

Interactive comment on “Effect of changing vegetation on denudation (part 2): Landscape response to transient climate and vegetation cover” by Manuel Schmid et al.

Anonymous Referee #1

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Review of Schmid et al.

Summary: The authors attempt to link changes in landscape form to changes in climate and vegetation. Although this is a worthy topic to explore, it would be a challenge for even a more complicated model because we simply don't know enough about the processes and feedbacks involved. Considering that this model greatly oversimplifies what we actually do know, its contribution to our understanding is not obvious. Those familiar with these processes will view the results with skepticism, and those who aren't may believe the results without fully appreciating all of the short-cuts and assumptions baked into the governing equations. I know how much work goes into

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modeling exercises like this so I always try to be open-minded when reviewing these types of manuscripts but, in this case, I cannot recommend publication.

Main comments

My comments are mainly focused on the governing equations. Because I believe them to be either unsupported or flawed, I don't address the results in detail. If the governing equations of a model are not honoring reality in some fundamental way, then its output will be unreliable.

It didn't seem like the authors tried to determine whether the model was working correctly. Thanks to 10Be, we have erosion rates for many watersheds around the planet, including catchments that are similar to the ones modeled here. Before we can believe that the model works, the authors ought to run it on one with published erosion rates. This would have been an important first step before embarking on the rest of the project.

This model is driven by, essentially, two governing equations. The first describes soil creep via linear diffusion. The authors use a formulation, proposed in a paper from 2005, that links the diffusivity to vegetation density that was based on little data and only accounts for physical processes (eg, rainsplash) and ignores bioturbation. Are there field observations to support that physical processes dominate soil creep at their field site? Importantly, there is no support for the nonlinear equation that relates diffusivity to vegetation density. Over a narrow range of precip, Ben-Asher et al (2017) found a linear inverse relationship between diffusivity and precipitation which, when combined with the present study's relationship between veg and precip might yield something like the negative exponential equation adopted here but the authors have not demonstrated that. Moreover, a paper that examined diffusivity across a wider range of precip (Hurst et al, 2013) found the relationship to be weakly positive – which runs counter to what the authors have assumed. There is little support, then, for the way the soil creep equation has been parameterised.

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The second governing equation describes erosion by flowing water. Before describing my main concerns, I should point out that this section (lines 193-215) was difficult to follow and was missing some critical details. For example, variables appear without explanation or description and the relationship between eqn 5 and the others that follow was unclear. Also, there was no explanation of how rainfall is applied (eg, storm frequency and magnitude), how runoff is generated, or how runoff generation is affected by vegetation density (eg, via interception). The point about storm frequency and magnitude is especially important because changes in climate will affect the distribution of both of these but not necessarily in a uniform way. Given that, here are my main comments regarding the way that erosion by flowing water is treated in the model.

1) Linking the roughness coefficient to the vegetation in the way that was done here ignores the fact that the effect of vegetation goes beyond a simple measure of 'vegetation density.' For example, imagine two landscapes with 70% vegetation density: one is covered by shrubs such that the ground surface between each plant is essentially bare while the other is covered by grasslands. These two landscapes, despite having the same vegetation density, will have different manning's n values on the hillslopes. Since we know that vegetation community changes with climate, the model's attempt to scale manning's n on the basis of vegetation density is not realistic. Indeed, I looked at the field sites via Google Earth and it was clear that vegetation community does change as a function of precip in those regions. I can easily imagine situations where manning's n actually increases with a decrease in vegetation density, the opposite of what is assumed here.

2) Given the comments above, both landscapes will also have different critical shear stresses. For example, soil with shallow grass roots will be more difficult to erode than the bare soil between shrubs. It doesn't appear that the model takes this into account.

3) It appears that the model doesn't distinguish between overland flow on hillslopes and river flow. If so, the authors are assuming that a source of roughness on the hillslopes – the vegetation – is also contributing to roughness in the rivers. For example, if the

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authors envision shrubs growing on their hillslopes then they must also be growing in the rivers. Again, I don't see how this is realistic. Moreover, the type of vegetation really matters here with respect to flow depth. Short grasses would have a greater manning's n with low flow depths (ie, overland flow) than with deeper flows (ie, rivers). Conversely, shrubs would have a lower manning's n with overland flow than with river flow.

4) There was no explanation of how the critical shear stress was calculated. Presumably some assumptions were made regarding bed and hillslope material but these were not described. Does the model keep track of the evolving particle sizes as climate changes? More vigorous runoff will coarsen the river beds and hillslope surfaces but I didn't get the sense that this was incorporated into the model. Also, a lower vegetation density will expose the ground surface to raindrop impacts that will mobilize finer material more readily. Was this accounted for? Again, it didn't seem like it.

One of the model's limitations are well-illustrated by Figure 9. It predicts long-term erosion rates on the order of about 0.2 mm/y. This is on the high end of known soil production rates; I would venture to guess that soil production rates at the sites in Chile are quite a bit lower given the dry conditions (0.2 mm/y is what you get in weak-ish bedrock in the Oregon Coast Range where its wet and has lots of trees doing physical weathering). This means that, at these high erosion rates, the landscape would run out of soil yet the model seems to assume an inexhaustible supply of erodible material. In the real world, the loss of soil would have important consequences for runoff processes and the ability of plants to grow but the model seems blind to these.

Finally, there was no attempt to provide any error estimates in the predictions. I understand that this is not common practice with landscape evolution models but it should be and can be done (see papers by Tom Dunne on stochastic modeling). For example, the model makes certain predictions about how erosion rates may vary over time after changes in vegetation (Figure 9). Given all the potential uncertainties embedded in the governing equations and how they were parameterized, how confident are the authors that a predicted erosion rate of 0.2 mm/y is statistically different from a predicted ero-

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sion rate of 0.4 mm/y? I'm skeptical that this model can predict annual erosion rates accurately to one tenth of a millimeter.

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