Heimsath and Whipple criticize me for not presenting a comprehensive theory for how rock damage and microclimate influence P_0 values (their point 1). I chose to focus the theory/modeling portion of my paper on the relationships among P_0 values, erosion rates, soil thicknesses, and topographic slopes, all of which I mapped across the central portion of the SGM as a test of my model. A lot of relevant theoretical concepts exist, well-known to everyone in the hillslope geomorphic community, regarding the relationships among weathering rates and the fracture densities of bedrock and intact regolith. For example, it is a textbook result that chemical weathering rates of fresh bedrock increase proportionally to the surface area available for weathering reactions, which depends linearly on the density of vertically oriented fractures. It is also well known that a higher density of vertically oriented fractures provides proportionally more incipient weaknesses that weathering agents acting on the contact between soil and intact regolith (or between soil and bedrock) from above (e.g., infiltrating water and tree roots) can exploit to produce intact regolith and soil (e.g., Langston et al., 2015). Regarding microclimate and its influence on wildfire intensity and severity, the fact that intense heating associated with wildfires can fracture bedrock and intact regolith in areas of thin or no soil cover is well established both experimentally and in the field, as the references demonstrate (Blackwelder, 1927; Goudie et al., 1992; Dorn, 2003; Shtober-Zisu et al., 2010). It is far beyond the scope of the paper to develop a comprehensive theory for how microclimate relates to vegetation cover, wildfire frequency and severity, and soil production rates, assuming such a theory is even possible. I will, however, add additional discussion of the available theory and concepts related to the relationship between soil production rates and bedrock fracture density to my revision.

Heimsath and Whipple's contention that P_0 values cannot be estimated point-by-point (point 2) is contradicted by the fact that the relative decrease in soil production rates with increasing soil thickness is nearly identical throughout the portions of the SGM sampled by Heimsath et al. (2012). My analysis assumes that there is only minor variation (compared to the 1.5 order of magnitude variation in P_0 values) in how soil production rates depend on soil thickness in the SGM. This is precisely what the data show: soil production rates in slowly and rapidly eroding portions of SGM with finite soil thickness (which include a wide range of aspects and distances from faults) are well represented by an exponential function of soil thickness with decay parameters that are almost identical, i.e., 0.027 and 0.031 cm^{-1}, based on Figure 3 of Heimsath et al. (2012). On a more practical level, I don't understand how we, as a community, could make significant progress on understanding the controls on P_0 values if we accept the logic of Heimsath and Whipple that only two P_0 values can be reliably determined from 57 CRN analyses.

Heimsath and Whipple criticize my inclusion of slope as a control on P_0 values on the basis that microclimate cannot thus be untangled from the slope controls proposed by Heimsath et al. (2012) (point 3). However, Heimsath et al. (2012) demonstrated a slope control on *soil production rates* (P values) whereas I am considering *potential soil production rates* (P_0 values). Slope exerts a strong control on P values because steeper slopes drive higher erosion rates (up to a maximum value of P_0), thinner soils, and hence higher soil production rates as a result of the generally inverse relationship between soil production rate and soil thickness. In contrast, as I explained on p. 3, lines 17-20, P_0 values "do not depend on soil thickness and its controlling factors; hence, they isolate the effects, if present, of environmental factors (e.g., water availability, vegetation cover, wildfire severity and frequency) and material factors (e.g., bedrock fracture density and lithology/mineralogy) that influence soil production rates." Slope appears as an input to the model of section 2.1 for south-facing slopes. Its inclusion is necessary to capture a real feedback in the natural system, i.e., that steep, south-facing slopes of the SGM, as the type habitat for chaparral (Holland, 1986), are prone to frequent, high-severity wildfires (Keeley and Zedler, 2009) that, in turn, have high rates of soil production if soil cover is thin or absent (Blackwelder, 1927; Goudie et al., 1992; Dorn, 2003; Shtober-Zisu et al., 2010). High rates of soil production, in turn, permit higher erosion rates and thus steeper landscapes because rock uplift rates increase where rates of erosional unloading are higher. I demonstrated that P_0 values are controlled by slope aspect, especially at the extremes of directly south- and directly north-facing slopes (Fig. 3B), so it is impossible that the dependence of P_0 values on microclimate is some type of spurious result based on slope gradient alone.

