

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2021-175

Alexander Robel (Referee)

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Referee comment on "Brief communication: A roadmap towards credible projections of ice sheet contribution to sea level" by Andy Aschwanden et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-175-RC1>, 2021

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This brief communication from Aschwanden et al. argues for a new approach to organizing future iterations of the Ice Sheet Model Intercomparison Project (ISMIP). ISMIP is a community-driven effort to collect and compare simulations from ice sheet modeling groups that are capable of simulating ice sheet evolution relevant to sea level rise from the recent past and the near future. The sixth iteration of ISMIP has recently concluded, with a series of papers in The Cryosphere reporting the outcome from this inter-comparison exercise (Goelzer et al. 2020, Seroussi et al. 2020). There are two main thrusts to the suggestions made in this manuscript:

1. ISMIP should make an effort at formal uncertainty quantification using standardized sets of parameter-perturbed ensembles
2. ISMIP should calibrate projections of future ice sheet change by comparison with observations of past ice sheet change.

Overall, I think this is a valuable manuscript with a message that should be considered by the ice sheet modeling community and particularly those who organize and participate in ISMIP (particularly as ISMIP7 begins to ramp up). There are places where it can be improved, and I have provided constructive suggestions in this regard below, organized into more significant conceptual issues and minor textual/technical issues.

Major points:

1. It seems accurate to say that section 1 and figure 1 are meant to be the "problem statement" of this manuscript, drawing attention to the shortcomings of the projections from the ISMIP6 multi-model ensemble, with a particular focus on simulated cumulative mass loss from Greenland. While I agree with the general sentiment (and I think most ice sheet modelers would as well), I have some conceptual issues with how the argument is made here that prevent me from being 100% convinced:

(a) It is not obvious that a cumulative metric, such as the one that is used in Figure 1 is the correct one to make the point that there is a mismatch between models and

observations. In particular, use of cumulative mass loss in the Figure makes it hard for me to assess whether models are consistently (through time) underestimating mass loss, or just at some point, leading to a persistent offset with respect to observations. The fact that the mismatch between models and observations doesn't appear to grow in time would indicate that the mismatch in mass loss rates is not consistent through time. It would perhaps be helpful to also provide a plot (or a second panel to this plot) showing instantaneous loss rates in simulations and observations to determine whether there are any times during the historical period where models are able to reproduce observed loss.

(b) Related to this issue, I think the following sentence (L45-46) is carrying a lot of the rhetorical weight of this section: "Underestimating recent mass loss likely translates into underestimating mass loss at 2100 as well." This is not obvious to me, and I'm not sure you've provided sufficient evidence here to support this statement. Particularly, it assumes that the sensitivity of the modeled ice sheet change to climate forcing will remain similar (or at least the gap of this sensitivity between models and observations) between the recent past and the next century. Given that we know there are many aspects of ice sheet dynamics (SMB, MISI, etc) that lead to strongly nonlinear and changing sensitivities, this statement seems hard to support without evidence. I think it is fair to say that a model that can't reproduce the past is unlikely to be skillful in predicting the future, but speculating about the direction of this disagreement seems ill-advised, unless you have evidence to support.

(c) At various points throughout this section there is switching between referring to Greenland ISMIP projections, and all ISMIP projections. Yet, you have only shown this mismatch of cumulative loss for Greenland. Could you plot the same thing for Antarctica? Would it show the same mismatch? Given the recent manuscript by Slater et al. (2020) showing a better match from the Antarctic projections, my guess is that it would show that observations are tracking the high end of simulated Antarctic loss, but within the range of ISMIP6. Perhaps this does not make the exact point you are trying to get across here, but it would be a more accurate representation of the full ISMIP6 exercise, which included both ice sheets. Otherwise, focusing your discussion here on Greenland and discussing why the same mismatch might not be true in Antarctica would be a more comprehensive assessment of ISMIP6.

(d) It doesn't seem fair to compare observations to simulated mass losses where you have removed the unforced control simulation. It is argued in the Data Availability section that this "is intended to account for unforced model drift and mass loss committed as a result of non-equilibrium ice sheet conditions at the start of the simulations". However, the real Greenland ice sheet was probably not at equilibrium in the latter half of the 20th century, thus you are not making a fair comparison between the two, and potentially biasing to less mass loss over the simulation period. I would suggest not to remove the control simulation.

