Primary and Secondary had long been obsolete and that the Cenozoic had been divided into Palaeogene and Neogene, with the consequent demise of the Tertiary (Fig. 1). The issue of the Neogene has been an important additional factor confounding discussions of the Quaternary and the Pleistocene. The term was introduced by the Austrian Moritz Hörnes (1815–1868) (1853). For background on the Neogene and Palaeogene, see Berggren (1998).

Given the evidence for late Cenozoic glaciation in some parts of the world earlier than 1.8 Ma, and the fact that the Vrica GSSP was based on neither clear-cut bioevents nor climatic criteria (Partridge 1997, p. 8), an earlier date for the boundary was sought, particularly in the light of the discovery of the arrival of organisms indicative of a cold climate in the Mediterranean region prior to 1.8 Ma. Thus, the Gauss–Matuyama reversal at 2.6 Ma has been suggested as a suitable boundary (Pillans & Naish 2004). Although glaciation is known to have occurred in some parts of the world earlier than 2.6 Ma, this geomagnetic reversal meshes with determinable biostratigraphic changes indicative of climate change, a clearly identifiable event in oxygen-isotope stratigraphy (Shackleton 1997), and changes in grain size in Chinese loess deposits. The chosen golden spike, in Sicily, for the bottom of the new Gelasian Stage of the Upper Pliocene is thought to be only about 20 ka younger than the Gauss–Matuyama reversal (Rio et al. 1998, p. 85), so the fit is good. Moreover, the climatic deterioration could be related to a change from the dominance of orbital precession to that of the obliquity of the ecliptic, according to Milanković theory (Lourens & Hilgen 1997; Maslin et al. 1998).

But to place the base of the Pleistocene at 2.6 Ma would take the Quaternary down to the bottom of the uppermost (Gelasian) stage of the Pliocene (Fig. 1). Alternatively, it would require a decoupling of the Pleistocene from the Quaternary; yet, both have long been associated in geoscientists’ minds with the ‘glacial epoch’. In consequence, some authorities have proposed that the term ‘Quaternary’ should be dropped from the stratigraphic column (e.g. Berggren 1998). In Berggren’s view, it should (or would or could) only survive ‘for geopolitical purposes’! The issue has been particularly sensitive because Quaternary studies are such a well-established branch of geoscience, with the International Union for Quaternary Research (INQUA) having been established back in 1928. An attempt in 1998 to have the Pliocene–Pleistocene boundary placed at the bottom of the Gelasian by the Commission for Neogene Stratigraphy of the IUGS was unsuccessful (Ogg 2004, p. 125).

Given this messy situation, various proposals have been put forward (e.g. Suc et al. 1997; Pillans 2004; Gibbard et al. 2005; Suguio et al. 2005). But none of these provided a neat and consensual solution. Suc et al. (1997) favoured the move of the base of the Pleistocene to the bottom of the Gelasian and disuse of the term ‘Quaternary’. Pillans (2004) believed that the Quaternary should be preserved and should run from the bottom of the Gelasian to the present (there being no Holocene unit as the Neogene also runs through to the present); but the Pliocene–Pleistocene boundary should continue to be located at the top of the Gelasian; and there should be a Holocene above the Pleistocene. This was anything but tidy. Details of the comings and goings of the debate may be found at www.quaternary.stratigraphy.org.uk/meetings/Quat_TaskGroup_25Aug05.doc: Definition and geochronologic/chronostratigraphic rank of the term Quaternary. Recommendations of the Quaternary Task Group jointly of the International Commission on Stratigraphy (ICS, of the International Union of Geological Sciences, IUGS) and the International Union for Quaternary Research (INQUA). The issue had to do with the problem of synthesizing different dating methods and the various overt institutional and invisible networks of members of different research fields.

The debates were not confined to the Anglophone community. The Chinese Association for Quaternary Research (2005), based on the significance of Chinese loess deposits, supported the INQUA position, and maintained that the Quaternary ‘should be a formal unit with full Period/System status in geological time work’ with a base at 2.6 Ma. In 2007, the Chinese President of the IUGS (Zhang Hongren) wrote to the bickering chairs of the relevant ICS subcommissions, as well as the ICS Executive, telling them, in effect, to co-ordinate their activities and formulate a solution ready for ratification by the IGC in Oslo in 2008. (The letter, and many other relevant documents, may be viewed at http://www.quaternary.stratigraphy.org.uk.) And as it appears at the time of writing, the Quaternary will survive as a received stratigraphic unit (Period or System) with its base at 2.6 Ma, the Pleistocene and Holocene being its constituent Series or Epochs (Bowen & Gibbard 2007) (Fig. 1).