The conceptual model presented by Heimsath and Whipple (point 4) is missing a key element: soil production. They propose that more rapid rock uplift produces the higher elevations, greater relief, and more rapid erosion in the eastern portion of the SGM. Heimsath and Whipple mention soil production as a relevant process only once the gradient in elevation, relief and erosion rate is established based on structurally controlled spatial variations in rock uplift rates. However, *soil must be produced* in order for erosion to occur (in the absence of widespread landsliding within bedrock or intact regolith). If P_0 values are lower than rock uplift rates in granitic landscapes for a sustained period, cliffs, tors, and stepped topography form (e.g., Wahrhaftig, 1965; Jessup et al., 2010), features that occur in many granitic mountain ranges but that are especially common in arid environments where P_0 values tend to be low. The formation of cliffs, tors, and stepped topography may be enhanced by a feedback in which steeper slopes result in thinner soils and *lower* erosion and soil production rates, as evidenced by numerical models (e.g., Strudley et al, 2006) and global CRN datasets that demonstrate lower average erosion rates in bare bedrock landscapes compared to soil-mantled landscapes (Hahm et al., 2014; Figure 4). As such, the correlation between topographic steepness and erosion rates and the relative absence of cliffs, tors, and stepped topography in the SGM is not inevitable but is directly related to the fact that P_0 values increase with erosion rates. The central question, then, is *how* P_0 values and erosion rates are correlated. Is it that erosion rates control P_0 values, as Heimsath et al. (2012) proposed? Or, is it that P_0 values, controlled by bedrock material properties (e.g., lithology and fracture density) and the abundance of weathering agents/events (e.g., water availability, vegetation cover, wildfire severity and frequency), control erosion rates because erosion cannot occur faster than soil is produced, together with the fact that P_0 values and erosion rates have similar bioclimatic controls? I leave it for the reader to decide, but note that Heimsath et al. (2012) and Heimsath and Whipple have not identified a mechanism by which erosion rates might control P_0 values, nor have they evaluated the possibility that a factor besides erosion rates (e.g., climate, microclimate, lithology, rock damage, etc.) might control P_0 values in the SGM.

In my conceptual model I emphasized the importance of spatial variations in rock uplift rate (e.g., p. 9, line 16). These variations depend, of course, on fault and fold geometries but also on erosion rates because the erosional unloading of faults and crustal roots drives deformation and rock uplift (including both active and isostatic uplift). Heimsath and Whipple state that it is impossible for rock uplift rates to be influenced by erosion rates at the spatial scale of an SGM block (~10 km) (point 4). Since they provide no explanation, I can only guess what they might be thinking. If Heimsath and Whipple are referring to the fact that the rigidity of the lithosphere resists deflection in response to sufficiently small-scale spatial variations in erosional unloading, I agree in general but note that in a mountain range such as the SGM with pervasive crustal-scale faults, the effective elastic thickness of the crust can be as low as ~3-5 km and the associated flexural wavelength can be as low as ~5-10 km (e.g., Lowry and Smith, 1995). As such, spatial variations in erosion rate can certainly result in spatial variations in rock uplift rate at these scales. All numerical models of mountain belt evolution clearly demonstrate that erosional unloading affects the force balance on faults and crustal roots, which, in turn, affects spatial patterns of rock deformation and uplift.

Heimsath and Whipple argue that some of the relationships I claim to be linear might be nonlinear (point 1). My analysis explicitly included the possibility of a nonlinear dependence between P_0 and D (and also between P_0 and A) by fitting the data to a power-law function. The relevant text on this point (p. 6, last paragraph) is as follows: "To constrain the mathematical form of the relationships among P_0, D, and A, I performed a multivariate linear regression of the logarithms of P_0 to the logarithms of both D and A. Transformed in this way, the best-fit coefficients obtained by the regression are equivalent to the exponents of power-law relationships of P_0 (the dependent variable) to D and A (the independent variables). This regression yielded exponents of 1.1 ± 0.4 and 1.1 ± 0.3 for the relationships of P_0 to D and A, respectively. These values are sufficiently close to 1 that I chose to fix the values of the exponents to 1 (i.e., eqn. (2)) for simplicity and reanalyze the data to determine the value of c_1 that yields the best fit of equation (2) to data." The fact that the exponents of the best-fit relationship are 1 (within uncertainty) is a *data-driven result* that excludes the possibility of a significantly nonlinear relationship between P_0 and either D or A. Regarding slope aspect, I am using the *standard microclimatic index* used by many previous studies and that has been shown to correlate strongly with variations that depend on slope aspect and gradient (e.g., ground surface temperatures, potential evapotranspiration rates, vegetation cover).

There is an implicit criticism throughout Heimsath and Whipple's review that I am using proxies for rock damage and microclimate rather than more fundamental variables such as fracture density, soil temperatures during wildfires, etc. Clearly it is impossible to measure these variables over the spatial scale of an entire mountain range, hence the use of proxies is essential if progress is to be made at these spatial scales (and progress should be attempted at all spatial scales). The use of proxies to constrain properties that cannot readily be measured due to challenges associated with large spatial or temporal scales is as old as Geosciences itself.

Heimsath and Whipple state that there is no reason why the power-law relationship between bedrock fracture density and the distance from faults established by Chester et al. (2005) and Savage and Brodsky (2011) for portions of the SGM should apply at the landscape scale (point 1). Heimsath and Whipple are correct that most of the data establishing the scaling relationship between fracture density and the distance from faults that I referenced extend only to distances of ~100 m from faults. To address this point, in my revision I will expand the discussion to include studies from other regions that demonstrate power-law scaling between fracture density and the distance from faults out to larger distances (e.g., Sturzenegger et al., 2007). In addition, I will include a discussion of the geometric properties of fault networks (e.g., length, spacing, slip) that demonstrate robust power-law scaling up to spatial scales of ~10 km in southern California (e.g., Bonnet et al., 2001). Despite Heimsath and Whipple's dismissal of my approach, the use of a scaling relationship between fracture density and distance from faults, together with the best-available geologic maps to identify the faults, is both reasonable and perhaps the only possible approach to quantifying range-scale variations in rock damage at this time. Heimsath and Whipple further state that "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." How would these reviewers suggest that rock damage be more precisely quantified at these large spatial scales? I am not sure why "existing" geologic maps are insufficient, as they reflect careful mapping by many scientists over many decades.

