**The transferability of adjoint inversion products between different ice flow models**
**Minor revisions - response to referee comments**

Author responses written in blue.

In general, we have made small adjustments to the text where required by the referees, and have updated Fig.6 and Fig.7 according to suggestions.
We have split section 5.1 into two parts, results and discussion, as it was felt a clearer distinction was needed.
We have also added well-deserved acknowledgements to the two reviewers and the editor for their efforts to help us improve the quality of our paper!

Referee #1

This paper has improved significantly since my first review.
My comments are now mostly minor in nature.

Line 110. Completely optional, but it would be simpler to have a single equation (e.g., equation 2), with generic values for the coefficient (e.g., D) and vo, then simply say in Ua D=(Co+C)^(), in ISSM and Streamice D=beta^2. UA and SI use values of vo of xx and yy, respectively. ISSM uses vo=0 but….

We have decided to keep the original formatting, for a simple visual comparison of the sliding laws.

Line 194. Not clear to me what it means "hydrostatically inverting for the bed" (is this on grounded or floating ice). If on grounded, please elaborate more. If just on the shelf, please say inverting for thickness (or the bottom of or thickness of the ice shelf – not the bed, which implies rock, not water).

This sentence has been rewritten for clarity.

Line 212 to better justify the assumption would be good to add …parts of Ua's BC to zero, WHICH GENERALLY FOLLOWS THE CATCHMENT DIVIDE (assuming this is the case).

This is a good suggestion, and has been added to the text.

Line 230 – the part about interpolating to streamice. This seems true for some figures, but not for others. Why not just use the instersection of the two domains when comparing (as in earlier figures).

In all figures, the largest amount of non-extrapolated data as possible is included in the plots. The mask based intersection of domains is only applied to pairwise comparisons of differences between two models. The text has been updated to clarify this choice.

Line 348. The evolution… This sentence reaches a conclusion based on the results, before identifying what results are being discussed. Move sentence from line 351 before (i.e., so Figure 6 is called out first).

The sentence introducing Fig.6 has been moved to the start of the paragraph.

Figure 6. Please show each color line style in the legend, which should fit (use a two-column legend if you have to). It's too confusing the way it's currently presented.

The legend on Fig.6 has been updated as requested.

Figure 7. Please put "Forward …" along the lefthand edge, rather than right. Consider using separate color tables for floating and grounded ice. As shown, it says more about the melt conditions than the model response.

Fig.7 has been updated with a different colour scale to make changes over grounded ice more visible, and to move the row titles to the left side.

General comment about the last couple of pages. I found the discussion of the comparisons with other MIPs to be to apples to oranges. You have far fewer models and less variation in parameters. That doesn't mean your findings aren't significant; just you are reaching a bit too far to provide context. Also at the top of page 17, the ISMIP6 results cited here cover 85, not 100 years. They also cover a huge range of melt and SMB forcing. If you look at the results in that paper for fixed forcings, applied to WAIS, the range is much smaller. Please cite the more relevant numbers (see Figure 8-11).

There are certainly limitations to these types of comparison, which we have tried to make clear in the text. Regarding the initMIP paper we cite, the results are over 100 years (perhaps there is another paper as part of ISMIP6 which contains 85 year experiments?). We refer only to the control experiment in that paper, in which climate forcing was kept constant. The text has been updated to make this more clear. Explicit values for the more relevant comparison (the two ISSM runs included in initMIP) have been added to the text as suggested.

Referee #2

Thank you to the authors for the work on this revision and their great attention to details. The authors have clearly accounted for my previous comments, running a significant amount of new simulations and clearly detailing modelling choices and inherent limitations of each model. I think this extra work has strengthened the manuscript and gives the reader more transparency on what to expect when transferring inversion products from one model to another. I have only a few additional comments, which are suggestions rather than critical remarks.

In the end, I am very happy with this version of the manuscript. Like the authors, I particularly think that the results of the forward modeling over a 40-year period exhibiting ~10 to ~30-40% difference in SLR contribution is very encouraging for the application of inversion product transfer in the future. As underlined by the authors, I am sure that this work will be useful to many other modelers interested in such transfer of inversion product, either to save computing time (by reusing outputs from previous studies) or for intercomparison experiments.

