Review of Jesse et al. “Sub-shelf melt pattern and ice sheet mass loss governed by meltwater flow below ice shelves”

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I feel I am always a better reviewer when I am not anonymous and I encourage others to consider reviewing non-anonymously whenever they feel able.

I also want to apologize to the authors for the lateness of my review. It was the result of a personal matter and in no way reflects a lack of enthusiasm for this work.

General Comments:

This manuscript describes a set of idealized experiments using a coupled ice sheet and 2D sub-ice-shelf boundary layer model (IMAU-ICE and LADDIE), comparing the LADDIE results with those using simpler parameterizations that are widely used in the ice-sheet modeling community. The experiments are based on the MISMIP+, ISOMIP+ and MISOMIP1 setup, using the same bedrock topography and initialization along with a qualitatively similar forcing approach. The main findings of this work are that the 2D representation of the sub-ice-shelf flow can capture significant physical processes that are missing from simpler parameterizations, and that these processes have a major impact on both the timing and the nature of ice-sheet mass loss and retreat. The authors make a compelling case that 2D plume models like LADDIE can provide substantial improvements in physical accuracy at high model resolution (2 km in this study) compared with simpler parameterizations while remaining substantially more affordable than 3D ocean models.

I found the results compelling and the paper to be well structured and well written. The figures and tables do an excellent job of supporting the paper. The choices of what material to put in the main text and what to present in appendices also seemed great to me. The numerous experiments are well designed and the results are compelling, and complementary to ongoing work in realistic configurations with these components. These findings are also complementary with the MISMIP+, ISOMIP+ and MISOMIP1 results, which are in various stages of publication.

I have a number of more specific comments as well as a few suggested grammatical and typographical corrections below. After some minor corrections, I think this paper will be ready for publication.

Specific Comments:

l. 17-19: “The parameterisations either inherently overestimate the persistence of margin thinning, leading to a sustained strong volume loss, or they underestimate margin thinning, delaying the onset of strong volume loss.” In the main manuscript, I think you do an excellent job of providing enough context that it is clear why LADDIE provides the higher physical fidelity and thus can be postulated to be closer to the “true” solution. I would be careful here in the abstract, though. This sentence in particular makes it seem like the results with LADDIE are the truth that can be used to evaluate the deficiencies of the parameterizations. I would strongly suggest you tone this down by adding something about "compared with LADDIE results" or something to that effect here.

sec. 2.1.1: I think it would be really good to include the 2-km horizontal resolution here somewhere. I know it's in Table B2 but that's pretty buried and this is a fairly fundamental feature of the modeling in both IMAU-ICE and LADDIE.

l. 91-93: "First, it was demonstrated that the choice of sliding law has little effect on the results for perturbation experiments in an idealised setup." I am concerned about this statement. It seems in direct contradiction to the findings of Cornford et al. (2020, https://doi.org/10.5194/tc-14-2283-2020), which found that the choice of basal friction led to the largest differences in model results in MISMIP+. There is an increasing consensus in the community (e.g. Joughin et al. 2019, https://doi.org/10.1029/2019GL082526) that Coulomb-limited friction laws like the Shoof parameterization that you use are both the most physically correct and the most consistent with observations. So I think it would be better to argue that you are using the "right" friction law, rather than that the friction law generally doesn't matter.

By the way, Cornford et al. (2020) would be an important paper to cite in your paper, since your setup is similar to MISMIP+ as you comment.

l. 121: I think "steady-state" needs a bit more explanation here even though you explain it later. In particular, LADDIE time-steps until it reaches steady state based on a given ice-sheet geometry and ambient ocean fields. It's important to make clear that the melt rates do evolve in time based on both ice-sheet geometry and (potentially, though not in this work) evolving ocean forcing.

l. 134-136: Could you say something about whether there is any coupling of heat fluxes?

l. 144-145 and caption of Fig. 1: "The coupling between the two models is asynchronous, meaning that draft geometry and sub-shelf melt rates are exchanged at a certain frequency: the coupling frequency." "The models are coupled asynchronously, meaning that they exchange geometry and melt rates at a fixed coupling frequency (in this case: 8 times per year) which is independent of the time step in the individual models."