Future changes notwithstanding, the term Quaternary is used in this Special Publication to refer to the Pleistocene and the Holocene, the latter denoting the last 10000 radiocarbon years (11.5 ka calendar years), which is approximately equivalent
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<th>Erathem/Era</th>
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Fig. 1. Age of base of the Pleistocene and status of the Neogene–Quaternary periods after the International Commission of Stratigraphy (2004) (Gradstein *et al.* 2004) and Bowen & Gibbard (2007).
to the end of the last ice age and the emergence of civilized humans.

Stratigraphically, then, the Quaternary is subdivided on the basis of climate into cold (glacial) and warm (interglacial) stages. Global landscapes cannot be explained without reference to these alternations of cold and warmth associated with the repeated alternations of ice advance into, and retreat from, middle latitudes of the northern hemisphere. For example, alternations of wetter and drier conditions during the Pleistocene in the present deserts of the northern hemisphere reflect the pervading influence of climate changes that led, farther north, to glacial and interglacial conditions. Similar conditions occurred in the southern hemisphere, but there the potential impacts of climate change have been cushioned by the dominance of oceans, such that during cold stages nival and aeolian effects often take precedence over actual glaciation, although multiple glaciations have been identified in Tasmania and Patagonia. The development of ice sheets in high latitudes also caused the migration and compression of other climatic zones towards the equator so that nival or periglacial conditions extended well beyond their present limits. The climate changes were such that even in low latitudes many glaciers developed on the mountains of Papua New Guinea, Borneo, the tropical Andes and East Africa. Also, glacio-eustatic changes in sea level are global events. Thus, the climatic changes of the Quaternary had worldwide impacts.

Nevertheless, remnants of older landscapes persisted in regions beyond the reach of glaciers, and even in glaciated areas major preglacial features survived the passage of ice sheets, which, although scraping away loose debris, made only minor modifications to bedrock forms.

The environmental changes of the Quaternary have been influenced in part by the continued migration of Earth’s continental and oceanic plates, albeit at much slower rates than climate change, and by episodes of greater or lesser volcanicity, which often ejected large quantities of climate-changing tephra into the atmosphere. Nevertheless, for most of the climate changes experienced in the Quaternary, there is now broad consensus that changes in Earth–Sun relations are broadly responsible, accompanied and aided by complex feedback mechanisms including ocean–atmosphere and biosphere–atmosphere linkages. Thus, variations in the eccentricity of Earth’s orbit, the obliquity of its axis relative to the plane of the ecliptic and the precession of the equinoxes have caused cyclic changes, within periods of about 100, 41 and 21 ka, respectively, onto which shorter, more rapid fluctuations have been superimposed.

Modern ice sheets and glaciers are but shrunk remnants of their former areal extents, but the interpretation of the deposits left behind by the ice and the landforms shaped by the moving ice has been greatly facilitated by research on modern glaciers and by imaginative theoretical ideas. The first systematic explorations and studies of the Quaternary Period were carried out in Europe and North America, because by happy coincidence these were glaciated lands where the geological sciences initially blossomed and where social conditions encouraged travel and research. The knowledge of glacier motion and the recognition of glacial and interglacial deposits that flowed from this research have proved invaluable to the understanding of Quaternary events elsewhere, for example in Africa, Australia and South America. Similarly, early observations in the northern hemisphere regarding changing sea levels, now attributed to a variety of climatic and tectonic forces, paved the way for sophisticated studies of Quaternary land–sea relationships elsewhere in the world.

The Quaternary Period is important for many reasons. For the geologist, it affords insight into relatively recent events that also contribute to the understanding of the more distant past. For the climatologist, it presents evidence of past climates in a world beset with problems associated with continuing climate change. For biologists and soil scientists, events during the Quaternary help to explain much of the Earth’s present plant and animal distributions, and its soils, which are so important to agriculture.

