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Earth Surface Dynamics Discussions

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

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Referee 2 has provided helpful criticisms and questions regarding many specific points of the manuscript. Here, we address many of these points. Please see our general response and our response to referee 1 for more details regarding out revised manuscript.

Referee 2 makes a point that mention of the validity of the SIA model should be added and discussed earlier in the paper as opposed to just being discussed in the section specifically on the SIA. However, in the Introduction (section 1), the third paragraph is devoted to this exact topic and mentions many of the particulars that the referee has pointed out, such as the role of low surface slopes. This discussion includes citations of many relevant works, particularly those of Egholm, which provide detailed study of

comparisons of the SIA and higher-order models in steep topography. In the revised manuscript, we further clarify this section and distribute parts of this discussion to other sections, such as into the model description in section 2.3.1.

Regarding the simplifying assumptions in the temperature/pressure dependence of A (p. 398) in ice deformation and the water pressure (p. 399) in the sliding term: while these choices are partially motivated by numerical ease, we wanted a comparison using the simplest baselines of ice physics between the models. See the Detailed Comments below for further discussion related to both of these.

Referee 2 concludes that this model does not evaluate when and where SIA models are still valid. In this study, we do not necessarily prescribe these bounds, but we conclude that the choice of ice flow model must be made with care depending upon the questions asked and the domains being studied.

Detailed comments

p. 395: see General Response above

p. 398: Preliminary runs (not published) with thermomechanical coupling via the A factor showed only small variations over the long time scales used for this study. The effects were significantly smaller than those due to either climate or model choice. Due to this initial test, the choice was made to keep A as constant to somewhat simplify both models with minimal overall effects. A short discussion of these effects has been added to the revised manuscript discussion section.

p. 399: Using a simple version of the effective pressure definitely has a huge influence on the sliding velocity (and therefore the ice coverage, thickness, etc.). Given the complex interactions of sliding and hydrology, this has been kept simple to further emphasize the role of the ice physics only. We expect that more complicated hydrologies would likely only exacerbate the effects as shown. In the revised manuscript, this is now more explicitly stated and clarified in its current discussion in section 4.3.

P. 401: The spatial resolution of the COMSOL model is discussed in section 2.4, along with how the HO velocities are put back into the SIA model. While we send readers to the literature for most details on ICE-Cascade, we have added more information on the spatial resolution of this particular model.

p. 403: Since we have kept A constant for this analysis regardless of model choice, it does not seem to necessary to use another parameter in equation 13. This is also true for Asl in equations 14 and 7.

p. 404: The typo of “interglaciers” has been changed to “interglacial period.”

p. 407: The referee makes good points about our original statement, particularly with regard to simple models reaching the right result for the wrong reasons. We modify the statement to more along the lines of, “if a more complex model and a simpler model can be in agreement when the assumptions and approximations in the simpler model are valid, then the simpler model with fewer free parameters is generally preferred.”

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