Review for Lambert et al. :
Modeling Antarctic ice shelf basal melt patterns using the one-Layer Antarctic model for Dynamical Downscaling of Ice–ocean Exchanges (LADDIE)
Submitted to *The Cryosphere*

Reviewer: Clara Burgard
I do this review un-anonymously to clarify from which background and level of expertise some remarks might come from and because it makes the conversation during review more transparent on both sides.

Summary

The authors present the new simple model LADDIE that can be used (1) as a high-resolution parameterisation to link hydrographic properties in front of an ice shelf and melt at its base and (2) as a method to use information from coarse ocean models resolving the circulation in ice-shelf cavities to simulate high-resolution basal melt patterns. The authors present the model and its tuning (done on the Crosson-Dotson ice shelf) and then evaluate it on two ice shelves with different characteristics: Crosson-Dotson and Filchner-Ronne.

This model is an advancement compared to "classic" parameterisations in the sense that it includes 2D effects like the Coriolis force and provides the possibility to include fine-scale bathymetric characteristics in the resulting melt patterns. The topic is timely as the representation of basal melt in models remains a large source of uncertainty for future Antarctic ice-sheet projections. In particular, LADDIE enables the resolution of fine-scale channels and regions near the grounding line, where high melt occurs, and which are therefore crucial when forcing ice-sheet models. Its application therefore has the potential to improve the forcing of ice-sheet simulations.

The manuscript is pleasant to read and the procedure to set up and evaluate the model is thoroughly described. I am curious to see how the application of LADDIE will change the behaviour of ice-sheet simulations when it will be ready for a more widespread use!

Before publication, however, I think that a few points need to be addressed to clarify this manuscript and make it more robust, especially concerning the evaluation procedure. I hope it is only a matter of restructuring and reformulating and does not involve redoing a major part of the analysis. I realise there are a lot of remarks but they come from sincere interest in the study. I hope that the authors can use them constructively and am looking forward to reading a clearer revised manuscript!

GENERAL COMMENTS

General messaging

One of the main confusion sources when I finished to read the manuscript was that it was not completely clear to me where we stand with LADDIE in the end. Is this manuscript a proof of concept, showing us that it works on two distinct ice-shelf categories or is it the presentation of a model that can be used directly now by ocean

and ice-sheet modelers? I recommend that the authors clarify this more in the introduction.

In particular, Table 2 nicely summarizes the tuned parameter for the two ice shelves considered. However, it is not clear from reading the manuscript which parameters to use when applying LADDIE to other ice shelves. Is retuning needed every time? I suggest to either start the manuscript with the clear message of "This is a feasibility study and we show it is possible" or discussing in the Discussion section how to decide on the parameters to use when applying LADDIE to other ice shelves.

Tuning and evaluation setup

LADDIE is tuned on one ice shelf, the Crosson-Dotson ice shelf, and then evaluated on this same ice shelf and another one (Filchner-Ronne - FRIS). In my opinion, this evaluation approach biases the results of the evaluation. Using the ice shelf used to tune as 50% of the evaluation is not necessarily robust. There is a high-resolution observational melting pattern available from Shean et al. 2019 and input profiles (Dutrieux et al. 2014) for Pine Island Glacier. I strongly suggest that the authors consider using this ice shelf to replace Crosson-Dotson either in the tuning or evaluation.

In addition, the tuning was done using idealized profiles as input and then comparing to a number of remote sensing and in-situ observational sources". I wonder why the tuning was not done using observational input directly, instead of idealized profiles. This would increase the consistency between "what comes in" and "what comes out". Can the authors clarify their reasoning?

Evaluation setup

The authors use several types of temperature and salinity profiles/fields as input to LADDIE: (1) idealized 1D profiles in front of the ice shelf and (2) spatial fields from ocean simulations. They then compare the resulting melt patterns to (1) observational estimates from satellites, (2) simulations from MITgcm/NEMO and (3) sometimes to "reasonably expected" patterns. Also, sometimes they compare the simulations from MITgcm/NEMO to observations.

I found it very confusing and difficult to read the evaluation and to follow it objectively because the patterns are cross-compared (obs to simulation, LADDIE to obs, LADDIE to simulations, LADDIE to reasonable expectations when obs and models were uncertain) in an order that, very crudely said, sometimes felt like "we choose to compare what suits us the most". I am sure that there was a more systematic approach than that in the comparisons but it is not necessarily clear yet unfortunately. I suggest the authors restructure these comparisons to clarify why they choose one comparison over another and why their way of doing is robust.

Another aspect that puzzled me regarding the evaluation is why 1D idealized profiles are used to then compare melt patterns to observational estimates. Why not use either the measured (Crosson-Dotson) and simulated (FRIS) profiles directly to make the evaluation more consistent?