The Quaternary has also seen the continued evolution of Homo sapiens. Some human impacts have been deleterious. As with climatic changes, their importance can be exaggerated but cannot be ignored. Without the advent of the human mind there would be no Quaternary, for the System (like all the units in the stratigraphic column) is a human construct – in this case the latest of several known periods of climatic aberration.

The papers

The papers contained in the present volume do not pretend to cover every aspect of the innumerable components of geomorphology or Quaternary geology, but they touch upon many and in a number of cases deal with topics that are not generally known to those interested in these branches of geoscience.

For the first paper in the present collection, Klemm, an historian of science at the Institute of History, Vienna University, Austria, explores the contributions of Adolphe Morlot to the establishment of the term ‘Quaternary’. Both he and Jules
Desnoyers have been credited with introducing the term, but, in fact, they did so independently, at different times and places. Desnoyers simply wanted to give a name to distinctive strata that lay above those that Ardouin had named Tertiary back in the eighteenth century. Morlot’s proposal came later and for quite different reasons. He found evidence in Switzerland for two distinct glacial episodes, separated by deposits that suggested a milder climate: an interglacial as we would say. These three sets of sediments, together with those of recent times, gave four ‘layers’. Hence, the name Quaternary. Professor Klemun’s work is a result of her interest in the historical development of scientific classificatory terms.

The papers by Baker (Department of Hydrology and Water Resources, University of Arizona, Tucson, USA) and Orme (Department of Geography, University of California, Los Angeles, USA) discuss issues relating to the Quaternary geology and geomorphology of large parts of western North America, of which both authors have detailed knowledge. Readers are likely to be familiar with the ‘Great Scablands Debate’ (Gould 1978), which had to do with certain features of the topography of eastern Washington where gigantic ripple marks, too large to be discerned from a single vantage point on the ground, suggested to the Chicago geologist J Harlen Bretz (1882–1981) that they had been produced by ‘catastrophic’ flooding. Joseph T. Pardee (1871–1960) had previously suggested large-scale flooding produced by the unblocking of a lake ponded by a glacier (Pardee 1910). Bretz’s fieldwork in the 1920s suggested that there had, indeed, been some such ‘catastrophic’ cause; and, in fact, the hypothesis that was eventually accepted was somewhat akin to the one suggested long before in the nineteenth century, by Leopold von Buch, to account for the movement of erratics in the Alpine valleys, which was also later adopted by the ‘uniformitarian’ Charles Lyell. Yet (ironically), Lyell’s ‘uniformitarian’ programme became so successful that any seeming ‘catastrophist’ theory, such as appeared to be suggested by Bretz’s observations, was dismissed for a long time.

The intricacies of this interesting debate are carefully traced by Victor Baker, who used to know Bretz personally and so knows his side of the story well. Baker’s account also includes reference to the philosophical issues involved: the reluctance of geologists in America in Bretz’s time to countenance anything that hinted at catastrophism; and the American preference at that time for what might be called naïve empiricism. For Baker, however, a theory that ‘binds together’ a number of distinctive facts (giving a consilience of inductions) has much to recommend it. He (and we) are devotees of the use of ‘coherence’ as a criterion in the pragmatic pursuit of truth, the virtues of which approach are well illustrated by the Great Scablands Debate. Baker was the keynote speaker at the Vilnius meeting.

The paper by Antony Orme on the Quaternary pluvial lakes of the American West is detailed, but at the same time offers a wide historical sweep, from the observations of the early European explorers in the region to the issues arising from the use of modern dating methods in the study of these lakes. Formerly, the study of the history of such lakes may have seemed a somewhat esoteric undertaking, but in more recent years it has come to be a matter of considerable practical concern, both from the perspective of water supplies and of data relevant to current problems relating to climate change. Here, then, is a case where the study of the history of geology turns out to have practical value to contemporary problems. The problem of understanding the history of these pluvial lakes is complex because there have been several independent causal factors involved in their history, such as rainfall, temperature, erosion and crustal deformation. Orme’s paper is also valuable, for the purposes of the present collection, in that it attends to issues relating to dating in the Quaternary.

