Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2017-47-RC1, 2017

Earth Surface Dynamics Discussions

Interactive comment on "Where is Late Cenozoic climate change most likely to impact denudation?" *by* Sebastian G. Mutz et al.

A. Wickert (Referee)

awickert@umn.edu

Received and published: 23 September 2017

Summary:

Mutz et al. look to use paleoclimate GCMs to identify drivers of past geomorphic change. This is a topic for which I hold great interest, and I feel that the authors have crafted a very useful set of model results that they leave underutilized. As such, I feel inclined to accept the paper on the basis of the useful results, but to request major revisions such that they do their own work justice.

My major concerns, which will become clear in the line-by-line comments (please feel free to respond to similar comments en masse) are as follows:

1. The paper is motivated by denudation rates and landscape evolution, but really

includes this as a speculative wrapper that is not substantiated. I suggest that instead you propose testable hypotheses surrounding your findings.

2. Related to #1, much of the text is a litany of "temperature was X here ...". I find such statements of results useful only insofar as they expand upon a figure (associated with a supplementary data set) that presents the results. While these sections are written clearly, I would suggest that the authors focus on a set of geomorphic questions (if this be their motivation) and how the model-data set informs those questions.

3. Many of the discussions of model results are of ice-covered regions, yet no consideration of direct glacial erosion is given. Furthermore, no reference to the changes in the statistics of discharge or catchment area in ice-covered regions is given. This seems a disservice to this relatively high-resolution paleoclimate AGCM: the geologic setting *must* be considered, otherwise it seems that the authors' pushing on the modeling end has not been matched by a simple geological history sanity check. I would suggest that either significantly glaciated regions and the catchments that they feed be masked out, or that glacial erosion and its associated processes be included in the discussion.

4. (Discussed only here): You have not compared your models against any data. I understand that this may be simply a modeling exercise that you do compare to other models. However, I think that such a comparison could assuage skepticism about your results and lend support to your case, especially if you include it as part of a local case study (see the third point below).

The core of these three points is that, with a bit more care, I think your results could say something really useful to the geomorphic community. Currently, the paper seems to be more a statement of, "this is important to geomorphology", followed by a long list of the model results. I challenge the authors to demonstrate (rather than simply stating) the importance of their work to geomorphology in a way that includes how it may impact the way scientists view Quaternary landscape evolution. Ideas include:

- Changes in means (done)

- Changes in statistical distributions of temperature and precipitation – think extreme events, frost-cracking window, etc.
- A focus on a few iconic regions while *explicitly* ignoring significantly ice-covered domains (I think this would be easiest, though obviously would be thrilled if you decided to tackle glacial processes)
- Using this focus to build a template for how to use paleoclimate GCM outputs to advance the field of geomorphology.

Currently, I think that the work is acceptable following changes for internal consistency and geological accuracy (see #1 and #3), but I think that you could be selling yourselves short if you don't dig just a tiny bit deeper to investigate your forcings and their impact on geomorphology.

I hope that you find these comments helpful in continuing to craft an insightful piece of work out of what seems to be a strong modeling approach.

Line-by-line:

23. US Pacific Northwest Pacific → drop second "Pacific"

29. future observational studies interested in quantifying → future observational studies **that quantify** (studies can't be interested in things, strictly speaking)

53. orogen scale → orogen-scale

∼57. A couple of recent studies from the climate science community shed light on the impacts of the Andes (first ref below) and continents in general (second ref below). In case these are interesting to you, I'm pasting the bibliographic information here:

Maroon, E. A., D. M. W. Frierson, and D. S. Battisti (2015), The tropical precipitation response to Andes topography and ocean heat fluxes in an aquaplanet model, J. Clim., 28(1), 381–398, doi:10.1175/JCLI-D-14-00188.1.

Maroon, E. A., D. M. W. Frierson, S. M. Kang, and J. Scheff (2016), The precipitation response to an idealized subtropical continent, J. Clim., 29(12), 4543–4564, doi:10.1175/JCLI-D-15-0616.1.