Out of curiosity, have the authors compared the output of LADDIE run at the same resolution than the 3D models (using 1D input) and the output melt of the 3D models? This might be a good first step to evaluate LADDIE's large-scale patterns as a sanity check. I do not expect this to be done if the authors do not have it ready but it would be interesting to include if it had been done.

Large VS small ice shelves

The current version of LADDIE is computationally demanding for large ice shelves and 1D input profiles lead to larger uncertainties in the pattern. I suggest stressing in the conclusion that LADDIE is easier applicable to small ice shelves, especially with the 1D profile input, like classic parameterisations.

"Validation" VS "Evaluation"

The authors decide to use the word "validation" for their model. Personally, I have a strong opinion about the wording, and "validation" is very strong. In my opinion, a model can only be "evaluated" to understand if it does the things reasonably that we expect from it. As we know, "all models are wrong, but some are useful" and therefore "validating" seems a strong word for something we know is wrong. This is a personal opinion and I let the authors judge if they agree but I suggest the authors use "evaluation" instead of "validation".

DETAILED COMMENTS

L4: I suggest adding "on long timescales" after "this resolution"

L6: Could the authors define "offshore" in the manuscript. I would suggest calling this "far-field". My understanding of "offshore" is much further from the ice shelf, i.e. sometimes even further than the continental shelf, as is given by some coarse CMIP-type models. In this manuscript, offshore seems to stand for a region in front of the ice shelf, on the continental shelf. It is a fine but important distinction for the choice of input temperature and salinity, so I would appreciate if the authors could clarify the wording.

L20: I suggest leaving out "in particular those beyond 2100". There is enough uncertainty before 2100 already.

L70: I suggest adding "near Antarctica" or "at a latitude of XX°S" after 7.5 km because the km-resolution depends on where we are on the globe.

L79: add "e.g." before Favier et al., 2019

Eq(5) and Eq(9): missing the term containing $\dot{m}*S_i$. I suspect this is probably because S_i is assumed to be 0. It might be worth mentioning this somewhere?

L139: Can the authors add a few words on what the reduced gravity stands for and why it is needed?

Eq(7): Define alpha and beta in the text.

L146: To avoid confusion, I suggest moving "below sea level" into the parenthesis after the zero.

Section 2.1.1.: \gamma_T, K_H, A_H are only introduced in a later section although they appear in Eq.1-5. I suggest that the authors already introduce them here. At least say what they stand for.

L156: Remove "." In front of citations

Eq(10): Should this be z_b?

L160: c_p and the lambdas should be defined here.

L166: In Jenkins (1991), \nu is called kinetic viscosity. Should \nu_0 be kinetic or molecular viscosity then? Same for the Prandtl number, it is defined as the "molecular" Prandtl number in Jenkins (1991).
On this, I admit that I am no expert but I just wanted to make sure these differences were no mistakes.

L166: I suggest adding "of seawater" in the end of the sentence

L173: Use \citep[e.g.][]{} for the citation

Eq(14): Define \mu. Also, I researched where this formula comes from. In my understanding, this might come from Eq. 35 from Gaspar (1988) but why is it missing the last term (the one with $E_m^{3/2}$)?

L183-185: I suggest combining these two sentences because the first sentence does not read well as it is unclear between which terms there is a balance.

L191, 203: Gurvan et al. should be Madec et al. Gurvan is the first name.

Table 1: add "coefficient" for alpha and beta descriptions, again should \nu_0 be the "molecular" viscosity?

L208: I am not an expert on this particular concept but the problem of the transient state sounds like a serious limitation. Can the authors mention this again in the discussion and discuss a bit more how to reassess the parameter choice in future studies?

L216: "we have used" => "we use"

L235: I suggest replacing "reasonable" by "plausible"

L241: I suggest replacing "stressing that" by "although we are aware that"

L243: add "and" after "(2017)"

L260: To add one more layer of evaluation and to be consistent with FRIS, would it make sense to use the Adusumilli data for Crosson-Dotson as well?

L267: "shouthernmost" => "southernmost"

Figure 2: If possible, I suggest to move the colorbar to a more intuitive place

Section 2.3.1: This section is very similar to Section 2.2.1. I suggest combining them in the place it makes most sense. Maybe make Sec. 2.3.1 shorter with the information really focused on the model setup.

L300: As mentioned in the "General Comments", I do not follow why the authors use idealized forcing fields and then compare the resulting melt to observations. In particular, for Crosson-Dotson there are measurements, as the authors say themselves in L316. Using them would make the comparison more consistent and robust in my opinion.

L304: It is not clear to me why the authors do not directly use the fields from Padman et al. 2018 and choose to stay with constant U_tide. I would think that it would give insight into the effect of tidal velocity on the resulting melt. Can this choice be clarified?