Heimsath and Whipple criticize me for not including precipitation and temperature in my analysis (point 1). My analysis includes vegetation height, which is an effective integrator of precipitation and temperature that has the advantage of being mappable at high resolution over the entire mountain range, unlike precipitation and temperature, which must be interpolated from a few climate stations. The influence of range-scale climate variations was explicitly addressed in the last paragraph of page 8: "The largest P_0 values increase and then decrease with elevation between 1.5 and 2.5 km elevation, as indicated by the dashed curve that defines the envelope of the data in Figure 4A…. Mean canopy height, constrained from the Existing Vegetation Height layer of the U.S.G.S. LANDFIRE database (U.S. Geological Survey, 2016), follows a similar pattern to that of P_0 (Fig. 4B), correlating positively with elevation below 1.8 km a.s.l. and negatively with elevation above 1.8 km due to limited energy availability, especially in the cold-season months when most precipitation falls in the SGM. Figures 4A&4B suggest that P_0 may have some dependence on range-scale climate or vegetation." Moreover, I explicitly addressed why relationships between erosion rates and precipitation are difficult to interpret uniquely: "… the strong correlation that Spotila et al. (2002) documented between exhumation rates, elevation, and MAP. Spotila et al. (2002) cautioned, however, that this correlation could be coincidental as 'prevailing winds happen to deliver the most precipitation along the southern range front where the most active structures are.'"

I agree with Heimsath and Whipple that range-scale climate variations likely control P_0 values (point 4), and that spatial variations in uplift play an important role in controlling range-scale climate variations. I proposed that range-scale climate variations may control P_0 values (p. 9, lines 3-4) and I also emphasized the importance of tectonic uplift rates in my conceptual model (p. 9, lines 18-20), but I did not explicitly connect the two points as perhaps I should have. I did not include range-scale variations in climate and explicitly in my conceptual model because I could not document a statistically significant relationship between range-scale climate and P_0 values (in part because the relationship between P_0 values and range-scale climate is not simple or monotonic) and because I could not clearly separate range-scale variations in climate from rock damage, as discussed in the last paragraph of p. 8. I did explicitly note that higher potential soil production rates tend to increase erosion rates because erosion rates are limited by P_0 values and because both have similar bioclimatic controls. In the revision I will further emphasize uplift as an important mechanism for generating range-scale variations in climate and vegetation that likely control P_0 values and, as a result, erosion rates.

Heimsath and Whipple state that I provide no new data. To their dataset I added proxies for rock strength and microclimate, predicted the interrelationships among P_0 values, erosion rates, soil thicknesses, and topographic slopes throughout SGM, and compared those predictions to data. As noted in the manuscript, I also worked hard to find relationships with lithology (using quantitative indices related to mineralogy developed by Spotila et al., 2002 together with the best-available geologic maps) and vegetation cover.

Heimsath and Whipple further accuse me to throwing out data (point 3). All of the statistical analyses I present, save one, include all of the data. As a thought experiment (clearly presented as such), I observed that the correlation between P_0 and D values would improve (to 99.9%) *if* five data points from an area with an unusually high density of mapped landslides were removed. It is well established in the literature that landsliding, as a more episodic transport process than creep or bioturbation, can adversely influence soil production rates determined from CRN analyses. This point may be a distraction, so I will remove it from the revision.

Heimsath and Whipple declare that the correlations I obtained are extremely weak and not sound. I agree with Heimsath and Whipple that the correlation coefficients are low, a point that I discussed at length on p. 10, lines 10-16. Low correlation coefficients do not change the fact that the null hypotheses that P_0 values are unrelated to D and A can be rejected with 98.6% and 99.9% confidence, respectively.

Heimsath et al. (2012) worked hard to create a remarkable dataset and, of course, deserve wide latitude in deciding how the data are interpreted. However, the importance of soil production as a process controlling the evolution of mountain ranges, together with my natural desire to defend my work, stimulated me to take the unusual step of attempting to reinterpret their data. In Heimsath et al. (2012), the authors attacked a paper that I published in 2009 with Craig Rasmussen, referring to it as part of an "entrenched paradigm inconsistent with data" that "greatly exaggerates changes in critical-zone processes to tectonic uplift." In this paper I have shown that the model I developed with Craig is consistent with the data of Heimsath et al. (2012) (even though it was not developed specifically for the SGM) if the effects of rock damage and microclimate are taken into account using the best-available proxies.

I wish to thank Heimsath and Whipple for an engaging set of comments.

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