The theory of continental glaciation, beginning from observations of ‘living glaciers’ and areas previously occupied by ice in the Swiss Alps, was worked out independently by Otto Torell (1828–1900) (in 1872), Piotr Kropotkin (1842–1921) (between 1862 and 1876) and others in northern Europe, to which workers in the Baltic States made significant contributions. Raukas at the Institute of Geology at Tallinn University of Technology, Estonia, describes contributions to the evolution of the continental glaciation theory by (amongst others): Karl Eduard Eichwald (1795–1876), the first person in the Baltic provinces to consider the possibility of the wide distribution of ice in lowland areas in 1853; Friedrich Schmidt (1832–1908), who studied the Quaternary deposits of Estonia, demonstrating a correlation with those in Sweden in 1865; and the Estonian stratigrapher Gregor Helmersen (or Gelmersen) (1803–1885) (who became Director of the St Petersburg Mining Academy), who rejected the drift hypothesis in favour of a continental ice sheet in 1869 on the basis of the distribution of erratic boulders, glacial clay and striations. It was Schmidt, in 1871, who proved that during the Pleistocene the Scandinavian ice sheet covered the Baltic Sea depression and surrounding territories, and thereby resolved the early controversy over continental glaciation in northern Europe.

Milanovsky is one of the ‘grand old men’ of Russian geology and a former head of department
at the Moscow State University. He comes from a family of distinguished geologists and has led a remarkable life, including fighting with a tank unit from Moscow to Berlin during the Great Patriotic War! He has recounted that, when he was a young man, he did not know whether he wished to study art or geology but in the end he opted for geology. Yet, his facility for drawing has been retained through his long career, and in this paper we see numerous examples of his quick sketches made during the course of his fieldwork. His special area of interest has been neotectonics; and for the purposes of this Special Publication volume his ‘proof’ of the occurrence of glaciation in the Pliocene is particularly interesting, as it contributed evidence that was relevant to the changes in the Quaternary and the Pleistocene that were discussed earlier in this introduction. Milanovsky’s use of the term ‘Eopleistocene’ in his stratigraphic table should be noted, as that has been the preferred term in Russia, as opposed to Lower Pleistocene. The present paper is to be understood as an autobiographical contribution to the study of the history of Quaternary geology.

Ivanova & Markin, from the Earth Science Museum of Moscow State University, Russia, give an overview of a work by the famous anarchist philosopher, Prince Piotr Kropotkin: Researches on the Glacial Period (1876). This book is virtually unknown to Anglophones and there appears to be no copy of it in either the Library of Congress or the British Library. It is, we are told, a rare volume, even in Russia. Kropotkin’s book Mutual Aid (1902) is well known in the West and makes reference to his travels in Siberia. Early in his career he travelled in that part of the world as a military surveyor, and later he went to Scandinavia, as a result of which he became interested in the ‘land-ice theory’. His recognition of the evidence for glaciation was not remarkable at the time of his travels, but his studies of eskers were noteworthy and original. Researches on the Glacial Period was written when Kropotkin was in prison in St Petersburg on account of his subversive political views, before he made his dramatic escape to the West. It is pleasing that his scientific investigations, so different from his well-known philosophical and political writings, should be recognized here. Kropotkin’s work also shows that there was not a huge difference between ‘Eastern’ and ‘Western’ science in his day. Unfortunately, not a great deal is known in the West about early Russian geology and the commonalities are not sufficiently recognized.

Ideas associated with the development of Quaternary research in the Baltic countries are also chronicled by Gaigalas from the Department of Geology and Mineralogy at Vilnius University, Lithuania, in terms of their geopolitical position, economic and social conditions, establishment of science centres, progress of geological thought, and the natural environment. Quaternary geological investigations in the Baltic region were, and continue to be, an important link between the East and West in northern Europe. Among the lineage of those involved in Quaternary research, the contributions of the Lithuanian geologist Česlovas Pakuckas (1898–1956) to glaciomorphological investigations of the Baltic marginal highlands in Lithuania and Poland are documented by the paper by Gaigalas et al. Pakuckas, who is regarded as a pioneer of modern glaciomorphological investigations, concluded that during the last glaciation the continental glacier was not a single ice sheet but consisted of a number of flows, each dependent on topography and each with its specific glacial centre. A section of organic-rich sediments that he discovered on the banks of the Nemunas River has recently become the stratotype of the Eemian Interglacial in Lithuania.

