Is it possible to predict the past?

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On p. 343–354 of this issue Engelder and Pelletier (2013) argue that relatively long-term autogenic variation in the degree of channelization of gravel-transporting fluvial systems can result in cycles of aggradation and incision that can reduce the slopes needed for gravel transport and hence allow for unexpectedly long distances of gravel runout. The basic autogenic mechanism has been described by Kim and Paola (2007) and Van Dijk et al. (2009) in experiments involving active braided river systems. Whether one agrees with the main conclusions of Engelder and Pelletier or not, the paper proposes to add a new facet to the classical problem of what we can and cannot reconstruct about past transport systems from their deposits.

There are many reasons why we would like to reconstruct past environments as accurately and quantitatively as possible. As part of understanding our planet's history, it is an end in itself. Once we know, for example, that we are dealing with an ancient river system, it is natural to begin taking its measure: how deep was the flow? How wide, how fast? Was this a major trunk stream or a minor offshoot? Where are we in the larger scheme of things, i.e. the sediment-routing system (Allen, 2008)? These quantitative questions seem fairly natural—probably most curious lay people would readily understand why we ask them. By comparison, taking the measure of paleoslope seems a little… technical, to put it kindly. The first-order observation about continental hypsometry is that, outside of orogens, the continents are pretty close to sea level; one consequence of this is that, over most of the continents, the slopes of surface topography are pretty low. For alluvial rivers, afforded the luxury of building themselves only as much slope as they need to transport their cargo of gravel, sand, and fines, a slope of one part in a thousand is almost precipitous. For big coastal rivers, the slope falls to a few parts in a hundred thousand. With values like these, it's not hard to see that even without the effect proposed in Engelder and Pelletier, slope and relief per se are unlikely to impose much of a limit on the transport distance of gravel out from its source: even at the high end of the range of slopes, a kilometer of relief would suffice to deliver the gravel to a distance on the order of 1000 km. Building the required transport slope becomes even easier if we include the "pumping" effect of autogenic transport variations presented in Engelder and Pelletier. Thus, it is hard to see slope as a major limiting factor in long-distance gravel transport; rather, in depositional systems the main limitation on how far gravels can get from the source is preferential loss of gravel to deposition—in other words, the gravel supply is simply used up. Hence the emphasis in Engelder and Pelletier, and in our work on this topic (Heller and Paola, 1989; Heller et al., 1988; Paola et al., 1992), on post-orogenic conditions as fundamental in producing long gravel run-out.

Looked at another way, though, the slope problem becomes more interesting. If the relatively steep ramp described above is built entirely of sediment, then we have a pile a km or so thick near the source—an amount of sediment that even the less field-seasoned among us would find hard to miss. If, taking care about getting the correlations right, we find that the requisite thickness is lacking, we have to start thinking of other mechanisms, and here we arrive at an interesting intersection of sedimentology and tectonics. The intersection is nicely illustrated by Engelder and Pelletier's coupled post-orogenic foreland-basin model. Reliefs in the order of tens to a couple of hundred meters and length scales of hundreds of km—the scales associated the upper end of sedimentary ramp slopes—also fall into, for example, the ranges associated with flexure near and outside of the forebulge. So if we can get paleoslopes right, even at the factor-of-two level claimed in our earlier work (Paola and Mohrig, 1996), we can start putting useful constraints on flexure as it applies outside the domain of classical subsidence modeling (Beaumont, 1981; Jordan, 1981) in the more distal reaches of foreland basins and/or under post-orogenic conditions (McMillan et al., 2006). The basic idea is pretty simple. Thanks to decades of work, mostly in the engineering and geomorphology communities, we have a pretty good idea of what kind of tractive forces it takes to move gravel (and just about any other non-cohesive sediment) under flowing fluid. If—and this is a pretty big "if"—we are prepared to ignore the effects of local accelerations, mostly associated with topography in the channel, then a simple force balance tells us that the main variables that set the tractive force (shear stress) are the mean flow depth and slope. Paleoflow depth can often be constrained from the deposits, leaving us with a fairly simple, if not very precise, way of estimating the transport slope. Slope values estimated this way fall in the ranges discussed above that typify alluvial rivers. But this is where Engelder and Pelletier would urge caution; their autogenic mechanism implies that even the fairly low slopes estimated using the simple force balance could be systematically too high.

