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ESEX Commentary

The changing landscape of geomorphology

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Mike Church has provided a view of recent changes in geomorphology, especially regarding the growing role of geophysicists and the diminishing role of 'geographical geomorphologists' in the discipline (Church, 2005). Here I offer a different diagnosis and prognosis based both on my interpretation of the changing research priorities in geomorphology, and on experience of having collaborated on geomorphological research programmes with geophysicists and geologists over the past two decades.

These are certainly interesting times for geomorphology: from a discipline regarded by the great majority of Earth scientists as being, at best, of peripheral interest and having as its subject matter the understanding of a trivial property of the Earth (the morphology of its landsurface), geomorphology has now become a prominent focus of research for those concerned with how the Earth works. This is evident in numerous recent meetings involving 'geographical geomorphologists' along with geologists, geochemists and geophysicists, as well as the rapid increase in the number of geomorphological papers appearing in interdisciplinary and general Earth science journals – a few years back it would have been difficult to imagine *Nature* publishing three geomorphological papers in a single issue (Burbank *et al*., 2003; Dadson *et al*., 2003; Reiners *et al*., 2003). This growing prominence of geomorphology is also well captured in Peter Koons' observation about the traditional approach of geophysicists and structural geologists to the understanding of orogenesis: 'In retrospect, it is difficult to understand how models of collision zones could be constructed without reference to the inevitable topography but somehow we managed' (Koons, 1995). In short, earth scientists, and particularly geophysicists, have decided that geomorphology matters: so how has this 'geomorphological turn' come about?

My own interpretation has already been summarized (Summerfield, 1996, 2000), so here I will just highlight some of the points where I differ from Mike Church. Although the ready availability of the computing power necessary for numerical simulations of coupled tectonic–surface process models of landscape evolution has certainly been important (Beaumont *et al*., 2000), two other developments have been critical. One was the arrival of digital elevation models (DEMs) at ever-increasing resolutions and growing coverage which now enables the rapid quantitative analysis of topography up to continental scales (Montgomery, 2001; Montgomery *et al*., 2001). Geophysicists were not much interested in the labour-intensive construction of DEMs from existing topographic maps, but have avidly employed the digital elevation datasets now widely available. The second key development was the appearance of techniques that allowed long-term rates of denudation (and therefore crustal mass redistribution) to be quantified. The most significant of these has been low-temperature thermochronology (Ehlers and Farley, 2003; Gleadow and Brown, 2000), and in spite of the initial confusion as to whether the information provided referred to 'uplift' or denudation (Summerfield and Brown, 1998), the cooling history of rocks moving towards the Earth's surface as a record of regional-scale spatial and temporal patterns of denudation has now become the primary basis for empirically constraining numerical models of long-term landscape development (Brown *et al*., 2002; van der Beek *et al*., 2002; Willett *et al*., 2003). More recently, cosmogenic isotope analysis has assumed increasing importance as a means of refining the coarse resolution of thermochronology by providing denudation rate estimates at shorter temporal and smaller spatial scales, but still over time scales sufficiently long to be of relevance to models of long-term landscape development (Cockburn and Summerfield, 2004). Techniques applicable to different time scales are now being combined to address the long-standing issue of the long-term temporal variability of denudation rates (Burbank *et al*., 1996; Cockburn *et al*., 2000), and cosmogenic isotope analysis is providing the missing link between modern denudation rate studies and the geological time scale perspective.

Although quantitative denudation histories are crucial as constraints on numerical landscape development models, a further key variable is elevation since this must be predicted if the change in topography through time is to be defined. Whilst some geophysicists have flirted with the notion (long-abandoned by most geomorphologists) of 'erosion

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surfaces' being a basis for empirically constraining the vertical displacement of the landsurface relative to sea level (Gurnis *et al*., 2000), a range of palaeobotanical, isotopic and other palaeoaltimetric techniques have been developed that can provide useful elevation constraints on palaeotopography in some situations (Chamberlain and Poage, 2000; Gregory-Wodzicki, 2000; Sahagian *et al*., 2002; Spicer *et al*., 2003).

