**Reviewer 3 (Simon Mudd):**

Comment: In this paper Jon Pelletier has used the dataset from Heimsath et al 2012 to explore controls on soil production. In the original paper, Heimsath and colleagues argued that rapid erosion rates could affect the P0 term in the soil production function. The obvious follow on question is: by what mechanisms does erosion rate modulate P0? As stated by Heimsath and Whipple's comment (doi:10.5194/esurf-2016-37), the original 2012 paper did not mechanistically explain observed trends. So, does Pelletier's paper give insight into the mechanisms? Firstly we can look at the damage indicator. I found this interesting since many authors have speculated on the role of fracturing in controlling weathering rates, and the implementation of equation (3) is a novel attempt to translate mapped faults into a metric for fracture density using results from detailed field studies. To compare this metric with soil production, Pelletier calculates P0 from every data point by regressing the soil production function, using a slope of h0 previously regressed in the Heimsath et al paper, to its h = 0 intercept. To do this, one must assume that the individual P0 results are meaningful and not simply the results of scatter in the data due to local heterogeneities in shielding and erosion history; Heimsath and Whipple feel this unwise, a point I will revisit later in this comment. However once Pelletier follows this thread he finds a weak correlation between the D metric and P0;regressed data (I'm not sure if I'd be so bold as to call it measured). One can explain

Response: Simon's point seems to end abruptly. However, I gather from his comments that he is somewhat convinced that a statistically significant relationship exists between P_0 values and D values. In the proposed revision, this point the model has been modified to be based on the topographically induced stress hypothesis.

Comment: What about aspect? There are a few rather high P0;regressed values for south facing slopes. Of the 11 points with P0;regressed values greater than 300 m/Myr, 8 of them are on south facing slopes. But there are also a large number of points on south facing slopes that don't have P0 values that are higher than the mean P0 value. The model combining topographic gradient and aspect again shows a correlation between it and the P0 values, this time explaining

Response: The revised analysis has demonstrated that aspect is not statistically significant. In the *ESurfD* paper I extracted slope aspect using the location data provided by Heimsath et al. (2012). Ten of the sample locations were ridgetops where the local slope is zero and slope aspect is undefined. When the data were reanalyzed to include only areas that were not ridgetops, P_0 values are slightly higher, on average, on south-facing slopes, but the null hypothesis that P_0 values are independent of slope aspect cannot be ruled out. The revised analysis is focused on average slope and climatic controls on P_0 values. I apologize for making more work for the reviewers with this major change to the manuscript, but I believe that the revised paper makes a convincing case.

Comment: I am somewhat confused by section 2.2. It seems strange to generate a map of steepness and from that calculate the spatial distribution of h and E. Global topographic maps are readily available so why calculate S from equation (8), which contains many assumptions, rather than just use topographic data? It also seems quite odd to use equation (7) since theory suggests that for a given erosion rate and P0, hillslope-scale gradient will vary as a function of hillslope length. More explanation of these choices is warranted.

Response: The purpose of section 2.2 is to model demonstrate that a model based on a combination of soil production functions and nonlinear slope-dependent sediment flux can reproduce the observed spatial variations and interrelationships among geomorphic and pedogenic variables in the SGM. I think the full power of the model is not clear until it can be shown to reproduce the full suite of variables across the range. This requires that slope be modeled first, then compared to an independent dataset. Equations (6)-(8) includes all the variables mentioned, (hillslope length, hillslope-scale gradient, erosion rate, and P_0), so I think the model is consistent with the theory Simon is referring to. In the proposed revision section 2.2 is motivated using the following text: "In this section I invoke a balance between soil production and transport at the hillslope scale in order to illustrate the interrelationships among potential soil production rates, erosion rates, soil thicknesses, and average slopes across the SGM. The conceptual model explored in this section is based on the hypothesis that the average slope depends on the difference between uplift and erosion rates. Uplift rates (assumed to be equal to exhumation rates) are lower in the western portion of the SGM and higher in the eastern portion (Spotila et al., 2002, Fig. 7b). As average slope increases in areas with higher uplift rates, erosion rates increase and soils become thinner. Both of these responses represent negative feedback mechanisms that tend to decrease the differences that would otherwise exist between uplift and erosion rates and between erosion rates and soil production rates. If the uplift rate exceeds the potential soil production rate, soil thickness becomes zero and soil production and erosion rates can no longer increase with increasing slope (in the absence of widespread landsliding in bedrock or intact regolith). In such cases, topography with cliffs or steps may form (Wahrhaftig, 1965; Pelletier and Rasmussen, 2009; Jessup et al., 2010). However, if the potential soil production rate increases with average slope via the topographically induced stress fracture opening process, the transition to bare landscapes can be delayed or prevented, thus representing an additional negative feedback or adjustment mechanism (Heimsath et al., 2012). At the highest elevations of the range, however, soil production is slower, most likely due to temperature limitations on vegetation growth since the slopes there are among the steepest in the range. The interrelationship between these variables can be quantified without explicit knowledge of the uplift rate, since the relationship between soil thickness and average slope implicitly accounts for uplift rate (i.e., a smaller difference between uplift and erosion rates is characterized by a thinner soil). This conceptual model predicts positive correlations among potential soil production rates, erosion rates, and topographic steepness, and negative correlations of all of these variables with soil thickness."

Comment: It is worth commenting on the use of scatter in soil production data to regress P0 values for individual samples. Because these numbers were collected at specific points in the landscape (i.e., they are not basin-averaged data), one must consider if the local sources of scatter. Suppose one measured 10 P values in close proximity (e.g., in a 15 m radius): how variable would those P values be? We don't actually know how representative the P values are on a local scale, but we know soil thickness can have quite a bit of local variability, chemical weathering can have substantial local variability, and you can have substantial local variability in the production of 10Be (from where snow falls, any transience in erosion history, etc.). So I do not think Heimsath and Whipple's concern about interpreting the P0 values is unwarranted: I share this concern. So, in summary, I am worried that the potential uncertainties in P values makes it difficult to come to strong conclusions about influences of other factors on P0, that even if you believe the P0 values are representative the correlation with D is rather weak, and that I do not feel the effects of aspect have been sufficiently separated from gradient effects.

Response: I agree with Simon that some variability in P or P_0 values is due to methodological uncertainty such as snow shielding, etc. I also agree that, if that variability were dominant it would be dangerous to attempt to interpret P_0 values (because, for example, snow shielding varies with aspect and hence a methodological bias could be misinterpreted as an aspect control on soil production processes). However, I don't think that errors associated with the methodology are anywhere close to the order-of-magnitude variations in P_0 values observed in the data.

Please see my responses to reviewers 1 and 2 for additional information relevant to the proposed revision.