Review of tc-2021-130, revised version
Dear Christian Wild with co-authors,

I have now gone through the revised manuscript and read your responses to the initial comments from reviewer 1 and 2 (myself). I must say that the authors have done a great job addressing the comments raised by both reviewers. This has turned out as an excellent paper. However, a few aspects remain, and I am going to suggest that some minor revisions are needed.

There a few things that should be addressed, outlined below. The line numbers refers to the tracked-changes document, tc-2021-130-ATC1.

HO vs SSA ice flow
Great that you now are using the same ice-flow approximation across the inversions and the forward run. I think using SSA for all is ok; however I do see that there are some differences between how well HO and SSA represents the observed velocity field, as shown in Illustration 3 in your response to reviewers. The continuous colormap for the difference maps in Illustration 3 (right-hand side panels) makes it a bit hard to compare the model – observed misfits (a red/white/blue difference colormap would have been better). I understand that you'd like to save a bit of computational time by using SSA for all, and I am 100% supporting you to stick to the same ice-flow approximation across all experiments. However I am tempted to suggest that you should use HO for everything, both because you have it so readily available in ISSM and also because you have already done half of the job. It would not (?) be so much extra work to use HO for the inversions as well, as they are all stress balance calculations, in contrast to the forward transient runs. I am aware however that the manuscript is at an advanced stage. Alternatively, the authors need to clearly justify why HO is not needed, and illustrate that the results and conclusions are essentially the same as with SSA only.

Minor/technical comments
L210. A very minor comment: I would write "recent" rather than "past" to distinguish from more historical/paleo-type studies.

L223-24. ISSM uses a Budd-type friction law (Budd et al. 1984), not Weertman, as you have written now. Please rewrite. The equation 6 is still correct though. In my initial comment, I wrote that

"Regarding the friction law (Eq. 5), you should mention that you are assuming a linear Budd-type law, and that this is just one among several possible friction laws in the literature, and whether/how you think using other friction laws often used in Antarctica (e.g. a Schoof or Tsai law) would influence your results, if at all (this could also be done in Discussion)."

I cannot find where you have addressed the latter point (how using other laws would influence your results).

L226. Great that you have now added the equation for the effective pressure N. However, you have not yet addressed the first and the latter part of my initial comment. A sentence or two would be sufficient.

"Need to mention what the assumption is for the effective pressure (perfect hydrological connection between the ocean and the grounded ice base), what the equation for N is (can be done in-text), and also what processes/factors this formulation of the effective pressure neglect."

L250-52. Please explicitly state (in parentheses) what a Dirichlet and Neumann boundary condition means in practice here, to help the non-modelers among the readers.

L553-55. Thanks for rephrasing in a clearer, IPCC-like way. Perhaps I am being picky, but I think that the "very likely" ungrounding is still an hypothesis; I would be careful here. I'm not sure that "very likely" is consistent with the new, more balanced phrasing in L347-50. If you postulate ungrounding within the next decade (which I interpret as an anticipated acceleration of current thinning rates?), I would explicitly state why that is "very likely" to occur on the shorter time scale. What processes/feedbacks makes it "very likely" that current thinning will accelerate and ungrounding will occur sooner than later?

Figures

Figure 3b. Colorbar label not complete, m what?

Figure 7. (a). colorbar label should be ice surface (m a.s.l.); (b). colobar label should be bed elevation (m a.s.l.)