That geophysicists and geochronologists rather than (with some exceptions) 'geographical geomorphologists' have seen the value of these techniques as a means of re-examining long-standing models of landscape evolution is largely a matter of scale; the 'quantitative' shift of geomorphology from the 1950s led to a progressive abandonment of questions about landscapes at the large scale and over the long term (since the techniques to provide the required quantification were not then available), and the effective re-definition of the discipline as the study of surface processes at small spatial and temporal scales. The naming of this journal squarely reflected this realignment (the later addition of 'landforms' to the title being somewhat cosmetic). With this focus on the small scale and the pervasive view that process in 'process geomorphology' excluded endogenic mechanisms (Embleton and Thornes, 1979), 'geographical geomorphologists' were poorly prepared to engage with the technical developments from the 1980s that have revolutionized our ability to address quantitatively questions about landscape evolution. Such questions *have* to include an understanding of tectonic as well as surface processes, and the dominant theme that is now emerging in landscape evolution is the relative roles of tectonics and climate, a debate that has intensified with the idea that denudation can be a significant driving mechanism of deformation in orogens (Beaumont *et al*., 2001; Lamb and Davis, 2003; Zeitler *et al*., 2001).

Rather than focusing on the physics underlying the relevant processes, the primary aim in such studies is to apply basic physical principles in order to understand the temporal and spatial characteristics of specific cases of landscape evolution. Geophysicists are not interested in the Himalayan–Tibetan Plateau orogen simply as an example of 'Himalayan-type deformation'; rather they have focused on the specific space and time contingencies involved and how these have expressed the underlying mechanisms of continental convergence. Certainly there are attempts to create general models (such as critical wedge models for simple orogens), but the real challenge is applying these successfully to individual cases which can be adequately constrained by empirical data. Hence the plethora of models to explain the uplift of the Tibetan Plateau that are extant because we still do not have sound empirical data recording its change in elevation through time.

So where can 'geographical geomorphologists' fit in here? First, they could provide a health check on some of the geophysics-based modelling studies that are being undertaken. With their field experience and feel for the variables likely to be important in landscape development, if not their high-level mathematical or modelling skills, they could be more prominent in questioning some of the assumptions employed – for instance, in modelling the effects of glacial erosion (Tomkin and Braun, 2002) or proposed links between mantle plume uplift and increased erosion rates and sediment delivery (White and Lovell, 1997). By interacting more energetically with those outside the traditional geomorphological community who are now 'doing geomorphology', they could also assist the efficiency of the overall research effort by minimizing instances of re-inventing the wheel; in a recent example, a discussion of the response of landscapes to perturbations (Allen, 2005) bears some remarkable similarities to the ideas of Brunsden and Thornes (1979) and others presented more than a quarter of a century earlier. Second, 'geographical geomorphologists' could also explore much more actively the border territory between 'field-scale' processes and landscape-scale aggregate process relationships – for instance, between the details of sediment entrainment in a river, and the generalized representation of fluvial erosion in large-scale models that usually depend on some formulation of the stream power law. Third, there could be a contribution through providing a much better understanding of landform elements whose behaviour is key to the overall development of the landscape; hillslope–channel linkage, knickpoint behaviour, escarpment retreat and bedrock channel erosion are examples. This research agenda will require the application of the range of geochronological techniques now available that can provide estimates of process rates and landform behaviour over time scales that are actually relevant to numerical models of long-term landscape evolution. No longer do we always need to make untenable temporal and spatial extrapolations from short-term, small-scale measurements.

Such an engagement is only feasible in research teams where complementary skills enable the appropriate techniques and modelling to be deployed. This is becoming more and more evident in the published literature where multidisciplinary teams involving 'geographical geomorphologists', geochronologists, geologists, geophysicists (and even occasionally nuclear physicists) are tackling geomorphological problems. In short, those undertaking geomorphological research from a background in physical geography (and therefore presumably with a reasonable knowledge of related aspects of, for instance, hydrology, pedology and biogeography) can play a valuable role as part of a multi-disciplinary team. This, of course, does not exclude the contribution by geomorphologists to environmental issues envisaged by Mike Church, but there is enormous scope to advance geomorphology as a whole at probably its most exciting time since it emerged as a distinct discipline – and I have not even mentioned the dazzling research frontier in planetary geomorphology.

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