Forum Comment

doi:10.1130/G38929C.1

Preservation or piracy: Diagnosing low-relief, high-elevation surface formation mechanisms

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Whipple et al. (2016) take issue with the proposed genetic mechanism presented by Yang et al. (2015) for the formation of high-elevation, low-relief landscapes. Whipple et al. correctly point out the need to examine competing models to determine how existing data can differentiate between them, going on to present and analyze a new model purporting to simulate the mechanism proposed by Yang et al. Unfortunately, this new model omits much of the physics essential to the original model. The result is a strawman argument in which the failure of one model is presented as the success of an alternative, whereas the original Yang et al. model has remained untested. Below, I explain the deficiencies in the proposed model and what a proper analysis might reveal.

Yang et al. proposed that elevated, low-relief surfaces form as a complex system response to an external forcing that perturbs the geometry or topology of a river network. The forcing can be surface strain, differential uplift or exhumation of variably erodible rock, but the response always involves topologic change of the river network including what is referred to as the "area-loss feedback" (ALF) (Willett et al., 2014), whereby a drainage basin that has lost drainage area will increase in elevation due to its reduced erosion rate. Increased elevation and lower erosion rate increases vulnerability to additional drainage area loss, triggering a cascade of subsequent captures and inward drainage divide migration. Essential to this model is the multi-step response with positive feedback, involving not just the victim basin, but also its neighboring basins. Feedback also implies a response time of many multiples of the characteristic time of individual basins (Goren et al., 2014). For these reasons, Willett et al. (2014) and Yang et al. (2015) carefully and repeatedly refer to this process as a feedback, not a forcing or direct response.

Whipple et al. purport to test the hypothesis of Yang et al. with 1-D and 2-D models of a basin subjected to a loss of erosional power, simulating a tributary experiencing an increase in its local base level. However, their model is for a single drainage basin subjected to a single perturbation. There are no neighboring basins, therefore no subsequent drainage area loss, no area-loss feedback, and the analysis is limited to less than the response time of the tributary. The area-loss feedback that is the defining mechanism of the Yang et al. model has been completely omitted.

There are four models presented between Whipple et al. and Yang et al. that can be evaluated with respect to observations: (1) the ALF model of Yang et al., (2) the preexisting surface (PES) model of Whipple et al. (their figure 1A), (3) a modified PES model in which transient perturbations are dispersed by non-uniform forcing or physical properties, and (4) the strawman model of Whipple et al.'s figure 1B. Several proposed characteristics are difficult to evaluate based on current data. Clark et al. (2006) identified surfaces using elevation as a primary criterion and specifically excluded alluvial valleys, so these data cannot be used to argue for planarity, or to identify potential early-stage surfaces at moderate elevations, one of Whipple et al.'s first suggestions. The strawman model predicts high-relief rims to low-relief catchments, a feature not ubiquitously observed, leading Whipple et al. to reject this model. However, these features are not predicted by the ALF model as the subsequent inward divide migration erodes these rims, and the longer timescales inherent to the model permit reduction of relief (Willett et al., 2014).

Perhaps the most interesting characteristic suggested by Whipple et al. is the coincidence of a water-divide and the edge of a low-relief surface, which I refer to as coincident divides. The Whipple et al. strawman model has no divide mobility, so makes no predictions. The strict PES model does not allow coincident divides as these imply differential erosion rates and divide motion, which is not consistent with the hypothesis of an old, equilibrated surface. Contrary to the contention of Whipple et al., the ALF model does not require that a low-relief surface be completely bounded by coincident divides. It requires only that part of the surface boundary be a divide. Provided parts of a basin are losing area, the feedback is triggered and the basin will evolve toward lower relief. In addition, as water divides in the ALF model are advancing inward, I expect additional captures of rivers on the surface, so that at any point in time, it is not unexpected to see multiple basins draining a single surface. Therefore, the coincidence of a water divide and a low-relief surface edge is not an exclusive diagnostic of either the ALF or the modified PES models, but its existence precludes the strict PES model and its complete absence, i.e., a surface with no coincident water divides, would preclude the ALF model. The observation in this region is that every elevated surface has at least one coincident divide. The surfaces in Yunnan are almost exclusively single catchments completely surrounded by coincident divides. The surfaces in Tibet have more complex drainage patterns, but even at higher elevations, where glacial and paraglacial modification is likely, there are no surfaces that are not at least partially bounded by a coincident divide. We can conclude that the strict PES model is precluded, the ALF model is permitted as an exclusive model and the modified PES model combined with a component of ALF is permitted. The high frequency of coincident divides, particularly at lower elevations supports an important role for the ALF mechanism.

The other important data are the normalized river profiles, which make a distinction between the PES model that predicts internally concordant channel profiles and the ALF model which predicts discordant profiles. Yang et al. investigated this in detail and found exclusively discordant profiles even at the scale of tens of kilometers, explained in pattern by the ALF model. Whipple et al. claim that a modified PES is consistent with the river profiles, but offered analysis only at the 100 km to 1000 km scale, with no analysis of individual profiles or drainage basins in support of this contention. Conclusions based on river profiles are thus identical to those based on river pattern analysis: these data can be fully explained by the ALF model, are inconsistent with a strict PES model, but could be consistent with a modified PES model, pending a detailed study. The regional analysis (Whipple et al.'s figure 3) does demonstrate that if rivers continued to steepen as hypothesized, channel heads will reach elevations in excess of 8 km, making this the highest topography on Earth.

Finally, I note that there is room for common ground. As Yang et al. acknowledge at the end of their paper, I cannot preclude a regional increase in uplift rate in the recent geologic past. ALF is a feedback, not a driver, and although tectonics is almost certainly the driver, the form of that forcing and the initial condition are not specified. My point was, and remains, that large perturbations to a landscape will induce changes in the river network topology, thereby triggering the area-loss feedback. ALF will play a role, perhaps dominant, perhaps subordinate, in modifying the transient landscape. A better way of framing the question of landscape evolution in southeast Tibet is not in terms of exclusive end-members, but in terms of the relative importance of specific mechanisms.

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