[Abstract:] If space is not a problem, I would also add the value in mm in addition to the percentage variability of sea level.
• Line 24: specify the location of Ice Stream E (e.g., tributary of Ross Ice Shelf, Antarctica).
• Line 29-32: add a reference to Gillet-Chaulet (2020) who shows a promising ensemble Kalman filter method.
• Line 74: I would specify the period of observation: "[…] and has been accelerating over the last decades (Sutterley et al., 2014)"
• Line 75: I would change "can produce" for "produce"

All of the above suggested changes have been made to the text.

• Line 92 to 97:
o Although the paper is really aimed at ice-sheet modelers that know the different iceflow equations, I would give a small explanation about what is L1L2. Maybe one sentence stating the hypothesis and differences between SSA and L1L2.
o I would also specify here that ISSM and Ua are both FE models (since you introduce STREAMICE as "not a purely FE model" in line 157). Also indicate why STREAMICE is not a purely FE model.

Additional detail has been added to explain this description of STREAMICE, and the L1L2 approximation.

• Line 105: I think "rate factors are […]" should be singular
• Line 179: "inversion processes have performed" instead of "[…] has performed"
• Line 226: change "inversions run" run for "inversion runs" or just "inversions" (same in the caption of Figure B2)

All of the above suggested changes have been made in the text.

• Line 246-248 and Appendix B: It is interesting to see that Ua misfit largely increases when using B from ISSM and only inverts for $\beta_2$ (Figure 3b and B2c look really alike, especially when looking at the misfit on the Thwaites glacier tongue). I think you rightly point out that, in addition to the logarithmic cost function used in ISSM, the fact that the inversion of both parameters is sequent (i.e., B and then $\beta_2$) and not simultaneous could have a large impact.
• Line 294: I think that this statement could be linked with my previous comment: $\beta_2$ is fairly similar in all the models but the inversion of B in ISSM seems to suffer from the fact that $\beta_2$ is only using its prior value when inverting for B. Did you try to invert

for $\beta_2$ first and then B, or even reinverting for B a second time (I detail this a bit further)? Notice that I understand STREAMICE and ISSM showing similar B values on the shelve maybe suggest that this is not that important.

Inverting the sequence in a different order or reinverting for B were not attempted. This is certainly an interesting idea, and one which the ISSM users among us are interested to try in future. However, we have not added any new experiments to this manuscript.

• Line 326: Should not it be "sequential nature of ISSM" instead of "non-sequential" (since ISSM uses a sequence of B and $\beta_2$ inversion)?

This has been corrected.

• Line 375: what do you expect or see as differences between the GL retreat of SSA models and the L1L2? I think that this should be slightly develop since you point out the difference in the equations.

This sentence was actually in the wrong place in the paragraph, as it was supposed to be a separate point from the GL retreat. The paragraph has been rearranged.

• Line 424: "They each use different methods and employ different techniques during the inversion process, […]" looks a bit repetitive, maybe change for "They each use different inversion techniques […]"?
• Line 463: add a comma between "[…] are selected" and "the corresponding […]"
• Line 492-493: please consider adding the mean magnitude misfit obtained with the Interior Point algorithm (for direct comparison with the value you give for M1QN3).
• Line 516: "to be" instead of "be be"

All of the above suggested changes have been made to the text.

• Line 519: remind here the value of the Pearson correlation so that the reader can directly make the comparison with the value 0.513 given in line 522.

The Pearson coefficients (including those between the original experiments) are all displayed in Table B1, which is referred to at the end of the paragraph.

• Figure B3: Why is ISSM $\beta_2$ so low over Pine Island fast ice when using Priors 1? Maybe add a potential explanation?

A potential explanation has been added, attributing the change to the uniform values of B in Priors1. Since Úa is the only model which inverts for the values of B over the grounded ice, it is the model least affected by the choice of priors. In fact, it may well be the case that with a good choice of B, the prior for beta^2 would not have much affect on the outputs from ISSM or STREAMICE. However, we do not have the necessary results to prove such a hypothesis.

As it is highlighted in the paper, the “bad” result of the steady misfit in ISSM is not necessarily a problem when looking at the evolution of the misfit after a forward simulation. Maybe it is worth to clearly state this (maybe in the discussion) ; it is somewhat understood when reading the paper but emphasizing this could be useful.

An extra small paragraph has been added to the discussion to emphasise this point.

The figure 7 in the new version is indeed much more legible. Please, indicate that the black line is the grounding line. Could you also plot the initial position of the grounding line (right after inversion)? It would allow a better visualization of the grounding line migration.

The initial grounding line has been added in Fig.7, and the caption updated to refer to it.