

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2021-204

Anonymous Referee #1

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Referee comment on "GREB-ISM v1.0: A coupled ice sheet model for the Globally Resolved Energy Balance model for global simulations on timescales of 100 kyr" by Zhiang Xie et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-204-RC1>, 2021

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This paper presents the new capability of the simple GREB model being coupled to an ice sheet model (ISM). This presentation introduces the new ISM itself and its coupling to GREB. The authors use a series of stand-alone ice sheet model intercomparison projects (EISMINT) to test their new ISM and a series of simple coupled simulations to investigate the feedback between the ISM and the climate system. This model has the great advantage to be computationally very cheap.

The paper is written in a logical fashion and is well organized. I will begin with general comments before addressing specific minor details.

Overall, I thought the authors were trying to combine too many model development details in a relatively short manuscript. While I appreciate the will to keep the paper concise it results in a lack of convincing arguments to make the reader believe the ice sheet model is behaving in a sensible way especially since one the goal here is clearly to capture sea level change. In particular, the paper lacks in basics sensitivity experiments that could shed more lights on some model limitations that are inferred in the text. For example, in the EISMINT experiments, the authors hint that the coarse vertical resolution might impact the strong differences in basal temperature. The model being very cheap, I would appreciate seeing a sensitivity to rule this out. Additionally, the horizontal resolution of the ISM is increased in the EISMINT experiments to better compare the results to the one published (I assume). Typically, good results at coarse resolution translates to as good or better results at higher resolutions. However, you plan on using your ISM on a much coarser global grid than the one used in the EISMINT experiments. I would have liked to see a comparison at that resolution as well.

There is a lack of discussion about the consequences of using such a coarse horizontal

resolution for ice sheet modeling while the literature has shown abundantly that a grid resolution of at least 4km if not higher is necessary to simulate ice sheets (Pattyn et al. (2012), Pattyn et al. (2013), Gladstone et al. (2010), Leguy et al. (2014), Seroussi et al. (2015), Seroussi et al. (2018), Cornford et al. (2020), Leguy et al. (2021), ...). This is especially important to simulate marine ice sheets like the West Antarctic Ice Sheet for which GREB-ISM show large differences compared to observation.

Many aspects of the ISM and choices are not clearly stated. For example, you choose to use the basal sliding velocity from Greve 1997. I am not saying you should not, I am simply asking to justify your choice while so many sliding laws are now available and studied (Weertman (1957), Schoof (2005), Schoof (2007), Aschwanden et al. (2013), Leguy et al. (2014, 2021), Tsai (2015), ...). Also, you mention in many places in your paper how well simulated calving matches observation, while you never clearly define how you actually calve the ice (unless I missed it in which case I apologize). In section 3.2.2, you mention that a condition for floating ice shelves is that  $H \geq 10\text{m}$ . Does it mean that 10m is a thickness threshold below which ice calves?

There is also no discussions of any sub-grid scale parameterizations (for grounding lines, calving fronts, ...), does the reader need to understand they aren't any?

I do note that the authors left a more in-depth study of the ISM properties for another publication, but if so, I almost feel like it should have come before publishing this paper to rule out many possibilities leading to large differences in the results.

I would also have appreciated a discussion on the strength and weaknesses of the model and emphasize a bit more the type of experiments for which the model is worth "trusting", and those that is best not. For instance, Figure 10. Clearly shows that the model is good at capturing trends and in some cases with a time lag of several  $10^{\text{th}}$  of thousands of years. This indicates that the model is not best used for short time periods whatsoever.

Finally, I would encourage the authors to improve their figures. I thought the choice of the color scale to be difficult to distinguish, especially the green scale and some labels hard to read. The figure captions need to contain more details for some of them. I will add more details for specific figures below.

## **Minor comments**

P2, l48: "Ice sheet modelling ...". It really depends on what you are doing, and some

models are capable in simulating changes on century time scales. Please rephrase.

P2, l59: replace CO<sup>2</sup> with CO<sub>2</sub>.

P2, l60: add "(ISM)" after "ice sheet model" as you will use it later (Fig2) and never defined the acronym. It is best to define in the text as opposed to in a table.

P4, l11: "state-independent". I think this deserve a longer explanation and clarification as it is key to understand your coupled experiments.

P4, l103: spell out CGCM.

P4, l115: remove and reword "due to limited space". GMD is not page limited.

P4, l124: "Four vertical layers are chosen". Why only 4? See general comment above.

P5, l140: What happens when  $H < 10\text{m}$ ?

P5, l141: Add below that  $H$  is used for both sea ice and ice sheet. You mention this way later.

P6, l156: space out " $T_m$ " and "and" in the equation.

P6, equation 11: what happens when  $F_{\text{surf}} = F_{\text{max melt}}$ ?

P6, l176: how do you convert snow to ice?

