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# *Interactive comment on* "Design and results of the ice sheet model initialisation experiments initMIP-Greenland: an ISMIP6 intercomparison" *by* Heiko Goelzer et al.

**Anonymous Referee #1**

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### Summary

This paper summarizes models, methods, and the results of ice sheet model initialization experiments for the Greenland ice sheet, which are (eventually) targeting the ISMIP6 contribution to CMIP6. The broad motivation for the exercise is the recognition – largely motivated by previous, community-endorsed ice sheet model intercomparisons – that decadal and century scale simulations conducted using present-day ice sheet models are keenly sensitive to the model initial conditions (so much so that transients in the initial condition often swamp the ice sheet model response to the prescribed climate forcing of interest). In this paper, the different initialization methods most com-



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monly used by the ice sheet modeling community are discussed, as are the perceived pros and cons of the different approaches. A set of forward model experiments are conducted, which are intended to clearly expose the magnitude and trend of (1) model initialization transients under steady forcing and (2) model transients under idealized climate forcing, here applied in the form of surface mass balance (SMB) anomalies.

While the results of the simulations and paper are not necessarily in themselves scientifically compelling, they serve as an important record and waypoint for the purpose of documenting the current practices and capabilities of different modeling groups, as well as the differences in standard model outputs when applying these different models to the same set of experiments. Therefor, while this paper does not necessarily represent any major scientific breakthroughs, I think it is worthy of publication in TC purely for the purpose of clearly documenting where the ice sheet modeling community stands on these issues at this time. The paper represents the combined and significant efforts of a large number of independent ice sheet modeling groups, including the countless hours spent on model development and testing prior to applying the models to any experiments.

I disagree a bit with the way some of the conclusions for the experiments have been presented or "pitched" here. For one, it's not clear to me that the spread and/or size of the transients in the steady-forcing initialization experiment do actual show improvement over previous efforts. This could be due, in part, to the way the data are presented (discussed further below). It could also be that a simple quantitative metric from this study, relative to previous studies, needs to be included to support this statement. Further, the fact that the perturbation experiment uses anomalies applied on top of the SMB used for initialization is already an indication that we aren't looking at an apples-to-apples comparison of response between models. For example, if an actual SMB forcing (as opposed to a set of anomalies) was prescribed for the perturbation experiment, the spread in sea-level-equivalent mass loss shown in Fig. 8c would likely be much larger. I don't necessarily disagree with this choice, but the summary statements don't seem entirely accurate.

One obvious place for improvement is in the conclusions, which currently do not provide any clear guidance as to a "path forward", in the sense of what did we learn here and where should the community be working towards in order to address some of the issues and problems discussed in this paper. It seems like one obvious conclusion that could be stated more clearly is that either using data-assimilation OR spin-up approaches in isolation is not a long-term solution. Rather, using some combination of these two approaches is going to be necessary if the goal is to initialize models to have both a realistic initial state while simultaneously minimizing unrealistic transients (good) or representing actual observed ice sheet transients (better). There are some references (noted below) that could be included for these types of approaches, which aren't currently being applied at the whole-ice-sheet scale, but which should be viable for doing so in the near future. This would help to end the paper on a more positive, forward-looking note.

Below, comments are tied to specific points in the submitted text using the key "X, Y", where X refers to the page number and Y refers to the line number.

**Major Comments**

1,18: I'm not sure I really agree with the last sentence. If the authors really think this is true, then they should back it up quantitatively somewhere in the text. Is the drift referred to here for the init. and steady-forcing experiment? The SMB anomaly experiment? Both?

2,3: "encapsulates most of the modeling decisions . . ." – I don't entirely agree with this. A major "modeling decision" would be whether or not I choose to use SIA or a Stokes model, but I can imagine using the same init. procedure for both if I chose to do so. It might make more sense here to just point out that the decisions made during the init. process have a very strong impact on any model outputs that follow from using that initial state. I think the point you are trying to make here is simply that these choices are critical as they have very large consequences for “projections”.

2,28-30: The last sentence here is not really true. For example, there are fairly extensive “internal” constraints available in the form of radar layers, dated at ice core sites and tracked over hundreds of km of the ice sheet. Even though we are not regularly using these data now as constraints, we should be, or could be in the future. Maybe this is a point to make in the conclusions / future directions part of the paper, with appropriate references to some of the work by Joe MacGregor (which is particularly relevant to Greenland)?

