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Discussions

Interactive comment on "Quantifying the roles of bed rock damage and microclimate on potential soil production rates, erosion rates, and topographic steepness: A case study of the San Gabriel Mountains, California" *by* Jon D. Pelletier

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This manuscript by Pelletier is focused on a reinterpretation of soil production rates measured in the San Gabriel Mountains, CA by Heimsath et al. (2012), and in particular the controls on the intercept of the soil production function, P0 in the present manuscript, which is the peak conversion rate from rock to soil and a key parameter that determines the presence and thickness of soils in upland landscapes.

Heimsath et al. (2012) used soil production functions determined from low and high-uplift zones in the San Gabriel Mountains to show that P0 is higher in rapidly eroding areas with steep slopes and high local relief, with the implication that soil production rates increase in concert with regional erosion rates to help sustain a soil mantle in steep landscapes. Implicit in the claim of Heimsath et al. (2012) is the notion, supported by data, that for the San Gabriel Mountains, spatial gradients in rock uplift rate and thus relief are dominant compared to variations in rock material properties or climate, which also likely influence soil production rates (See for example Owen et al. 2011 ESPL, 36: 117-135). The Heimsath et al (2012) sampling strategy was designed specifically to minimize climatic and lithologic differences between sites.

Here, Pelletier proposes that the variability in P0 present in the San Gabriel Mountains (SGM) dataset is due instead to spatial variations in rock material properties driven by faulting-related damage and aspect-dependent microclimate. While it is well worth exploring how rock properties and microclimate may influence soil production rates both in the SGM and elsewhere, this paper presents no data on either and the analysis provides scant evidence to support the proposed re-interpretation. Moreover, the theoretical underpinnings of the empirical microclimate-fault-damage "model" are tenuous at best. In the absence of either sound theory or any new data, we cannot recommend this manuscript for publication. Below we articulate our 4 main concerns as concisely as possible without belaboring the points:

(1) Poor theoretical basis for Equation 2. The core of the paper is centered on an empirical expression for P0 as a function of a damage index D and microclimate A. However, it is completely unclear how these parameters are connected to the actual process of soil production. For example, the damage index is based on empirical studies of fracturing in bedrock as a function of distance from a fault over scales typically <100m. There is no reason to believe these relationships can be meaningfully applied at landscape scale. Moreover, why should the relationship between fracture density and P0 be linear? There are no data to substantiate this, and no mechanistic justification. Indeed one might anticipate a non-linear dependence. Second, the microclimate factor "A", is defined as a function of the product of aspect and slope. Again, there is little

justification for this treatment beyond a weak empirical fit to the Heimsath et al. (2012) data. A major concern here is that the microclimate factor is strongly slope-dependent , and thus cannot be untangled from the slope controls proposed by Heimsath et al. (2012). Additionally, "microclimate" is obviously a factor that modifies the local "macro-climate" (Precipitation and Temperature) but there is no representation of these factors in the analysis.

(2) Inappropriate data handling. Computing P0 values from every single measurement of P is unwise. Normal practice is to collect many measurements of P under different thicknesses of soil, thus defining the "soil production function" and enabling estimation of P0 via regression analysis. Heimsath et al. (2012) only felt that their data justified two estimates of P0. One might attempt a finer-subdivision of the data as a function of rock properties, microclimate, or catchment-mean erosion rate to perhaps generate a handful of potentially robust P0 estimates, but what is done here in this manuscript has little value. The scatter in P0 values obtained in this manner is not particularly meaningful – it only reflects in a less-than-quantitative way the uncertainty in our ability to determine what P0 is and how it might vary with environmental conditions.

(3) Suspect empirical basis for Equation 2. Ultimately such an empirical approach might be justified if the proposed driving factors were quantified and showed a sound correlation with P0. However, this is not the case. The damage index D is determined from fault traces on existing geologic maps, and likely has little bearing on the actual pattern of rock damage. At a minimum, we expected to see at least some validation of this ad hoc treatment beyond the extremely weak correlation presented in Figure 3A. (as a side note, there is no justification for throwing out the cluster of five points described on Page 5, Line 20 besides "area with an unusually high density of mapped landslides"). The empirical basis for the microclimate factor is similarly weak, particularly given that any correlation is likely due to the slope-dependence (which may lead to a spurious interpretation of climate control due to its primary dependence on rock uplift rate). It should also be noted that there is not a strict correspondence between

precipitation (which varies primarily with elevation) and rock uplift rate – the presence of elevated, low-relief surfaces in the San Gabriel Mountains allows the decoupling of potentially confounding climatic and tectonic controls on soil production (see Dixon et al., 2012 EPSL).

(4) Weak logic chain in discussion. Even ignoring the limited theoretical and empirical basis for Equation 2, and the inappropriate handling of local measurements of P, the discussion does not present any particularly useful new insight. The discussion of focused tectonic uplift in regions with high P0 (Page 10, Line1-9) is off base and may be confusing to many readers. It is true that at the scale of an orogenic wedge rock uplift rates – and slip rates on associated faults – may increase in response to rapid erosion (e.g., Willett, 1999), but this feedback simply cannot operate at the scale of the SGM block. It is simply invalid to argue that differences in P0 driven by proximity to faults and microclimate can enhance erosion rates and thus induce more rapid uplift and create the steeper topography of the eastern SGM. There are very good structural reasons to expect more rapid tectonic rock uplift at the eastern end of the SGM. This more rapid rock uplift produces the higher elevations, greater relief, and drives more rapid erosion. Certainly until proven otherwise this should be considered the most likely and simplest scenario. Once this gradient in elevation, relief and erosion rate is established, there will be associated differences in climate (precipitation and temperature) and potentially the degree of rock damage (though this is unproven with any data), which may have some influence on P0. Further study of these potential influences is certainly warranted, but useful progress will require the formulation of hypotheses based on a clear theoretical basis as well as the collection of data appropriate to testing these specific hypotheses.

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