P6, l180: This is a bit confusing. In page 4, line 115 you refer to table 2 which describes the time step of the ISM which is 1 year. How do you obtain a seasonal cycle? Please, better describe what you are doing here.

P7, l186: This sentence is confusing, please reword. You do not show the Full Stokes equations here.

P7, equations 16 and 17: add a reference to which axis each equation needs to be solved for.

P7, l202: Correct " T' " to match the one in equation 19.

P8, l212: Can you motivate your choice of sliding law? (See comment above)

P8, l232: Can you motivate your choice of constant geothermal heat flux while observation is available?

P9, l247: how long will you run this simulation?

P10, l266: please add this equation here to limit the number of papers the reader has to look at to understand yours.

P10, eq.32. what happens to the energy that would typically be used to grow the sea ice but can't because of the 0.5m threshold?

P12, l340: can you justify this value of 0.3?

P13, l363: I don't understand the justification of EISMINT being designed on a cartesian grid in order for you to regrid your ISM. Please rephrase.

P13, eq. 37 and 38: please define the parameters in a table.

P13, l376: can you comment on the mass flux being 20% higher than EISMINT results?

P13, l378: replace "from" with "for".

P13, I380: see comment above.

P14, I404: delete "roughly".

P14, sec4.3: What do you invert for in these spin-up experiments? You have not mentioned anything about  $C_{sl}$  at this point. If you invert for this field, please provide a figure. What years does your climatology span?

P15, I423: See comments above. Grid resolution, choice of sliding law, ... could be part of the cause of your limitation.

P15, I434: You can omit this first sentence and begin your paragraph directly with "We next evaluate the capability of the global..."

P16, paragraph 2: big caveat to simulate sea level change. See comment above.

P16, I459: spell out EIVM.

P16, I465: add "of" after simulation.

P16, I469: "It is beyond ..." I don't think it is beyond this study to explore these deviations, since your model is very cheap to run and you're trying to validate the use of your ice sheet model configuration for future studies. See comment above.

P17, I491: WAIS growth is concerning, especially when exploring sea level change.

P17, I506: I suggest removing this sentence especially since the next sentence refers to results showing all 3 oscillation periods.

P18, I536: this might be a resolution effect even though these places have been covered by ice sheets at some times.

P19, I566: replace "ISM-GREB" by "GREB-ISM" to be consistent.

## Tables

Table 5: Please define the term "boundary calving". Use proper citation for BedMachine (author and year instead of "BedMachine"). I could not find the Martin et al. (2011) in your references, please add it. Also, Martin et al. (2011) cites observation and does not derive it. Please correct your table accordingly.

## Figures

Fig1: The green color scale encompasses too many layers and it is difficult to distinguish each of them. Please use an appropriate color scale. The blue scale is similar. Also, it is misleading to use white for thickness lower than 300m. Right now it looks like white=no ice. Also, please improve the figure caption. You are not only showing the topography but also ice thickness.

Fig3: This figure needs to be improved. It is really difficult to compare it with Fig1 due to the color scale and the range of the colorbar. Please use "ice thickness" or "ice surface elevation" instead of "ice height" which can be misleading. Same remark as in Fig1 for the color scale. Please, distinguish in your figure what is sea ice to what is ice sheet.

Fig4: Which year does panel (d) refer to? Improve the description of the figure in the caption. In particular, add what the black box is (it is cumbersome to go back and forth between figure and text to piece all the information together). Same remark as in Fig1 for the color scale.

Fig5: Please, indicate in the figure caption that the ice lifting takes place in the black box. Also, here or in the text, can you comment on why the response in  $T_{surf}$  is limited to the black box? Modify the color scale for panel (a).

Fig6: Define what "R" is in the caption.

Fig8: Same remark for the colorbar as in Fig1 for the yellow and red here.

Fig9: panels (a,b) Focus on showing Greenland only if you are not showing results for other Northern Hemisphere ice growth (similarly to fig8 (a,b)). For Antarctica, show the full range of positive ice thickness; its extend does not match the one from the velocity plot and it is confusing at first. Increase the font of the colorbar label. Regarding the colors, see remark as for fig1.

Fig11. Greenland looks quite small at -127ka. Can you comment in the text about it? I can't read the numbers on the colorbar, please increase the font size. Same remark for the color scale as in fig1.

Fig 13: same remarks as in fig9.

Fig14: Increase the axis title font to be the same as axis tick labels. Increase the font size of the labels as well. Indicate in the caption that the normalization is done with respect to the last control time slice. Also, in the caption you write "orange, unit:  $2W m^{-2}$ " while the label reads  $4W m^{-2}$ . Which one is it?

Fig15: Use bigger font for label and axis title. In figure caption, remove "that". How does the ice volume increase with negative precipitation over 25kyr and 50kyr in panel (b) and (c) respectively?

Fig16: same remark as in figure 1 for color scale.

Fig17: increase the font size of color bar labels.