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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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We stand by our criticisms of the manuscript in review. We do not offer a point-by-point response to the rebuttal of our review comments, but we do offer a few clarifications that may help.

1. Heimsath et al. (2012) did not attack any papers. They argued that models built on the previous paradigm would greatly exaggerate changes in critical zone processes in response to tectonic uplift and commensurate increases in erosion rate. The previous paradigm is that P0 is dictated by climate and rock properties and is independent of

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erosion rate. When that paradigm is used models require that soils thin as erosion rates increase such that landscapes become rocky and soil-free as soon as erosion rates exceed P0. Stripping off the soil is a rather dramatic change to the critical zone. One main point of Heimsath et al. (2012), described in point 2, is that soils in fact persist in areas of high erosion rate, and thus critical zone processes and conditions are much less sensitive to tectonics than predicted by most landscape evolution models that embed a fixed rule for soil production.

2. Heimsath et al. (2012) emphasized controls on P0, which they termed SPmax. In fact, the sentence in the author's rebuttal, "... the correlation between topographic steepness and erosion rates and the relative absence of cliffs, ... in the SGM is not inevitable but is directly related to the fact that P0 values increase with erosion rates", is a good summary of the main point made in that paper. The paper also expressed that this link has not been mechanistically explained – indeed it has yet to be replicated or confirmed empirically – and merits further investigation.

3. Given the inherent scatter in soil thickness and local P values (soil production rates) and the uncertainty in 10Be measurements, in most circumstances at least 10 P values are needed to define a robust soil production function, from which a single estimate of P0 and its uncertainty can be determined. In the approach used in the reviewed manuscript, local scatter attributable to many potential influences is interpreted in terms of variation in P0. This is unwise. At best one might hope to subdivide the SGM data into 2-5 subsets for evaluation of P0, had the data been collected to effectively sample across transects in the variables of interest, such as aspect. In preparation of Heimsath et al. (2012) we explored many potential subdivisions of the data to look at controls on P0, but settled on only having confidence in the two subsets presented. It is possible that further subdivision in terms of rock properties, climate (or microclimate), or erosion rate may indeed prove useful. However, additional data would be needed to refine estimates of P0 as a function of the variables of interest.

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