L330: Here also, why not use the simulated profiles from Holland et al. 2007 directly?

L337: Move the definition of A_h and K_h to earlier

L375: I do not see clear patterns corresponding to the 450-m isobath in Figure 3b, sorry... They are clearer in Fig3c and d, as said in L382. However, I am not sure that this should be the main feature used to evaluate LADDIE if this pattern is not that clear in observations.

L376: When talking about the thermocline, it would make sense to also point to Fig. 2.

L394-395: Here, the authors acknowledge that a feature is good because it was tuned to it. In my opinion, this highlights the ambiguity of evaluating LADDIE in a detailed manner on an ice shelf and quantity it was tuned to.

L397-404: This paragraph is very confusing and difficult to follow. The authors state that the observations are not reliable near the grounding line and turn to numerical models to evaluate LADDIE. They then also evaluate the melt in MITgcm and compare the melt between MITgcm and LADDIE. This is what I meant in "General comments" when I meant that it is difficult to follow the different levels of evaluation. This paragraph is mixing too much. Can the authors restructure and clarify?

L416: About "the western side of the topographic channel": When talking North-East-South-West in Antarctica, it often gets confusing. Can the authors add labels for degrees East/West on the maps? Or they could add labels in the figure showing the "eastern flank" and "the western flank". That might make it easier to follow where to find the western side.

Figure 6: I suggest annotating "atmosphere", "ice", "ocean" directly in at least one panel to make the figure better readable intuitively.

L425: Would it make sense to add the results from MITgcm in Fig. 6 for a direct comparison and following better this sentence?

L426-444: These two paragraphs are a description and interpretation of features only resolved in LADDIE. I find these paragraphs difficult to interpret. LADDIE is used to make conclusions on processes, while it is being evaluated at the same time. Can we use it to interpret physical processes if we are not finished with the evaluation?

L464-469: Again, what I find difficult here is the mixing between evaluation and interpretation, making it difficult to follow. I suggest to better separate them, for example: (1) evaluation, (2) what do we learn from LADDIE.

L485: There is one "freezing" too much

L491-492: Is this a suggestion or have the authors looked at this into detail? If the latter, could the authors add one sentence how they came to this conclusion?

L496: I suggest that this lack of spatial pattern comes, at least partially, from the use of 1D profiles as input, which the authors say in the following sentence. However, earlier, the authors suggested that using a 1D profile is acceptable compared to 3D fields. I think this shows that this assumption holds on small ice shelves, like Crosson-Dotson, but that, on large ice shelves, 1D profiles introduce more uncertainty than 3D fields.

L449: I do not completely agree with the conclusion that the qualitative large-scale pattern can be reproduced. It is good near the grounding line, yes, but the missing melting at the front, in the West, and at the southern tip of Berkner Island. I would suggest to acknowledge more that limitations remain.

L515: Is it the goal to retune the parameter often? Again, this comes back to: Are the authors presenting a model to be used out of a box or is this more a proof of concept?

L557-564: This paragraph is confusing. I suggest reformulating or restructuring.

L573: "more realistic forcing fields" => I again do not understand why realistic forcing fields were not used directly, especially if the authors suggest that it would have given better results.

Discussion: If the authors do not plan to change their tuning evaluation setup, I suggest a clear and robust discussion paragraph about the influence of tuning and evaluating on the same ice shelf on the conclusions of the study.

L581: I suggest replacing "idealised" with "existing"

L583: The list of citations is not very exhaustive here. Either add "e.g." in front or also include Lazeroms et al. 2018 and 2019, Favier et al. 2019, Beckmann and Goosse, 2002.

L591: "compared to basal melt parameterisations" => hmm, ok. But the authors have not compared the LADDIE results to "classic" parameterisations before in the manuscript. Maybe add one or more citation where to find the patterns. Burgard et al. 2022 would be a possibility.

L599: Maybe because LADDIE was tuned towards reproducing Crosson-Dotson?

L637-645: Yes, the conclusions sound plausible but the paragraph is confusing. I suggest reformulating. In the end of this paragraph, I am left confused again about what was the precise goal of this study. I suggest the authors re-affirm in the beginning of the paper if the goal is a quantitative and/or qualitative evaluation. Due to the different levels of comparison, I was a bit lost.

Discussion: It would also help to add a paragraph about the planned application of LADDIE in the future. Would it be used for offline use or within an ocean model directly?

L670-671: I agree. And would it not be also more consistent?

L674: Can the authors do a similar comparison for FRISP? This would show how much of an influence the 1D vs 3D field has on a larger ice shelf.

L682: "simulates" => "simulations"

L732: Using the same parameters for both might be optimistic. Can the authors show similar figures for FRIS, just to compare, or does this involve tremendous effort?