

## ***Interactive comment on “Sensitivity of Greenland ice sheet projections to spatial resolution in higher-order simulations: the AWI contribution to ISMIP6-Greenland using ISSM” by Martin Rückamp et al.***

**Anonymous Referee #1**

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General comments: The ISMIP6 project is an important effort towards improving projections of the contribution to global sea level rise from the large ice sheets in Greenland and Antarctica. This paper by Rückamp et al. presents result from a particular model from the ISMIP6 project. They show that the projected SLR is sensitive to the spatial resolution in their model, and they show further that the effect depends on the climate forcing, with oceanic and atmospheric forcings showing opposite and non-trivial responses. Investigations of how ice flow modelling techniques and model parameters influence the projected SLR and the uncertainties is highly relevant and very important,

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and the main conclusions of this paper are interesting and merits publication.

While the scientific quality of the modelling work is high, the presentation and text are not at the same level. As a result, the purpose of the paper is not clear, the text needs to be edited and self-contained, and the conclusions could be more clearly communicated and supported by the text. The paper appears to be hastily written. Furthermore, the important relation between the resolution of the bedrock topography and the model resolution is not sufficiently discussed. Here are some important issues: 1. The purpose of the paper is mixed and not clear. I recommend that the authors focus on investigating the influence from the spatial resolution and the different effects on oceanic and atmospheric forcings. Please be sure to structure the text to emphasize this purpose. Remove the secondary aim, i.e. to describe how the ISSM-AWI contributed to the ISMIP6 exercise. This is out of scope here, unless necessary to understand the results of this paper, and should then be part of the methods section. Rewrite the last sections of the introduction to reflect this. The current version seems unclear, particularly the last paragraph, lines 85-91. 2. The introduction starts out being very general and later becomes very specific. Shorten the details of the work by Aschwanden et al., they seem to be too detailed for the introduction. 3. Remove all the additional comments about how the ISSM-AWI model contributed to the ISMIP6 project, e.g. lines 127-126, and several paragraphs in section 2. I don't think that this really supports the conclusions of the paper. 4. Again, comments about how the ISSM-AWI model is set up must be presented in a self-contained way in this paper, so please re-write section 2, and avoid structuring the text as an appendix to the paper by Goelzer et al. Also, remove the first sentence of section 3.3.2, as well as providing details of the ISMIP6 AWI-ISSM6 simulation, which is not reported in this paper. Also, change figure 2 to show results from a run presented here, and not from a G8000 run, which was not reported. 5. The inversion parameters are not discussed in detail, and neither their influence on the simulated velocity field, e.g. how the regularization term smooth out sharp transitions in sliding (in order to avoid oscillations). How does this relate to the spatial resolution, and how sensitive are the results to the regularization parameters? 6. The connection between

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the spatial resolution of the model and the ability to resolve bedrock topography should be highlighted more. This work shows that the results converge when the resolution is improved. But is the convergence just because the model resolution approaches the resolution of the bedrock topography map? Would the model results be different if finer resolution bedrock data were available? These are important questions, and they should be discussed in the paper, even if they cannot be fully addressed. In my opinion, the importance of high resolution bedrock data and the relation to the projected SLR is the most important contribution of this paper. Please make this come out more clearly.

7. Throughout the paper: the use of the word “dynamic” is not consistent. Dynamic is used to describe dynamic response, i.e. ice-dynamic changes due to ocean forcing, or dynamic residual, i.e. corrected for front retreat, and sometimes used more generally to describe ice dynamics. Perhaps use the word “discharge” when relevant to avoid confusion.

8. There are numerous errors in the use of English, e.g. proper instead of properly.

A few specific points: - The effective pressure, line 108: is this used generally, i.e. also in interior areas with bedrock below sea level? - Regarding the inversion, please reference the remote sensing velocity product (it is not clear which product is used). - Fig 3b – it is very difficult to see the colored areas. Please modify, e.g. change the black color of grounded ice to white, and remove all black outlines. - Section 4 first paragraph: provide units of the  $q$  and  $Q$ . - Figures S1+2: confusing caption: difference of  $a$  to  $b$ , what does that mean –  $b-a$  or  $a-b$ ? please clarify. (difference between  $a$  and  $b$  is  $a-b$ ).

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