There are different understandings in the community about what "asynchronous" means but this is not a definition I am familiar with. I have heard "asynchronous" used to describe running one model, then the other (which is what you do, so in that sense the term would be appropriate). But I have also heard the term used to describe running one component with an accelerated time compared with the other (e.g. running the ocean for 1 year but the ice sheet for 10 years) each time you couple. To me, "synchronous" coupling would be to run each model simultaneously for a coupling interval, then exchanging information. This is not what you do, but also involves coupling at a fixed coupling frequency. I do not think "asynchronous" coupling is typically used to refer to coupling less frequently than every model time step, which I think might be your understanding of the term.

l. 152-155: "The required runtime to reach a new quasi-steady-state depends on the flushing time…we run LADDIE for 4 days between each coupling step to ensure a near-stable meltwater layer thickness and velocity." In my experience with ISOMIP+ Ocean0 (which is qualitatively similar to the beginning of your high-melt experiments), it takes several months for a 3D ocean model to reach quasi-steady state, suggesting a flushing time on the order of months. I believe you when you say that LADDIE reaches a new quasi-steady state in 4 days for this setup, but that suggests that it doesn't require anywhere near the cavity flushing time to do so. Instead, it suggests that the previous quasi-steady state was close enough to the new quasi-steady state to require only minimal adjustment, presumably over a time far less than the cavity flushing time. This is great because it saves you a lot of computation! But I think it means the cavity flushing time isn't the relevant time scale after all and the paragraph maybe needs to be revised accordingly.

l. 158-159: "To address this discrepancy, we use nearest neighbour averaging to extrapolate the resulting sub-shelf melt field to include the grounding line cells." Here is the part of the review where I tell you, perhaps unhelpfully, that this is not how I would have done things. I'm going to do that nonetheless because maybe we can have a discussion about it sometime. First, I'm a bit skeptical of the FCMP approach (even though I'm a co-author on the Leguy paper you cited for that). It seems like a low order choice from a mathematical perspective. But if that seems to be what works best, it is hard to argue with successful results. But regarding the extrapolation approach, wouldn't an alternative (maybe a preferable one) be to just have the LADDIE domain cover all cells with centers that are floating? And maybe even cover all cells that are even partially floating. You can always compute a melt rate in LADDIE but then use the fractional area that is floating as part of computing the total melt flux in the cell that you pass to IMAU-ICE. This could be done even if you stick with the FCMP approach.

l. 164: "and an upper limit of 0.125 years": Do you stick with 1/8 of a year even for your 50,000 year initialization simulation? If so, why (since you're not coupling)? If no, maybe state that since you are talking about initialization in this paragraph.

l. 158: "To obtain a stable central grounding line position at X = 50 km…" Please state somewhere in the text that you have defined your coordinate system differently (offset by 400 km in X and 40 km in Y) compared with MISMIP+, ISOMIP+ and MISOMIP1. Otherwise, the locations you refer to will be confusing to colleagues who are familiar with the original protocols and geometry for those experiments.

l.. 175-176; "We run the model for 40 days to obtain a quasi-steady-state melt pattern that aligns with the initial ice sheet geometry and forcing data." To me, this reinforces the comment above about the flushing time. To me, it seems like the flushing time must be on the order of 40 days for this setup with LADDIE, and that the subsequent 4-day LADDIE runs to quasi-steady state do not need to run for a full flushing time.

l. 190-191: "The salinity profiles are determined such that the density profiles of the moderate-melt and high-melt scenario are identical." I don't think you say anything in the paper about the equation of state you are using. Is it the same linear EOS as in Asay-Davis et al. (2016)? I think it may be necessary to mention that since you mention density profiles here.

l. 212-213 and 216: “...over the first period of the simulation…” “On longer time scales…”: Especially because you only introduce time-series plots quite a bit later in Fig. 7, I think it would be important to give the reader a sense for what these times are – the first 200 to 400 years, and then the final 600 to 800 years would be my interpretation from Fig. 7.

