Earth Surf. Dynam. Discuss., 2, C180–C183, 2014
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Earth Surface Dynamics Discussions Open Access



# *Interactive comment on* "Ice flow models and glacial erosion over multiple glacial–interglacial cycles" *by* R. M. Headley and T. A. Ehlers

**Anonymous Referee #1**

Received and published: 6 July 2014

Review of: Ice flow models and glacial erosion over multiple glacial–interglacial cycles, by R. M. Headley and T. A. Ehlers

I regret to say that I was disappointed in this manuscript. It begins with great promise: an interesting problem – glacier-landscape evolution over multiple glacial-interglacial cycles – using a sophisticated set of models and some simple, clearly-defined model experiments. But after a few times through the manuscript, I have trouble to pull out anything novel or informative.

The experiments are interesting and there are differences between them, i.e. the authors confirm that erosion rates depend on climate and the extent of glacier sliding, with evidence of positive feedbacks in erosion and landscape evolution over multiple glacial-interglacial cycles. What I don't see is what is new here, relative to the recent results of

Egholm in particular, but also the work of MacGregor and some of the more ‘classical’ works in glacier erosion (e.g. Hooke, Hallet), which lay out the framework for some of the positive feedbacks involved in glacier flow and bedrock erosion. What hypotheses are being tested here? What is the underlying theoretical question or observational feature that the authors seek to explore? The results and discussion are also lacking with respect to the processes and feedbacks that explain the model results. What is going on, and what can we learn from it?

Perhaps I am missing some key ideas and results here, but it is my sense that this is largely a model-based fishing expedition: run some simulations and plot the results, commenting on differences but without overarching questions, theoretical framing, or physical insights. The results as presented are mostly predictable, such as cumulative effects over time and larger differences between models (SIA vs. HO) when things are warm or wet, i.e. erosion is higher because sliding matters more. Where the authors have highlighted differences between the models, I am not sure why these occur, why there, or why they matter.

I apologize that this may be overly negative. I am partly disappointed because the set of models being deployed here is quite sophisticated, and there should be space for good contributions. The authors explore well the effect of SIA vs. full-Stokes physics on the 400-kyr evolution of glaciers and the underlying topography in a realistic orogen, and there may be some interesting results here that can be drawn out. I would like to see the authors go much further in this regard though: What is new? What are the geomorphological or glaciological implications? Is there something systematic with spatial patterns that can be understood in terms of ice physics or characteristics of the terrain? Why does the system work this way, i.e. what in the SIA vs. full-Stokes physics causes this divergence? Cold vs. warm, sliding vs. not sliding, have been well explored elsewhere, and these seem to be the first-order results here. Only qualitative insights concerning erosion rates and extent are possible due to various limitations that the authors nicely point out (sliding and erosion laws, effects of subglacial water, bedrock erodibility contrasts, the idealized climate forcing, etc.) – hence, the authors need some key phenomenological or physical/process insights to carry this work.

One example might be to focus a bit more on what is going on to drive the differences between SIA and HO models – just different sliding velocities? Systematically so due to…? Do the results change if the erosion rates were to scale with basal shear stress rather than velocity, or another form of erosion law that has been suggested in the literature? If the main point is to shed light on some of the differences between SIA and HO glacier physics, the authors could go deeper into these aspects of the system.

A few specific comments:

I cannot actually tell which may the model runs in time, forward or backward, i.e. the way that physicists or paleoclimatologists think? It does not matter much for the sinusoidal climate forcing, but test 3, with an ice-core looking climate history, is either a long, slow cooling ramp or a rapid cooling followed by long, slow warming. The former mimics natural glacial cycles, but it seems that the latter is used here?

Should 'interglacier' be 'interglacial period'? e.g., p.408, l.14

There are a few typos, e.g. p.409, l.27, should be 'mentioning', but overall this is very well written.

Figure 1 - an x-y plot of the ice thickness (and perhaps bed and ice topography) along transects A and B would be helpful (initial, final, perhaps at a key snapshot like this).

There are relatively few pulses of glaciation here compared with what mountain glaciers might experience over centuries or millennia in interglacial periods. I appreciate that the goal here may be to look more at larger-scale and longer-term landscape and glacier evolution. Some comments on what situations in nature are being explored might be helpful though: what scales, in space and time, and what kind of landscape or glacial features? Pleistocene valley development? I would welcome some more Earth surface dynamics context or motivation for the study.





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