

*Revisions of “GREB-ISM v1.0: A coupled ice sheet model for the Global Resolved Energy Balance model for global simulations on time-scales of 100 kyr”*

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Dear Editor and referees,

We would like to thank the two referees and the editor for their time spent on reviewing this manuscript, and for the many very helpful comments they provided. We think the referee comments have helped us to substantially improve the presentation of this work. Below we give a point-to-point response to all referee comments, hoping the revised manuscript has now been improved in clarity and is ready for publication.

With best regards,

Zhiang Xie, Dietmar Dommenges, Felicity S. McCormack, and Andrew N. Mackintosh

## *Referee #1*

The comments I made have been mostly addressed and I found this version of the manuscript easier to follow. The figures are greatly improved and much easier to read.

At this point I only have a few minor suggestions and one clarification to request after which the study would be ready for publication. The lines and page numbers refer to the latest version of the manuscript.

**Response:** We like to thank the reviewer again for the informative feedback, which helped improve our manuscript a lot. The detailed response is listed below.

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P5, l127: replace “thickness and temperature” by “thickness, velocity, and temperature”. Otherwise, it reads that you are solving 3 equations for 2 unknowns while you have section 3.2.4 that shows the SSA equation.

**Response:** We revised this sentence now state that we have two prognostic equations and the additional diagnostic velocities.

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P8, Sec 3.2.3.1: Thanks for adding this section. What I understand from it is that ice in the floating or grounded ice mask will need to thin down to less than 10m to become part of the ocean mask which then would become sea ice. If I am understanding this wrong, please add some details to this section. Also, please add a couple of sentences to describe what is happening to this ice that is now part of the ocean mask but has a thickness greater than 0.5 m. Later in the text, you mention that sea ice is not allowed to grow more than 0.5 m. Does this mean you quickly melt this thicker ice to be less than 0.5m? Please clarify this aspect here.

**Response:** The reviewer is correct, floating ice comes ocean grid point if it is  $< 10\text{m}$ . In Section 3.3.4 we explain how sea ice tendencies are computed. Here sea ice can be larger than 0.5m, but it cannot grow larger than 0.5m by atmospheric heat fluxes. The Sea ice transport by

isotropic diffusion ( $\kappa_{si} \nabla^2 H$ ) will reduce the 10m sea ice fast. So we are not quickly melting the sea ice, but it will be diffused by transport. We added some texts to highlight that only atmospheric heat fluxes is limited.

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P9, I239: what is the value of the constant viscosity?

**Response:** It is in Table 1. The table has been cited now.

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P17, I472 and 482: The SSA is an approximation rather than a parameterization. (It approximates the Stokes equation.)

**Response:** The parameterization we mentioned here is referred as to the viscosity parameterization for SSA.

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P17, I480: To be clear, BedMachine is a product for bed topography and ice thickness amongst other variables, but not velocity. Please remove its reference here.

**Response:** It has been corrected now. We used MEaSURES data as mentioned in section 2.

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P17, I482: “which is due to the parameterization...” I would leave this part of the sentence out or tone it down with a word likes “partly”. You have not explored many options to pinpoint “THE” reason of your velocity mismatch. As I mentioned in my first round of comments resolution is one of them, but your constraint on ice viscosity could be another one and your choice of inversion process.

**Response:** We revised the expression.

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P18, l505: replace “0.67” by “-0.67”, whatever is correct value between here and Fig. 10.

**Response:** Sorry, this is typo in the text. It is -0.67. To make a better comparison,  $\delta^{18}O$  proxy data axis has been inverted in Fig. 10 and we now clarify it in the caption.

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Fig 8: replace “Obsevation” by “Observation” in the title of the left column.

**Response:** It has been corrected.

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## ***Referee #2***

Review of Zhiang et al.

In the revised manuscript, the authors mostly addressed my concerns and questions raised in the previous review. In particular, they conducted additional sensitivity experiments similar to Niu et al. (2019) and showed that their new ice sheet model is capable of simulating changes in ice sheet over the glacial-interglacial cycle when reasonable atmospheric forcing is applied. On the other hand, I couldn't find sentences in the last section highlighting what kind of insight can the model give to better understand the ice-age cycle over the Quaternary. Note that in the response letter, it did say the authors included those sentences (page 20). I did find several interesting sentences in the track-trace file that attempt to make this point, but those sentences were deleted. Does this file really compare the most recent manuscript with the one that was published in the Discussion paper? Given this condition, I cannot make a decision at the moment. Below shows some comments that may help to improve the manuscript.

**Response:** Thanks for your comments. The uploaded version was the final version. We deleted those sentences since we realised the summary part has covered the main points of our model's new insight: global scale and high model efficiency. So, we think it has been highlighted already in the last section. However, considering your comments, we reinclude the detailed discussion about those new insights in the final section. We hope the current version does illustrate the advantage of the GREB-ISM.

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L408-409: The latter part of the sentence is inaccurate since the sensitivity experiment is forced with a combination of Greenland isotope record and model outputs from PMIP. Please modify it.

**Response:** It has been corrected by citing Niu et al., (2019).

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L423-424: It would be better to clarify that the authors are discussing the result of moving margin experiment here.

**Response:** Done.

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L428: Sorry if I have misunderstand, but I thought the authors are discussing the results from transient experiments in Fig. 6. If so, why is it citing Table 4, which shows result of steady state experiments?

**Response:** Yes, the reviewer is right. We are discussing the transient experiment. Here we should cite Fig. 6 instead of Table. 4. This has now been corrected.

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L497-499: I assume the authors are using only the raw LGM climate condition from AWI climate simulations. In that case, it would be better to just refer to  $TLGM(\lambda, \phi, t_{day})$ .  $((TLGM(\lambda, \phi, t_{day}) - T_{today}(\lambda, \phi, t_{day}))$  gives an impression the the ice sheet model is forced with anomalies between LGM and piControl in AWI model. However, that is not the case, right?

**Response:** Correct. Our LGM climatology is from AWI climate simulation while variables at present are from observation. We have modified the corresponding part in the text.

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