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

Anonymous Referee #2

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The authors simulate the pre-industrial, mid-Holocene, LGM, and Pliocene climates using the ECHAM5 general circulation model. The motivation is to understand how past climatic states may change regional climatologies, particularly over mountainous areas that have been the focus of much erosion and geomorphic work. The authors find that past climatic states (particularly the LGM and Pliocene) do produce changes in absolute temperature and precipitation and in the annual ranges of these two climatic variables.

Overall, I did not find the manuscript to be a particularly useful addition to the literature. Though the motivation is potentially novel, the analysis is not complete and needs substantially more work. Most of the manuscript focuses on simply describing climatic changes, while neglecting novel analyses. Consequently, at this point, I recommend

reject, though perhaps with substantial work (including more modeling, comparison with existing models, and/or comparison with data), it may become publishable in the future in ESurf.

First, the authors need to decide what the point of the paper is. Most of the paper reads as a description of climatological changes for 3 periods in the geologic past (MH, LGM, and Plio) (indeed, most of the text is written this way). However, this work has already been done, most prominently by the PMIP and PlioMIP set of model intercomparisons. What does this manuscript offer that these model intercomparisons have not already analyzed? A case could be made that these model intercomparisons are typically of a global nature (though there has been some work on changes in the Asian Monsoon systems using both PlioMIP and PMIP results (Jiang et al., 2013; Jiang and Lang, 2010; Zhang et al., 2013)), so that the analysis of the orogens in this study is useful. However, given that GCMs have difficulty simulating precipitation and in particular simulating precipitation over complex topography, the usefulness of simply describing changes over the Himalaya, Andes, Cascades, and St. Elias ranges is somewhat muted. For example, why should readers believe that ECHAM5 produces reasonable results over the St. Elias range? Why not use (or at least compare with) the existing model intercomparisons to look at changes in these locations? Most of the PlioMIP simulations are of a lower resolution than the model simulations presented here, though not all (Haywood et al., 2013). Many of the newer PlioMIP2 simulations are being run at a higher resolution and permit at least some comparison with the data here (Chandan and Peltier, 2017). Having myself tried to access PlioMIP data, I understand that it can be difficult to get access to the PlioMIP output, but if the point of the paper is to quantify actual climatological change, then comparison with other models is a must (or at least a thorough treatment of possible boundary condition uncertainties and additional ECHAM5 model runs to establish the sensitivity of precipitation/temperature in these areas); otherwise, we have no reason to believe that ECHAM5 presents anything resembling a proper picture of climatic change in the past.

Some parts of the paper address actual causes for climatic change (for example, discussion of the Pacific North American Teleconnection (no citation given). Is this related to the PDO? Lines 493-495). Again, if this is the primary point of the paper, then substantially more work needs to be done to address why precipitation, for example, increases in the Himalaya in the Pliocene. If this was the point of the paper, it would obviate the need to compare with other model simulations (see paragraph above), but would then require substantially more work to identify how various atmospheric phenomenon change through time. A generalized description of changes in the past is not particularly useful (most of this information can be communicated fairly effectively with figures), so addressing the causes of these changes (or comparison with proxiesâĂŤsee below) is worthwhile.

Parts of the discussion showcase comparisons with terrestrial proxy data, though this is limited to citing previous work and stating that there is general agreement with previous, proxy-model work. If a proxy-model comparison is the point of the paper, then more work needs to be done actually compiling the proxies and doing a proper statistical test to see if there is agreement between modelled precip/temperature changes in each of these orogens and existing data. This would be a useful contribution to the literature, but, again, would require substantially more work.

Much of the motivation for the manuscript appears to be to understand how climatic changes may change denudation/geomorphic analyses, but this is done in only a superficial way. If this is the point of the paper, then, again, much more work needs to be done, rather than simply stating that erosion depends on climate (lines 416-423 are a good example of statements that serve to motivate a paper, but don't provide any actual analysis). For example, can the authors take some of the climate model output and, given a potential 2000 mm/yr change in precipitation in the Himalaya since the Pliocene, actually re-interpret some of the existing exhumation/denudation data? If not, why not? What additional data is necessary? And if such a reanalysis isn't possible, then how does knowledge of such a change in precipitation facilitate future work?

