Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-13-SC2, 2018


Earth Surface Dynamics Discussions

ESurfD

Interactive comment

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

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Received and published: 20 April 2018

This manuscript has a lot of value in terms of exploring the implications of some of our current best guesses with respect to how vegetation cover and precipitation modulate erosion rates at the Earth surface and influence topographic evolution over long timescales. While the investigation of the individual influences of precipitation and vegetation cover provide insights into the role of each, the dynamic behavior that arises through the combined modeling of both comprises a testable set of results, which could lead the way to improved equations in the case of clear mismatches. People in our community with a focus on details of individual processes and individual types of vegetation cover may find the generalization necessary for such models running over such long timespans to be an oversimplification, but I agree with the approach of first testing whether or not this simple approach can explain first-order observations with respect to erosion rates and landscape morphology.

Along these lines, I think the authors could expand their discussion somewhat to better emphasize how those of us from the field-data side might help to test these results. Testing the results of the model with respect to the morphology of the Andes is useful, but also has some broad limitations. For example, it's difficult to know if the topography that we see today is fully adjusted to forcing conditions such that it reflects the predicted influence of precipitation and vegetation cover, or if there could be a persisting transient response to tectonic activity. Calibrating the model to the conditions in the Andes could be problematic if the landscape is currently in a transient adjustment state. I wouldn't suggest that this issue rules out the possibility of comparing model predictions with topography, but it does point to a reason why there might be mismatches.

Another possibility for comparison would be to consider datasets that have recorded temporal variations in erosion rates in response to changes in precipitation and vegetation cover, particularly over the precipitation and vegetation cover ranges that the authors suggest drive the biggest changes. Field data that support the model results (particularly Figure 17) would be a strong argument in favor of the equations used. Unfortunately, those datasets are somewhat rare; the only ones I am aware of that report both vegetation cover and precipitation changes include Marshall et al. (2015, Science Advances) and Garcin et al., 2017 (EPSL), although Marshall et al. was mostly focused on frost-cracking, which would be less relevant for the model presented here. While I'm a co-author of the second and don't want to insist that the authors consider that study, I think it could be valuable, as we found quite strong variations in 10Be derived erosion rates reflecting the onset and persisting influence of the African Humid Period. An in-depth comparison of your model results to those data would clearly be out of the scope of the manuscript, but a qualitative comparison could be useful.

One aspect of the modeling results that surprised me is that for the 10% vegetation cover case, increasing precipitation leads to a decrease in erosion rates in the model. I find this somewhat counter-intuitive, and contrasts in some ways with what we see in East Africa, where the onset of wetter conditions leads to a brief spike in erosion rates, however the rates rapidly decreased once denser vegetation cover (trees, compared to a mix of grasses and trees earlier) established itself. So why the difference with your results? I'm guessing there's not much of a time lag in the model between increased precipitation and increased vegetation cover... if there were, perhaps the model would show a response more similar to what we see in our data.

I'm unsure whether short short-term responses are important for landscape evolution models running on million-year timespans. But even if it's brief, a spike in erosion rates could have a reasonably good change of being preserved in stratigraphy, so there may be a good chance of recovering such responses with field data. Along the same lines, I wonder whether or not it would be possible to record (with field data) the high-frequency variations that the model shows during the declining-precipitation phase (again for the 10% veg cover case). Do you think we could we resolve a sudden, brief decrease in erosion rates with 10Be data? Two reasons why it could be tricky is that the thickness of any stratigraphic layer would be limited, and also at lower erosion rates, the erosion rates are integrated over longer timescales. This point may be worth discussing.

Some additional minor points:

Section 4.1, lines 248, 255, 266: It would be helpful to use the term "spatially variable" in this section rather than "variable" alone, to make it clear that you are not focusing on temporal transients.

Why isn't section 5.4 in the results section (section 4)?

Figure 16 is hard to interpret without Figure 17; it would be easier if the forcing (change in precip) were shown in Figure 16 or at least explained in the caption. Are changes in vegetation cover modeled or prescribed? I'm embarrassed that I was reading too quickly to discern that detail, but in any case, it would be helpful to have that information more prominent.

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