Kondratienė & Stančiukaitė at the Institute of Geology and Geography at Vilnius, Lithuania, evaluate the contributions of another Lithuanian scientist, Valerija Ėępulėtė (1904–1987). During 46 years of research, she deciphered different aspects of Quaternary stratigraphy, palaeogeography, the extent of the last glacial event (Weichselian) and deglaciation in Lithuania, studies that provided the framework for her 1968 doctorate degree in geology. Ėępulėtė’s primary interests were concerned with the development of geomorphological terminology and the methodology of geomorphological mapping in relation to the last glaciation – aspects that have influenced Quaternary research both in Lithuania and elsewhere.

A former petroleum geologist, van Veen at the Department of Earth Sciences, Technical University Delft, The Netherlands, writes about early ideas on glaciation in The Netherlands, using as his ‘spyglass’ the several essays on ideas about glaciation that were submitted as entries to prize competitions organized by the Hollandsche Maatschappij der Wetenschappen (Holland Society of Sciences) and the Teyler Genootschap (Teyler Society). In the nineteenth century, such prize competitions were popular, with questions being posed about controversial scientific problems, for which there were no known or definite answers. Several prize topics had to do with glacial theory, and eventually they yielded a submission from the Swedish geologist Otto Torell that clinched the land-ice theory for geologists in Germany and northern Europe, although he did not originate this idea. van Veen’s paper is helpful in that he delves into a corner of the history of geoscience that can only be studied by someone fluent in Dutch and with access to rare publications in that language.
Zhang, Associate Professor in the Department of Earth Sciences, Sun Yat-sen University, Guangzhou, China, is involved in research in tectonic geomorphology, particularly in the Ordos Plateau area of north China. He documents the punctuated progress of 100 years of investigation of Tertiary (Neogene) planation surfaces in China. This began with the pioneering work of Bailey Willis in 1903–1904, who introduced the Davisian idea of erosion cycles and recognized peneplain remnants in north China. Zhang documents studies of planation surfaces in that part of the world between 1910 and the 1940s, in which scientists arrived at different and similar conclusions as to their ages, and the discovery of similar surface remnants in south China, prompted by the inland migration caused by the southward advance of Japanese forces prior to and during World War II. Since the 1970s, restudy of planation surfaces over the whole of China has been undertaken with the contemporary focus being on uplift of the Tibetan Plateau and its impact on the environment.

Thick marine Quaternary sediments underlining the Kanto Plain of Honshu, Japan are regarded as having been deposited in a ‘Palaeo-Tokyo Bay’, a concept first proposed by Hisakatsu Yabe (1878–1969) in 1913 and 1914, based on molluscan fossils that record changing sea levels during the glacial and interglacial periods. Yajima, from the Tokyo Medical and Dental University, describes how this now-accepted idea came about in conjunction with resolution of the question of Pleistocene glaciation in Japan and a change from Palaeo-Tokyo Bay being open to the east and, at the time of the high sea-level phase, and perhaps also to the south, to modern Tokyo Bay which opens to the south.

Branagan, at the School of Geosciences, University of Sydney, Australia, is the doyen of historians of geology in Australia. His paper gives a comprehensive account of the earliest studies of the rocks of the Australian coastline and, subsequently, in inland regions as explorers pushed into the continent’s interior. There they encountered many hardships in the desert regions, but were also able to make numerous observations of Quaternary deposits in well-exposed country. In the first stages, all the information was transferred to ‘centres of calculation’ (cf. Latour 1987, chap. 6) in Europe and published there. Subsequently, geologists began to settle in Australia and publish there, even though they had mostly been trained in Europe. Thus, Branagan’s paper illustrates the first two stages of ‘Colonial Science’, as envisaged by George Basalla in his threefold classification of the stages of development of science in European colonies or settlements (Basalla 1967). In the early work described by Branagan, much of Australian geology was, as might be expected, seen through European eyes.