Fig. 4: I found the gray pixels in panels e-h confusing at first. I would explain them in the figure caption as areas of melt-through.

l. 244 and at least a dozen other places in the text: I believe when you use the term “collapse” here and in most other places in the text, you are referring to what I would call “melt-through”. To me (and I think to the broader ice-shelf community), the term “collapse” implies larger sections of the ice shelf breaking up by fracture, and is considered distinct from melt-through (though fracture would likely occur in reality before melt-through is possible). I would ask you to change “collapse” to “melt-through” or a similar term except where you are referring to a larger break-up of the ice shelf by fracture.

l. 245-246: “When the western boundary current encounters one of these gaps in the ice shelf…” As written, I was confused by what was meant by “these gaps”, since I did not understand “localized collapse” to cause gaps in the ice shelf but rather loss of larger sections. In revising this paragraph to reword the “localized collapse” phrase above, please make sure that it is clear what “gaps” are being referred to here.

l. 246: “(assumption in LADDIE)”: I think this could use a bit more explanation here.

l. 490-491: “This leads to phenomena such as collapse of the ice shelf as a result of peak melt rates at locations with insufficient ice shelf thickness (Wearing et al., 2021).” In addition to being another place where I would replace “collapse” with “melt-through”, I would suggest rephrasing or explaining “insufficient”. I guess it seems like a tautology to me to say that melt-through happens at locations with insufficient ice-shelf thickness.

l. 503-504 and Appendix D: “...for LADDIE, as well as for the Quadratic and Plume parameterisations, calving front retreat does not impact melting closer to the grounding line.” I was quite surprised by this. My results for MISOMIP1 IceOcean2, which has a similar approach to calving, though it used a 3D ocean model, showed qualitatively different results with and without calving (see e.g. Fig. 12 in Asay-Davis et al. 2016). I would have thought that 100-m thick ice would provide some fairly non-negligible buttressing in IMAU-ICE so that, even if melt rates didn’t change appreciably, the flow of ice would be affected more substantially. I just want to make sure the calving was applied in IMAU-ICE and not just used as a masking of melt in LADDIE. Maybe IMAU-ICE just behaves very differently than BISICLES, the ice-sheet model we were using.

l. 513-514 and Appendix E: “...adjusting the deep melt rates to match those from the LA_H experiment (Appendix E)” It wasn’t clear to me either here or in Appendix E what exactly is different in this retuning. My understanding was that the original tuning was done for the initial geometry such that the melt rate below -300 m was 30 m/yr. Since this procedure was used for LADDIE, a retuning using the initial geometry wouldn’t change anything. So is the retuning for a different period of time? Or what am I missing? Could you describe the procedure and how it different from the initial tuning in more detail?

l. 520: “We want to emphasise that our coupled setup is not intended to fully replace coupled ice sheet-ocean models.” I really appreciate you including this paragraph and this sentence in particular. I think it’s an important caveat for the work.

l. 525-526: “In that context, our setup can be used to employ LADDIE’s downscaling functionality by feeding it 3D ocean data to produce physically advanced 2D melt patterns on the ice sheet model grid.” I really like this. This seems like a very powerful potential use for LADDIE and similar 2D boundary-layer models.

l. 539: Unless I missed it, ISMIP7 should be introduced as the “Ice Sheet Model Intercomparison Project for CMIP7”. I think you can probably get away with not introducing CMIP7 but I will defer to the editors on that.

Typographical, Grammatical and Formatting Suggestions:

l. 15: I think “the parameterization’s limitations” should probably be “the parameterizations’ limitations” (i.e. both plural and possessive).

l. 113: Just a pet peeve of mine but maybe don’t have back to back “) (“ but instead use a semicolon.

l. 147: “...resulting into a coupling interval of 0.125 years.” I think this phrase is obvious from the frequency of 8 times per year and can be removed.

l. 189: “tangent hyperbolic” should be “hyperbolic tangent”

l. 456-458: “Hence, we believe that despite the schematic character of our simulations, these observations of Crosson ice shelf can likely be explained by the 2D horizontal meltwater flow.” I don’t have any problem with this sentence scientifically – it’s speculative but you make that clear. But I think there’s a missing intermediate logical step here. May I suggest a rewording such as the following? “Although our simulations are schematic in character, we see qualitatively similar behavior. Hence, we believe that these observations of Crosson ice shelf can also likely be explained by the 2D horizontal meltwater flow.” Still needs work but hopefully you get the point.

l. 460-461: “...characterised by a rapid retreat followed by suppressed volume loss.” I think this phrase is redundant and can be removed.