73-75. "Furthermore, recent controversy exists concerning the spatial and temporal scales over which geologic and geochemical observations can record climate-driven changes in weathering and erosion [e.g. Whipple, 2009; von Blanckenburg et al., 2015; Braun, 2016].": I see that you do not return to this point later, so could you describe the controversy for those who are not familiar with it?

81. I see that later you discuss a little about what an AOGCM may do, but I will be looking for justification about how an AGCM may suffice. Is this in part because you prescribe the b.c.'s and you are running it for 17 years only? If so, could you discuss potential systematic variations between this and an AOGCM?

89. "PLIO to the Last Glacial Maximum": as you include no time-slices between these, I suggest making these part of the list and dropping the "to the".

147. "This section describes the clustering method used in this study." You could drop this sentence – the section title should be enough for even an inattentive reader!

176-178. I was wondeirng how you picked the number of clusters: I am glad to see that you performed a thorough search.

Section 3: Much of this is information that I find better communicated through figures than with text. It is clearly written, however, and I am reluctant to suggest a rewrite for brevity in a length-unconstrained journal so long as the text can be co-located with the figure.

190-192. I see you have another "This section describes..." sentence. If this is your preferred way to write, you may keep it; here, the second sentence is not such a good topic-sentence replacement.

197-198. i.e. over the ice sheets. (This applies to other regions as well, and should be

important to point out if you are going to then discuss fluvial processes in orogens)

203-paragraph: Also because of local ice loss, presumably. So I think that the two prior paragraphs could have a new summary that "The greatest changes in temperature is observed where the greatest change in local ice extent occurs."

214-215. Have you considered discussions of the African Humid Period?

373-374. If you are looking at the influence of temperature and precipitation on erosion, and you are not including subglacial erosion, then your preceding text must indicate where your changes really are indicative of ice extent – both as a separate process domain and as a driver of fluvial processes and potential changes in the statistics of river discharge.

Section 4.1. Your first paragraph (weathering) differs from the content (comparing your model results with those published). These should be in different subsections, and the weathering paragraph may need to be expanded. Your "weathering and erosion" paragraph also neglects direct effects of glaciers, ice caps, and ice sheets, which were globallly significant.

Section 4.2. Once again, your discussion is often of formerly (or currently) ice-covered regions without explicitly acknowledging that this is a different process domain. In addition, as with the previous section, the body paragraphs are mostly about model comparison and regional changes with sparse link to the landscape-evolution factors indicated in the topic paragraph.

416-423. Please discuss the direct influence of glaciers on the erosion orogens in the context of changing precipitation (and therefore mass balance). Is it significant or not?

433-434. "Coastal North America"? Doesn't look like it: seems to be most of NA south of the ice margin.

Section 4.3. The authors describe the results here, but I find the connection to erosion rates to be insufficiently described compared to how they are highlighted in the topic

sentence, as well as in the abstract. I would like you to go one step beyond "ought to be considered" and actually posit how you expect the erosion rates – and therefore, the balance between erosion and exhumation and perhaps the equilibrium shapes of the mountains and their rivers – to vary. Otherwise, you are suggesting future work rather than actually describing the possible geomorphic significance – and I think underutilizing your results in a paper that is clearly targeted towards geomorphologists.

498. "which may favour frost driven weathering during glacial climate states" – the St. Elias range was covered by glaciers! Yes, there can be some frost-cracking around the ice, but don't you think this is important too? http://instaar.colorado.edu/groups/QGISL/ak_paleoglacier_atlas/gallery/index.html

508. "enhanced sediment production driven by frost processes" – same as above. Glaciers were there. Consider them.

Conclusions: Comparison to other models: is this match surprising or no? Did you (mostly) use the same inputs and simply increase the grid resolution? If so, could you comment on how the improved grid and possible variations in inputs and use of the ocean as a boundary condition may have affected (or not) your results as compared to those of earlier studies? This would be more useful to include in the discussion than a simple list of "Our temperature in place Y was T_0, and X et al. wrote that they found it was T_1, which is close to T_0. Think big-picture, in both process and numerics!

533. Did your 8-10 degC changes occur significantly over areas that would be affected by hillslope or fluvial processes? (i.e. unglaciated areas?)

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2017-47, 2017.