I agree with the generalized statements made throughout the paper (i.e., that denudation and landscape evolution depend on climate), but these are somewhat self-evident and, as currently written, the manuscript does not make a fundamental contribution to improving our knowledge on this subject except to state that climate changed in the past. Assumptions of stationarity are indeed a problem in interpreting modern datasets that have a component of geologic history, but a really nice contribution of this paper would be to show how these assumptions can be mitigated when one knows the history of precipitation or temperature.

Because ESurf is not a climate modeling journal, more discussion needs to be given as to the limitations of ECHAM5 in a way that Earth surface process folks can understand. For example, what are the uncertainties associated with simulating orographic precipitation? Though T159 is high-resolution, it still requires substantially smoothing topography, which presumably introduces some uncertainty in to the results. What uncertainties are associated with the PRISM reconstruction? (on a side note, which PRISM reconstruction is used? PRISM3D? PRISM2? I mention this because the topographies between different PRISM reconstructions are substantially different.)

I found the use of the cluster analysis to be not intuitively helpful. If the authors want to keep using it, then the authors need to at least walk the reader through an example of how to understand Figures 6, 9, 12, and 15. Is C1 always the same climate zone in each figure and in each time-slice? If so, why are different colors used? Why don't the authors use something more intuitive, like Köppen's climate classification scheme (Peel et al., 2007) to classify climates? As best I understand it, the clustering analysis is used to show the spatial extent of a given climate in a given time-slice and in a given location, but it's not clear how one should interpret these results.

Minor Comments: Are the topographies for any of these ranges modified at all (it's unclear from the PRISM reference (Line 131), whether this has been done).

Lines 57-59: "Cold-temperature island" is not a climatic term in widespread use. What precisely do you mean? Also, Boos and Kuang, 2010 specifically refute the idea that Plateau surface elevation matters for the South Asian Monsoon and rather focus on the Himalaya instead.

Line 61: Zhisheng et al. (2001) don't actually present any new geological data. Instead, it is all from cited literature. The focus of their study was GCM results. Dettman et al. (2003) is not the only study to look at this. Please see the following studies (which are just a sampling): (Caves et al., 2017; Kent-Corson et al., 2006; Lechler et al., 2013; Lechler and Niemi, 2011; Licht et al., 2016; Methner et al., 2016; Mulch et al., 2015, 2008; Pingel et al., 2016).

Lines 77 and 78: "documenting the magnitude" appears twice.

Lines 96-97: Though, importantly, several recent studies have run ECHAM5 at a higher resolution ((Feng et al., 2016; Feng and Poulsen, 2016).

Lines 102-103: This statement is somewhat odd, since the authors are specifically investigating climatological changes over mountain ranges, where resolution typically tends to matter.

Lines 114-133: For all simulations, stating the pCO2 used in the experiment would be most helpful, particularly since it won't take up much room. Also, how is the land-surface treated? For example, the authors state that they are using vegetation reconstructions, but it's unclear if this is then being fed into a "built-in" land-surface model or if they are explicitly using JSBACH.

Lines 126: "for the" used twice.

Lines 203-204: Changes in Greenland and Antarctica are almost certainly unreliable. Because PRISM uses a reconstructed ice-sheet extent, changes in temperature in Greenland and Antarctica are almost certainly reflecting the imposed boundary condition, which itself has quite a bit of uncertainty. It's hard to get around this, except to note that the change in temperature is entirely dependent upon the ice-sheet boundary

condition (see discussion in and of de Boer et al. (2015)).

Lines 416-423: Are runoff changes in these models coupled to precipitation changes? In all cases, does P–E (precipitation minus evaporation) scale with changes in precipitation. I'm not particularly familiar with JSBACH (presuming this is the land-surface model used), but if it has a CO2 fertilization parameterization, then runoff may be decoupled from precip. Some of these erosion processes may depend more on runoff than precip.

Figure 1: Would be nice to also plot the topography of the St. Elias range and the Cascades.

Figure 7b-Precip-PLIO: Why does precipitation appear to follow a wave-like pattern over tropical South America? Is this due to the spectral nature of ECHAM5?

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