Twidale is a geomorphologist of international standing and Honorary Visiting Fellow in Geology and Geophysics at the School of Earth and Environmental Sciences at Adelaide University, Australia. He is the author of numerous books on geomorphology and has a special interest in desert landscapes, particularly in Australia. His paper provides much information about the character of Australian deserts (which are notably different from those in North Africa or China, say, and can at times provide brief nutriment for abundant life forms) and the early history of exploration in these mostly inhospitable regions. He focuses on the history of the study of the dunes of the Australian deserts, and those of South Australia more particularly. Like Orme, Twidale also discusses dating problems – but as regards individual sand grains using thermoluminescence methods as opposed to the radiometric studies of materials chiefly used in studying the pluvial lakes of the American West. Some of the Australian dunes are only a few thousand years old and thus presumably Holocene in age. The dating of the lake-fill and alluviation of the Australian desert lakes has furnished information about the histories of past climates, which is relevant to biogeography and contemporary problems of climate change.

Oldroyd, Honorary Visiting Professor at the University of New South Wales, Sydney, Australia, is a historian of science who has written chiefly on topics to do with the Earth sciences. In his contribution, he examines the history of ideas about the landforms of the Sydney Basin, with special reference to the patterns of the area’s rivers, and considers two important early twentieth-century Australian geologists: Ernest Andrews and Griffith Taylor. Both men were keen disciples of the evolutionary ideas about landforms developed by Davis in the United States, whose ideas were enthusiastically applied in Australasia. From the appearance of a geological map, the geology of the Sydney area appears very simple, but detailed work has revealed numerous difficulties and complexities. Early studies of the area relied heavily on geomorphological considerations, which were used to try to probe the tectonic history of the area. Unfortunately, surfaces that were construed by Taylor and Andrews as being the relics of Davisian peneplains were (we think) depositional surfaces or ones that were generated according to the relative hardiness of nearly horizontal sedimentary strata. Oldroyd shows that, even now, there is no full consensus about the history of the Sydney Basin, and that debates on the matter still depend to a considerable degree on geomorphological evidence.
Mayer, at the Department of Earth and Marine Sciences, Australian National University in Canberra, is an authority on early scientific voyages to Australia, especially those conducted by French explorers. His paper relates to the Tamala Limestone, a well-known Quaternary formation that crops out extensively along the coast of Western Australia. Work continues on the study of this unit, which presents problems with respect to sea-level and climate change, and the role of tectonic deformations of the crust in that part of the world. It is noteworthy that the early investigators were struck by the indications of changes in the relative levels of land and sea, which was a ‘hot topic’ in the early nineteenth century. Other matters of interest were the discovery of the ‘living fossil’, Trigonia, which gave comfort to those opposed to the notion of transmutation because it indicated the possibility that various organisms no longer extant in Europe might turn up one day in remote parts of the world; and to certain enigmatic calcareous structures that might have had various origins. It was Darwin who first suggested that the Tamala Limestone was of aeolian origin.

From New Zealand, a biography of the internationally known geomorphologist Sir Charles Cotton (1885–1970) published in 1922 as well as numerous pioneering papers on a great variety of subjects in geomorphology. Although much of Cotton’s earlier work followed the ideas of W. M. Davis in terms of an explanatory description of landforms (structure, process, form), he also emphasized, in a qualitative way, the importance of climate change and tectonic movements in landscape-forming processes. Cotton could not have been in a better place to develop these ideas than in New Zealand, a tectonically active country where the continuing relationship between geologically recent Earth movements, denudation, erosion and associated deposition is particularly obvious.

Brook, from the School of People, Environment and Planning, Massey University in the North Island of New Zealand, a geomorphologist interested in geomorphic controls of landscape evolution, writes on the role of an amateur in geology. His paper recounts the pioneering studies of George Leslie Adkin (1888–1964), a farmer, amateur geologist and well-known ethnologist, whose observations provided evidence for localized glaciation and uplift in the axial Tararua Range of the North Island. Adkin’s work was published in 1911 and helped resolve a controversy over the extent of glaciation in New Zealand, although his ideas, in this instance, ran counter to those of Charles Cotton. Later studies have broadly vindicated Adkin’s work. It has been postulated that cooling of some 4 °C could have occurred during the Pleistocene, resulting in a lowering of the glacier equilibrium line by approximately 670 m, substantially below the crest of the Tararua Range.

We wish to express our sincere thanks to the valuable comments on a draft of this Introduction made by the contributors of this volume. We are particularly grateful to R. Twidale, A. Orme and V. Baker for their considerable input.

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An Introduction


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