3,28-30: “All three methods . . .”. Note that these could / can all be combined, and this is probably the future direction we as a community should be moving towards. It might be worth noting that here, even though none of the currently participating models do this (and then discuss this again in the conclusions by pointing to a few papers that are exploring what would truly be called data assimilation – that is, trying to assimilate observations of change (like dh/dt) and/or fields that include a record of change over time (like temperature, layer shapes)).

3,7-33: In general, I think the major problem being discussed here could be stated a bit more succinctly. That is, the spin-up approach provides an initial condition that has a realistic internal TRANSIENT with respect to some applied external climate forcing, but also generally gives us an initial STATE that is a poor representation of the present day. The current incarnation of optimization approaches does the opposite: we get a good representation of the present-day ice sheet state, but a very bad representation of the internal transients. The solution going forward should be to formally combine these two seemingly at-odds approaches through improved data assimilation approaches.

5,2-3: It is probably important to be explicit about the “lineage” of models here. While there are 17 group contributions, more than half of these are from “repeated” models (i.e., 9 of the models listed are either ISSM, PISM, or SICOPOLIS), so in effect, there are only really 11 different models represented here.

6,2-23: It might be nice to point out that the division of the three initialization strategies is a bit arbitrary, and not perfect. For example, most of the models that use data assimilation also use some form of spin-up (e.g., for ice temperatures). Also, the “DAs” method is really a sort of “ad hoc” approach to assimilation – instead of using formal methods to iteratively minimize a PDE-constrained, observation-minus-model cost functional, these approaches are minimizing the observation-minus-model difference using some other method (i.e., adjusting the basal traction locally to minimize the mismatch with surface elevations). I appreciate that some categorization needs to be made here to bin these approaches, but it’s worth pointing out that the categorization is far from perfect. It’s actually quite fuzzy and this is because it’s starting to be obvious that some combination of these different methods is required.

10,5-21: It’s not entirely clear to me what the purpose of this section is. It’s missing a clearly motivating intro / closing sentence to make that clear.

Figure 8: If you are going to keep panel (a) of this figure as is, then I think you also need to show a 2nd figure where you normalize by the initial volume (in SLE). As shown, these trends all look very reasonable but this is because you are showing the rate of absolute, rather than relative change. When normalized by the initial volumes, we would more easily see the volume changes with the more relevant units of cm rather than m. In panels (b) and (d), what time periods are the trends calculated over? In panel (c), the vertical axis label is “mass change”. For this, a positive sign would suggest ice sheet growth. Should the wording be changed (e.g., “mass loss”) so that it is clear we are looking at mass loss (which is obvious if we are talking about SLR)?

16,9: “inconsistencies between ice velocity and geometry datasets”. The reference to Seroussi et al. won’t help the unfamiliar reader here. I suggest you just be explicit about what the problem is: the optimized model velocities, while possibly being consistent with observed surface velocities, conspire with thickness errors to give a flux divergence that is wildly different than and unbalanced by the local SMB. This is where the large and unrealistic thickness transients come from when conducting a forward run following initialization using a velocity-based-only optimization approach. Note that the uppermost panel in Figure 10 shows this nicely – the very noisy, large amplitude thickness adjustments in the ISSM model are a manifestation of this (relative to the much more subtle thickness transients for the spun-up ICIES2 and PISM models).

19,16-18: This is left hanging a bit. As noted above in the summary comments, instead of simply pointing out that none of the methods discussed here prove to be optimal for initialization, you could take the opportunity to discuss recent efforts that target these deficiencies (even if they are not yet recognizable at the full-ice sheet scale) and push for their exploration, development, and wider adoption. I'm thinking in particular about formal efforts that attempt to include additional constraints and / or assimilate time dependent observations (for example, Goldberg et al., TC, 7(6), 2013; Perego et al., J. Geophys. Res., 119, 2014).

