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

Anonymous Referee #2

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General appreciation

This paper investigates the influence of model physics on determining long-term erosion and landscape development due to ice sheet and glacier motion over multiple glacial-interglacial cycles. While up to now, most ice sheet models tackling this problem used an approximation to the Stokes system that was rather simplified (but valid under certain circumstances), this paper uses for the first time a higher-order model (and therefore computationally costly) in its embedded form to investigate the impact of more complex model physics on erosion evolution. The novelty and merit of the paper lies in the fact that this is the first long-term and large-scale exercise on comparing the effect of approximated stress fields (shallow-ice approximation) on long-term erosion and landscape development.

However, there are a number of weak points in the analysis that need to be tackled before the paper is acceptable for publication. The first point is regarding the validity of the shallow-ice approximation (SIA). It should be made clear from the beginning what SIA stands for and why one can use SIA to simulate large-scale glaciations. In itself, there is nothing wrong with SIA, on the contrary. Its validity extends to shallow ice masses (therefore low surface slopes), which is the case for ice sheets. Furthermore, it is valid in areas where ice sheets are not in contact with the ocean (absence of grounding lines) and when sliding is relatively low, compared to the deformational velocity. Under these circumstances SIA is a valid alternative/approximation, and should yield results that are in line with higher-order models or full Stokes models. According to this study SIA becomes invalid in areas of high topographic relief, but it is not clear whether it is due to the surface slopes or due the effect of sliding (and the higher-order stresses in the basal sliding function). Furthermore, yet another factor of importance that has an impact on the results, is the spatial discretization of the model. What is the spatial discretization of the HO model and in the comparison, did the SIA model used the same mesh? Otherwise, comparison based on physics is not possible. This crucial information is lacking in the paper, which hampers my evaluation of it.

In summary, while the authors make a good job in comparing both models, they should clearly define what their premise is: SIA is a good approximation and valid under most conditions. A premise could be: When and where is SIA valid and under what circumstances is it not valid any more? However, the lack of a thermomechanical coupling in the model weakens the evaluation (see below). Furthermore, the conclusions of the paper are rather weak. It is obvious that if you calculate on longer time scales that the differences become larger, because the basal topography changes (and is irreversible) over time. This is different from the ice sheet itself that can recover over glacial cycles. Care should be taken in determining precisely what processes cause the difference in response between the HO and SIA models.

Detailed remarks

Page 395: Glacial models: please discuss the validity of SIA: under what circumstances SIA is valid and when would we expect a full Stokes model producing the same results? This should be done briefly here. It is repeated in more detail under the heading SIA, but from the beginning of the paper it needs to be clear what SIA stands for.

Page 398: Over long time periods and large areas, the temperature dependence of the value of A is important and cannot be taken as a constant. It should be carefully argued why it is possible to do so. Maybe the uncertainty in A (constant versus temperature dependent) is more important than the difference in physics, and this is not clear from the analysis. Anyway, testing higher-order physics by neglecting thermomechanical coupling on such time scales makes no sense. This exercise is about looking for the impact of HO model physics on erosion, so all parameters should be taken into account. The temperature field will also be influenced by HO physics!

Page 399: Fixing the value of N-P at 80% the overburden pressure is a way of minimizing the effect of higher-order physics. The latter become important when a full coupling between basal sliding and ice physics is consistent. Limiting the effective pressure is a way of reducing the higher-order model to a SIA model. Sliding is an integral part of the ice physics of an ice sheet model.

Page 401: What is the spatial resolution of both models? This is crucial information.

Page 401: What is meant by the fact that sliding velocity is 'later' calculated from the basal shear stress? Sliding is part of the model.

Page 403: 'A' is already used as flow parameter in Glen's flow law. Use other parameter here.

Page 404: interglaciers? Never heard of.

Page 407: 'If a simplified model can produce results similar to a more complex model, then the simpler model with fewer free parameters is preferred'. This is a wrong state-

ment. You can still have a similar result for the completely wrong reason. It should be stated differently: the complete model should give the same result as the approximation if conditions for the approximation are valid. Deviation from it may be for many reasons, i.e. numerical, implementation of boundary conditions, non-validity of approximation, etc. and the analysis should make clear what the origin of the difference is.

Conclusions: It is quite obvious to me that the difference between both model results gets bigger over time, because due to erosion, the bottom surface changes considerable; unless the ice sheet itself that can regenerate during the course of glacial cycles, the bedrock doesn't: what is eroded is gone. So over time, the discrepancy can only get bigger. The conclusions should be put in this context. Furthermore, there is no evaluation on when and where SIA models, which are still used in this model study for the majority of the domain, remain valid.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 389, 2014.