My own inclination, perhaps not surprising given that I had a hand in developing and applying the simple paleoslope estimation method described above, is to be skeptical about the importance of the autogenic effect Engelder and Pelletier have identified in affecting overall transport slopes. But I am hardly unbiased, and certainly they have made a case for an effect of transient conditions on slope estimates that is worth further study. The broader issue raised by the Engelder and Pelletier paper, though, is the accuracy with which we can reconstruct past physical conditions from the preserved physical record. (Reconstruction of past conditions from geochemical and other proxies is outside our scope here.) Over the years, there has been a variety of suggestions that the preserved record is, in one way or another, not representative of typical environmental conditions during the time period in question. One common line of reasoning here has been that most of the action takes place during rare events (Ager, 1993a, b)—the *N*-year storm or flood, some other extreme transport event, or perhaps a short-lived tectonic pulse. And certainly we know that extreme events can account for a disproportionate amount of change—for instance, the record of elevated sediment delivery from the Chi-Chi earthquake in Taiwan (Hovius et al., 2011). We also know that the stratigraphic record is nearly always incomplete, riddled with gaps at all scales (Plotnick, 1986; Sadler, 1999). One of the valuable aspects of the Engelder and Pelletier paper is that it proposes a different kind of "event," one not usually thought of in the literature on preservation biases in the stratigraphic record: an autogenic event in which a disproportionate share of transport occurs under conditions (transient zones of spatial acceleration) that are not preserved in the record. But whether the events are external or internal to the transport system, we end up with the same problem: what we see in the record may not represent the typical conditions of the interval we are trying to reconstruct. If only a vanishingly small fraction of everything that took place in a given location is preserved, then what is preserved must be, in some sense, atypical. Bailey and Smith (2010) have nicely described what is preserved as a series of "frozen accidents." There appear to be three possible scenarios for these accidents: (1) that deposition occurs during rare, outsize transport events and enough of a record of these events is preserved for us to estimate their magnitude; (2) that deposition occurs during rare, outsize events but their record is ambiguous, or perhaps erased and overprinted by mundane events; and (3) that deposition occurs under fairly ordinary conditions, and other mechanisms account for the gaps—for example, "supply" events in the catchment (Hovius et al., 2011), or episodicity in subsidence and net preservation.

In the first scenario, where the extreme event leaves traces that record its unusual magnitude, we can estimate the extreme event and attempt to place it in context. The problem arises in the second case, when the rare, high-magnitude events do not leave a clear record—in part, the concern raised by Engelder and Pelletier. I do not claim to be an experienced field sedimentologist but from what I have seen of fluvial sedimentary deposits, on the whole they imply conditions that I would describe as fairly "normal." The flows implied by, for example, common cross-stratified fluvial sands are ones you could easily encounter in sand-bed rivers in the world today during a typical bankfull flood. One explanation, nicely illustrated by Sambrook Smith et al. (2010), is that in terms of transport conditions, large floods are just not that different from more normal floods. A major reason for this is that once a channel reaches its bankfull state, most additional discharge goes to spreading the flood laterally over the floodplain rather than making the flow in the channel deeper and stronger. If this is the case, then it is not so crucial to know what flood frequency we are reconstructing from preserved deposits because, in terms of transport conditions if not of total water discharge, the events are not that different from one another.

Another, complementary, possibility is that we have been thinking too narrowly about what constitutes a rare event. It is perfectly possible to construct an extremely unlikely event as a sequence of independent events, none of which is all that unlikely in itself. We have tended to ask if, for example, the deposit we are looking at represents the "10,000 year flood," focusing on only one variable. If preservation in the long-term record requires a combination of circumstances, it might be better to think about the composition (coincidence) of, for example, events like floods, with longer-term "events" like channel avulsions (which move the locus of channel sedimentation from place to place; Slingerland and Smith, 2004), periods of increased sediment supply (Goodbred and Kuehl, 2000), and periods of increased subsidence. Even at the very simplistic level of "on" and "off," each of these sub-events has its own intermittency (the fraction of time when it is "on"). If the events are independent of one another—plausible, but not to be taken for granted—then the intermittencies multiply. We need only require coincidence of three independent events of intermittency 0.01 (the order of magnitude for a standard annual river flood) to get a total intermittency of 10^{-6}, and a record that starts looking pretty sparsely represented.

These are all arguments that the stratigraphic record is not as unrepresentative as its gap-dominated nature would imply. It provides support for people who, like me, want to take the stratigraphic record more or less at face value and reconstruct conditions from it. This approach is questioned by people who highlight the many ways in which we could be wrong for causes that one can describe and model but that leave little or no trace in the record. I would like to close this essay by proposing a kind of truce, and a way forward. It has been said that one cannot make "predictions" about the past, and while that is certainly true in a literal sense, I think it is misleading. It is perfectly possible to make testable assertions about as-yet unobserved properties of ancient deposits, and it seems to me that these are logically if not semantically equivalent to predictions about bosons or velocity profiles or anything else that we can observe in the world around us. The oil industry, of course, does this all the time, using a variety of techniques and no small amount of experience and intuition. It seems to me that we in academia have a lot to learn from industry in this regard. It is hard to see, in the long run, how quantitative or qualitative statements about past conditions can be checked if they do not lead to assertions about the record itself that can be compared with observations. A good example of this is ~~a~~ recent work by the Imperial College London group (Duller et al., 2010; Whittaker et al., 2011) in which measurements of stratal geometry are used to predict grain-size trends in the same rocks: one set of measurements is being used to predict another, independent set of measurements in the same unit. I believe there are many other combinations of variables that could be put together in this way, but the common theme is that the science of Earth's past is not fundamentally different from that of Earth's present.

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