19,20: "DAv is the method of choice for short-term projections." I strongly disagree with this statement. Unless you are talking about applying anomaly forcing (as in the perturbation experiment discussed here), this is absolutely not true. Most often, these are going to have the worst and most non-physical thickness transients when stepped forward in time under realistic climate forcing. Only if that climate forcing has been taken into consideration during the optimization process do these have a hope of minimizing non-physical transients. This can clearly be seen in the top left figure of Figure 10 (the ISSM, DAv initialized model has a noisy, large amplitude thickness transient that is very unphysical). In general, this paragraph is not true unless you are talking about anomaly forcing experiments, which I think in general, the community is trying to move away from (for example, an initial condition generated to work well only when applied in an anomaly forcing experiment is of little use for coupling to a climate model, since climate models are generally required to conserve mass, and anomaly forcing does not do that).

20,7-9: I am not yet convinced that this statement – here and in the abstract – is supported by the results shown in this paper. It could be the case, but based on Figure





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8a, it is very hard to judge. We would have to see Figure 8a normalized by initial ice sheet volume to be convinced of this. Also, if this statement is going to be supported, it would be nice to do so quantitatively based on previous intercomparison results (e.g. compare the spread from a similar experiment from SeaRISE with that from this study).

20,11-13: "If this trend continues . . .". While I agree with this statement, I don't think it is for the same reasons as discussed in this paper. If there's any reason this trend will continue to improve, it's that better formal optimization and initialization methods are being developed and applied to ice sheets. That is, methods that take into account not only observational constraints on the current ice sheet state, but also observational constraint on short (i.e. dh/dt) and longer (i.e., temperature profiles) term trends that are inherent to the ice sheet. Again, I feel like there is a bit of a missed opportunity in the conclusions of this paper to point out and promote some of these new directions and methods that should effectively minimize many of the problems highlighted and discussed in this paper.

**Minor / Editorial Comments**

Title: Check that the spelling of "initialisation" is correct? Autocorrect seems to prefer "initialization".

1,2-3: Should summary refs. for the SeaRISE and Ice2Sea projects be included here? Similarly, should ISMIP6 and CMIP6 efforts here point to some generic reference publications?

1,6: ". . . to estimate the associated uncertainties." (in what? Model outputs?)

1,10: schematic -> idealized ?

1,13-14: Is there really a wide diversity in the data sets and boundary conditions used? It seems like for the most part, the models are using similar datasets and boundary conditions (for the latter, I mean w.r.t. "observation-based" boundary conditions – I understand that things like basal boundary conditions, which include assumptions about sliding, thermal state, hydrology, etc., probably DO vary widely).

1,21-28: The initial reference to EISMINT, followed by a listing of many other non-EISMINT intercomparisons, is confusing. I understand that you only mean to use the categorization that EISMINT suggested, but it could be read as if everything underneath (e.g., ISMIP-HOM, MISMIP, etc.) was also “part of” EISMINT. Suggest rewording this slightly?

1, general: Do CMIP and ISMIP6 need to be defined here?

2,10: “removing mass at the margins” – this is a little unclear, as SMB also removes mass inland (although maybe this mass is transferred to the margins, e.g. as melt).

2,32-34: I would be more emphatic about this point – without very special care, the transient from initialization can entirely dominate the response in decadal / century scale projections.

2,41: When you say run forward in time here, it might help to add detail about the timescale you are thinking about (e.g., thousands to tens of thousands (more?) years, rather than decades or centuries).

3, 2: “the model’s state is internally consistent” – It isn’t clear what you mean by this. The model’s internal state is always internally consistent, regardless of the type of spin-up you do. I think what you mean is that models that go through a spin-up are, at any point in time, closer to being in equilibrium with the applied climate forcing, or at least contain internal transients (e.g., in temperature) that are more consistent with external forcing transients. I appreciate this is a subtle difference to try to express.

3,10: “by inversion” – be a bit more specific here. I think you mean formal, PDE-constrained optimization?

3,16-18: Other “errors” that are forced to be accounted for by optimized parameter fields include observations and model state variables that are entirely ignored in the optimization process. For example, one of the biggest reasons current optimization approaches lead to such large, unphysical transients is that they are ignoring whether or not the temperature (and hence rheology) is consistent with observations (and as such, all of that uncertainty gets pushed into the basal parameter optimization).

3,19-22: Here, I think you need to be explicit in noting that the thing that is being adjusted is the bedrock elevation (i.e., we have good constraints on the ice sheet surface elevation, but much less so with the bedrock elevations. So the thickness is adjusted with the assumption that the uncertainty is the bedrock location, not the upper surface location).

