

Review of manuscript gmd-2021-352 "Benchmarking the vertically integrated ice-sheet model IMAU-ICE (version 2.0)" by C.J. Berends et al.

General comments:

The submitted manuscript gives an overall clear and well-structured description of the ice-sheet model IMAU-ICE 2.0, which seems to be a suitable model for running long-term, continental-scale (paleo) simulations. The key developments that led to the release of version 2.0 of the model as well as its main features in general are described in a clear and understandable way. The benchmark experiments performed with the new model version are presented concisely, though in places I am missing a more in-depth discussion of the results (see specific comments below). In places more a few more references would enrich the manuscript.

I would support the publication of the manuscripts after the points below have been addressed.

Sincerely,
Johannes Feldmann

Specific comments:

My main point here is that while the number of benchmark experiments carried out for this paper is very convincing, I wondered why the MISMIP+ benchmark is not part of the analysis. It is the state-of-the-art benchmark regarding grounding-line stability and migration under the influence of strong buttressing. The experiments thus provide insight in how well a model can represent ice-flow dynamics on a smaller spatial scale. There might be good reasons why the authors neglected these experiments but I strongly suggest that the reasons should be at least mentioned in the discussion. Also, the prescribed spatial resolutions vary strongly between the different benchmarks carried out for this study. It would be helpful to give a short reasoning for the chosen resolution ranges. Please see the list below for further specific comments:

P1,L23: Not able to find van de Wal, 2019 in the reference list. Please consider other literature as well.

P1,L27-30, P2,L1-2: Consider adding literature that 1) gives examples of short-term future projections, long-term paleo simulations and 2) relate to the mentioned physical processes

P1,L24-26: This sounds like quite a strong statement to me. I am not sure whether this claim is explicitly supported by the cited study. I would suggest a different wording here. In detail, I don't see from the cited study that the SIA/SSA method has been shown to lead to unsatisfactory results.

P4,L3-4: This point is not entirely clear to me. I can see from Fig. 1 that there are three regions overlapping in the northern hemisphere. But I would wish to have a bit more detail on what is meant by double-counting. Does "no ice growth mean" in the mentioned regions mean that there will be no ice at all or does it mean that already existing ice cannot grow thicker?

P4,L6: Please add information to the figure caption on what the colors show (ocean + bathymetry/bed topography?)

P4,Eq1: I'm missing a brief explanation of the notation (indices x and y refer to derivatives, bars are vertical averages). Also, the description of the variables is incomplete (e.g. u and v)

P4,Sec.2.2: The introduction mentions the advantages of the DIVA approach compared to the hybrid SIA/SSA approach and briefly mentions which stress terms the DIVA approach covers. Sec. 2.2, that includes the mathematical equations of the stress balance would be suited to refer to these stress terms. I suggest to name which of the shown equations/terms correspond to which stress terms (SIA, SSA and additional stresses that are not captured by the SIA/SSA). That would give a lot more clarity on what the actual difference between DIVA and hybrid SIA/SSA is.

P6,L6-7: I would suggest to delete "the square of" for more clarity.

Figure 6: I am surprised that the velocity deviation of the SSA (red-dashed) to the Stokes reference (blue) increases with finer spatial resolution. Is there a plausible explanation for this?

P12,L16: Which version of IMAU-ICE is meant here? Please check also for possible other occurrences where the version is not given but relevant.

P13,L4: I wonder why the authors did not examine finer resolutions. A brief explanation here or in the discussion would be very helpful.

P13,L14: I am not familiar to the Robin solution. For the interested reader, at least a reference should be provided.

P16,L14: I would be interested in more details on the simplicity of the mentioned rheology, damage and subglacial hydrology. I recommend to discuss them here or to present details in the section 2.

Figure 9/10: As the shown results are very similar for ABUM and ABUK, maybe it is sufficient to show only one of the two figures in the main text (shifting the other into the supplement).

Figures B3/B4: There are no red-dashed lines visible in both figures. Does this mean that results from DIVA and SIA/SSA are identical here? If so, it would be good to mention this in the figure caption.

References: The list as it is presented makes it hard to identify the individual studies. It needs vertical spaces between the individual references.