2. There has been considerable effort in recent years to improve UQ and Bayesian calibration best practices in ice sheet modeling, which hasn't been cited here, particularly: Schlegel et al. 2018, Bulthuis et al. 2018, Nias et al. 2019, Gilford et al. 2020, DeConto et al. 2021 (among others already cited including the study led by the lead author focused on Greenland). My takeaway from surveying this work is that the field is moving in the

right direction, but not all groups have adopted the state-of-the-art practices in UQ and BC, with a large part of the reason being a lack of computational and financial/human resources, which are needed to do these sorts of resource-intensive ensembles. My suggestion would be to modify the message (perhaps softening it) to give credit where it is due (these uncited studies), and indicate how these "best practices" can be integrated into our-community wide intercomparison exercises (or perhaps have a completely separate inter-comparison exercise that is more UQ-focused). My hope would be that making these methods part of the standard intercomparison practice would spur a larger fraction of the community to be working on these problems, even outside ISMIP exercises.

3. While the mathematical formalism you adopt here is certainly nice and clean for separating and explaining the various types of model uncertainty, it is not clear that all sources of uncertainty can be separated so cleanly in reality. Particularly relevant to the suggested experimental design for ISMIP (of asking modeling groups to run ensemble simulations with a prescribed set of parameters) is the distinction between  $P(M)$  and  $P(k)$ . Not all models have the same parameterizations. Not all models have the same numerical implementations of the same parameterizations. Some models have processes that no other model includes (e.g., MICI calving, temporally-varying subglacial friction). Thus, the structural model prior and the parameter prior are already convolved in many ways. Sometimes this can be controlled for (i.e. turning off the non-standard process), but sometimes it is built directly into the numerics of the model in a way that hinders simple inter-comparison across models. It would seem useful for the authors to suggest some ways that this issue can be addressed by the ISMIP organizers if this suggestion is to be taken up.

Minor suggestions:

L23: attendant contribution to sea level exercise

L27: ice sheet change

L54-55: Confusing sentence

L62: Similar to point #3 above, since climate and ice sheet geometry feed back on each other are  $P(F)$  and  $P(M)$  and  $P(k)$  separable?

L65-66: This point about deterministic dynamics is somewhat in conflict with your later points about aleatory uncertainty. We would need to have perfect observations of all initial and boundary conditions along with a complete set of equations to have a system with deterministic dynamics.

L79: is it actually a random sample?

L86: its probably worth it to mention InitMIP here

L89: why not?

L98: the fast and small-scale fracture processes

L99: captured in a large-scale model with long time steps

L107: simulation of Greenland Ice Sheet evolution

L112: It has been argued that some ice sheet processes are sufficiently complex and chaotic as to be considered part of aleatory uncertainty: calving, subglacial hydrology, etc.

L121: worth it to cite Hoffman et al. 2019 here too

L144: sentence starting "Depsite..." is confusing

L146: parametric uncertainty for the contribution of the Greenland Ice Sheet to sea level exercise

L161: joint omission of...

L165-166: observations is used twice in this sentence

L167-168: confusing sentence

L170: should cite Edwards et al. 2021 here

L173: ...uncertainties in intercomparison projects. [Again, the point being that such

approaches have been used in the field, but not in ISMIP.]

L177: to sea level must include all types of uncertainty simultaneously...

L183: with many realizations of random climate...

L189: uncertainty produces a prior distribution...

L200: be a bit more clear - is the argument that modeling groups should not do any of their own calibration for simulations submitted to ISMIP (under the proposed plan)? Does inversion for initial conditions count as calibration? Moreover: should all the calibration be conducted centrally (i.e. after the fact by the ISMIP team using standardized tools)?

L209-210: I'm not sure multi-decadal observations of ice sheet change are able to rule out the possible effect of internally driven climate variability. 30 years is not a climatology for ice sheet changes which have intrinsic response time scales of many to decades to hundreds of years. Moreover, one of the challenges of the limited observational record is that climate variability during the observational period may intrude a bias during calibration (e.g. calibrating ice sheet models on the changes that have occurred at Sermeq Kujalleq/Jakobshavn over the past 30 years when they have also experienced a large internally driven warming event.)

L227: DeConto et al. 2021 and Gilford 2020 useful to cite here too

L230: the framing here is US-centric (particularly for an EGU journal)

L234-235: is there any way to include an estimate of how much of OPP budget actually went to modeling/SL projects (even just for 2019). This would be an incredibly useful number to have on the published record.

L240-241: I'm a bit confused by this. Is the issue that ISMIP is voluntary or that it isn't financially support at an appropriate level? I'm pretty sure the solution isn't to force modeling groups to participate (making it non-voluntary), but rather to incentivize groups to participate by making funds available to support the work and computation that require some of the changes you are suggesting.

L256: computing resources, will free ups scientists to continue conducting basic science reserch, while the global community benefits from needed advances in applied science (i.e. reliable sea level projections).