4,5: “to a large perturbation” – add “in SMB forcing”? or “in climate forcing”?

4,17: “. . . that all boundary conditions AND FORCING remain constant in time.” ?

Table 1: “schematic change of SMB forcing” -> “idealized change in SMB forcing”

Table 2: Is the list of “contributors” consistent with co-authors on the paper, or is it supposed to be more of a list of co-authors for the respective modeling efforts?

6,12-14: Note that SP approaches are generally also favored when the goal is to include an ice sheet model in a coupled climate model?

6,21: Formally speaking, SMB is not a boundary condition, but rather a source term in the mass conservation equation.

7,7: Suggest: “. . . that serve to evaluate the response of these initial states to ...”

7,10: “boundary conditions and assimilation targets” (plural)

7,17: “. . . analyzing models WITH RESPECT TO their individual . . .”

8,8: “prescribe a fixed ice mask” – clarify what you mean here. I think you mean that they just zap away any ice moving past a certain location (e.g., present-day extent)?

Figure 1 caption: “A complete set of figures . . . IS given . . .”

9,1: “much smaller than to area.” – provide a percentage value here?





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9,1-3: I think the summary point here is that the areal expanse of the modeled ice sheets differs quite a bit w.r.t. observations, but that in terms of overall ice sheet volume, the differences are smaller (because even if you have an extensive error in the areal coverage, that marginal ice is presumably pretty thin).

9,10: Again, SMB is not really a boundary condition in any formal sense.

9,13: RCM = regional climate model? Not yet defined?

Figure 3: Note whether or not these 3 samples are from the middle of the distribution or if they span the distribution?

11,15: “good agreement with the observed” – awkward

11,15-16: “. . . is more important . . .” Not clear what “more important” is in reference to here. More important to what?

Figure 5: The units on the axes of figures in panels b and c are hard to read. Enlarge?

Figure 6 caption (and elsewhere): “Root mean square error (RMSE) . . .” (define “RMSE” used in figure axes labels).

13,9: “occurring at the margins” – be explicit here that what is important is that these errors are occurring over a fairly small fraction of the total ice sheet area?

13,17: “internal consistency” – this phrase is used fairly often without it being very clear what is meant here. I think in most places, it simply means that the long timescale ice sheet physics (like temperature) are closer to being in equilibrium with the relevant climate forcing (or have a reasonable and realistic transient).

14,1: “. . . at the expense of a larger discrepancy with the observed geometry.” As noted above, more recent efforts include attempts to improve on this problem by including additional observational constraints beyond velocities (e.g., like the SMB forcing). See additional related discussion in Major Comments above.

14,9: “for almost any given rheology.” – You may need to expand on this point for the non-initiated. What you mean is that you can generally get a good match to observed velocities by simply putting all of the motion into the sliding field and ignoring the fact that you might have the internal rheology entirely wrong.

Figure 7: The “v” and “s” symbols here are very hard to see.

15,2: “Recent MODELED mass trends . . .” ?

15,9: “which is currently not available” – Realistically though, if the data was available, would most of the models discussed here be able to apply it in a useful way? It’s not really just the lack of data availability that is the problem here but also that the models aren’t really in the position yet to make good use of these data.

15,10-13: And, the models trying to do hindcasting experiments generally still suffer from the same initialization problems you are focusing on here (either getting the initial ice sheet state right, or the trends right, but not both).

15,20-22: “. . . but more often . . . during initialization (i.e., the trends are dominated by the model’s relaxation following an initialization “shock”).”

16,30: Close with something like: “Simply put, by design a larger ice sheet will be subject to larger rates of mass loss.” (note that one could try to correct for this by normalizing rates of mass loss relative to initial ice sheet volumes).

19,8: suggest: “. . . in other words, the models largely agree in their representation of the ice dynamical response to the applied SMB-anomaly forcing.”

19,10-11: I don’t know if I would call the range of approaches “wide”. A more accurate description might be “representative”.

19,20: “at the expense of long-term continuity.” I don’t follow what this means.

19,35: “differences in model ice density”. I don’t think this was discussed anywhere in the paper up until now.

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