Rebuttal to the review by an anonymous reviewer

We thank the reviewer for their insightful and constructive comments on our manuscript. We'd hereby like to address their concerns, and propose revisions to our manuscript to alleviate them. Reviewer's comments are displayed in boldface, replies in regular type.

Please note that all comments are answered in order, with one exception; since the reviewers comments about the sub-grid melt scheme, the sliding law, and the ABUMIP results are strongly related, we answer those together.

Major 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.

This is a good point, and one that was also raised by anonymous referee #2. We fully agree with both reviewers that the MISMIP+ experiment is very valuable. We are currently finishing up a project where we investigate the interplay between different sliding laws, basal melt parameterisations, sub-grid melt schemes, stress-balance approximations, and grid resolutions. We did this by performing two large ensembles of simulations; one with the schematic MISMIP+ geometry, and one with the present-day Antarctic geometry.

While we definitely want to publish these experiments, we feel that they are of a different nature than the work presented in the current manuscript. Whereas the current work consists of more basic numerical verification experiments, these new experiments move to a next level, studying the effects of different physical parameterisations on the rate of change of the Antarctic ice sheet. A discussion of this breadth and depth would require more text than could be feasibly added to the current manuscript, which is already almost 30 pages long. We therefore decided to move them to a separate publication, where we can leave out a lot of model description and benchmark experiments (as they will already be in the current manuscript), and spend the available text on a more thorough investigation of these model choices. We will mention these ongoing developments more extensively in the "future research" part of the Discussion section.

We here include a figure from the manuscript of this new project, showing the results of the default MISMIP+ simulations, compared to the ensemble results published by Cornford et al. (2020). As we see, the results of IMAU-ICE lie well within the ensemble range, indicating that the model performs well.

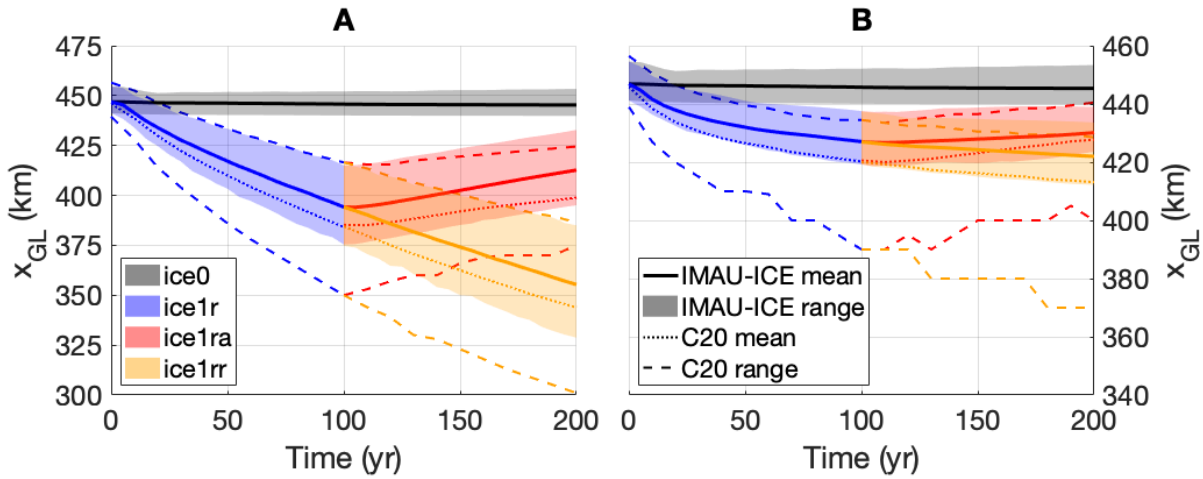

Figure 1: grounding-line position over time for the different MISMIP+ experiments. Solid lines (shaded areas) depict the mean (range) of results for IMAU-ICE. Dotted (dashed) lines depict the mean (range) of results for the model ensemble from Cornford et al. (2020). Colours indicate the ice0 (unforced control), ice1r (100-yr retreat), ice1ra (100-yr readvance), and ice1rr (100-yr continued retreat) experiments (see Cornford et al. (2020) or Asay-Davis et al. (2016) for a more detailed explanation). Panel A shows the ice1 experiments (fixed calving front, variable melt rate), panel B shows ice2 (high melt near the calving front).

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.

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

We agree that the choice of resolutions should be more consistent across the experiments. We have redone all of the simulations for the plan-view MISMIP, SSA ice stream, and ABUMIP experiments, at resolutions of 40, 32, 20, 16, and 10 km. The results remain qualitatively unchanged. This range of resolutions is motivated by the intended application of IMAU-ICE to palaeoglaciological experiments at a 40 km resolution. By showing that the model results remain unchanged in a variety of settings even at resolutions as high as 10 km, we provide confidence that the 40 km results are still reliable.
We will adapt the figures and the manuscript text to reflect these changes.

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

van de Wal et al. (2019) is already in the list of references.

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.

For the short-term future projections, we will add references to Goelzer et al. (2020), Levermann et al. (2020), Seroussi et al. (2020), and Sun et al. (2020). For the long-term paleo simulations, we will add Abe-Ouchi et al. (2013), Berends et al. (2018, 2019, 2021), de Boer et al. (2013), and Willeit et al. (2019). For the physical processes, we will add de Boer et al. (2014) for GIA, Berends et al. (2018, 2019) for feedbacks of ice-sheet geometry on the regional, and Abe-Ouchi et al., 2013 for changes in orbital configuration.

P1,L24-26: This sounds like quite a strong statement to me. I am not sure whether this claim is explicitely 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.

This reference is indeed incorrect; no SIA/SSA results were included in the original ISMIP-HOM study. We will instead refer to Goldberg (2011), who did make the comparison we were thinking of.

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?

It means no ice at all. For example, the North America region of the model contain a permanently ice-free Greenlandic island. We will clarify this in the manuscript.

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

We will do so.

P4,Eq1: I'm missing a brief expalanation 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).

We will add a table listing the model symbols, and we will clarify the notation.

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.

We will add these clarifications to the text immediately after Eq. 1.

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

We will not do this, as the response by anonymous referee #2 indicates that this is an important detail.

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?

The distances listed above the figure panels do not refer to model resolution, but to the spatial scale of the bumps in the bedrock (although the model resolution also changes between the experiments, to maintain a similar relative truncation error). The geometry of the experiment describes sinusoidal bumps superimposed on a sloping plane; the numbers refer to the wavelength of the bumps. Experiment A therefore implies a high aspect ratio (1 km of ice, 150 km wavelength in the bedrock bumps), so that both the hybrid SIA/SSA and the DIVA are reasonably accurate. In each subsequent experiment the aspect ratio decreases, making both approximations increasingly inaccurate, regardless of model resolution. We will clarify this in the manuscript.

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.

All experiments described in the manuscript are performed with IMAU-ICE v2.0, which is the model version being presented here. Only in the ABUMIP experiment do we compare to the previously published results of version 1.0, which is stated explicitly in the text.

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

It is an analytical solution for the temperature profile in an ice clumn, given a profile of the vertical velocity, thermal parameters, and geothermal heat flux. We will clarify this in the text, and add a reference to Robin (1955).

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.

We will briefly mention the treatment of rheology and damage in section 2. The (lack of) treatment of subglacial hydrology is already included in the description of basal sliding; pore water pressure is calculated solely based on bedrock elevation, following Martin et al. (2011).

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)

Taking also into account the comments by the other referees, we will combine these two Figures into a single one.

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

The SIA/SSA results had been accidentally left out of these two figures. Their results are indeed very similar to those by the DIVA (and both are close to the full-Stokes solution, since these experiments all have the same aspect ratio; the perturbation is in the bed roughness instead). We will update the figures.

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

The make-up of the reference list follows the Copernicus manuscript template. In our experience this will become easier to read after typesetting.