Review of: “Glacier Energy and Mass Balance (GEMB v1.0): A model of firn processes for cryosphere research”

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Summary

This paper describes a new model, GEMB, that simulates glacier/ice sheet surface energy balance and snow/firn evolution. The model has been integrated as a module in the Ice sheet and Sea level System Model. The authors describe the model’s framework and derive a new firn densification equation using previously published firn data. The authors show that GEMB’s outputs are comparable to a different firn densification model commonly used by the glaciological community. They investigating the sensitivity of the model to the vertical resolution of the model grid, and they compare modelled temperatures to observations from Summit, Greenland.

Broadly, GEMB is a good contribution to the cryospheric community. The paper strikes a good balance between model description, testing, and example applications, and as such I think it will be a good fit for GMD. However, I have numerous concerns that need to be addressed before its publication. I have divided these into General Comments and Specific/Line by Line comments.

We know how busy everyone is so we are very grateful that the reviewer accepted to review our manuscript and that they provided us with such thorough and thoughtful suggestions. We agree with nearly all of your comments and recommendations. Thank you. Please see inline responses to each comment.

General Comments

1. My biggest comment is that this manuscript, being a model-description paper, is light on the model details (Section 2) and lacks the pertinent equations that will inform the reader (and ostensibly model user) what is coded in GEMB. Instead of providing detail, the model description seems to rely on the reader already being an expert on firn and surface energy balance modeling (and being familiar with numerous papers on those subjects). I think this a barrier e.g. to early career scientists and graduate students who may want to use GEMB but will find that the description paper does not provide adequate description of what is in the model. The authors do not necessarily need to provide every equation and/or parameterization that they have taken from the literature and implemented in their model; however, I do think it would be appropriate to include the basic/fundamental equations that are driving firn evolution within each of the modules described (e.g., the basic densification equation from Herron and Langway is the baseline for numerous other models; the newer models typically just alter the prefactors c0 and c1). Further, I think it necessary in a model description paper to provide equation numbers in the references – i.e., if the authors have taken a parameterization from a paper, they should include the reference and the equation number in the referenced paper. For example: cite Calonne et al. 2011, Eq. 12 for the Calonne conductivity parameterization.

The questions that arose when I read the model description section:

- Is this an Eulerian or Lagrangian model? Or some hybrid?
- Section 2.4 talks about diffusive heat transport, but how are you dealing with advective heat transport?

- Section 2.4: what numerical scheme are you using to solve the diffusion equation? I am assuming some explicit scheme due to the need for the fine time steps?
- Does GEMB explicitly account for mass transport due to wind?
- (related to lack of equations): In section 3.4, you reference c0 and c1, but without any equations the reader does not know what c0 and c1 are.

This is a very good point. We will address this in the revised manuscript.

- 271: what is the criteria for whether meltwater can be accommodated? Your model description does not have discussion of irreducible water content; does it account for that? If so, what is the irreducible saturation? If not, why do you not include that? Similarly, you mention aquifers in the introduction; can GEMB simulate aquifers or retain any liquid water for some amount of time?

All good questions. Water can enter a model layer if the density of that layer is below pore hole close-off. The maximum amount of water retained within any given layer is the irreducible water content and is assumed constant at 0.07 by mass after Colbeck, 1974. If runoff is restricted, water can be retained within the model to form aquifers. We will make sure to address this more fully in the revision.

- 274: This description is not clear; which layer you are referring to when you say "pore space can accommodate": the pore space of the layer below, or current layer?

Here we are referring to the pore space of the below layer. We will make this clear in the revision.

- 298: you say there are seven approaches, but then you list 5. I see later that you have your own new equation, but that makes 6. It is not clear to me if GEMB includes the IMAU-FDM (Ligtenberg, 2011; Kuipers Munneke, 2015) equations (which would make 8, not seven?). Likewise, Herron and Langway has several equations (Lundin et al., 2017); which are you using?

This should be 5 not 7 approaches. This be fixed in the revision. We will also specify which Herron and Langway equation is used.

- 315: including this equation seems arbitrary because the original equation from Arthern et al. (2010) is not provided for context. What are b and m in this equation? (related to comment 4 below).

Agreed, we will include relevant equations for improved readability such that the reader does not need to locate as many original references.

2. I am curious about the choice of parameterizations for conductivity. The Sturm (1997) paper specifies that his parameterization is for density less than 600 kg m^{-3}; similarly, the Calonne et al. (2011) parameterization is for densities less than 550 kg m^{-3}. A newer paper led by Calonne (https://doi.org/10.1029/2019GL085228) suggests there is a transition in conductivity regimes (i.e., from one parameterization to another) as the snow transitions to firn. Calonne (2019) also states, "All the snow-designed formulas (Yen, Sturm, and Calonne) perform rather poorly for firn and porous ice above 550 kg/m^3". Please justify your choice to use the two snow parameterizations and potential implications, especially in locations where most of the firn

column comprises firn with densities higher than specified by the Calonne (2011) and Sturm (1997) equations.
At the time of writing GEMB, the Calonne 2021 parameterization was not available. Conductivity is indeed a point of uncertainty, due to lack of a physical model. This is why we included more than one parameterization (Sturm and Calonne) within our model framework. Future versions of GEMB will include more thermal conductivity parameterizations, including Calonne 2021, but for this paper the cost of implementing a new thermal model and reproducing all results would be too much. In addition, our test site (Summit Station, Greenland) is not well suited to identify errors in dense snow/firn thermal conductivity. This paper hopefully represents the first of many to come as GEMB evolves in sophistication.

3. Regarding your new firn densification equation (Section 2.9 and Section 3.4): I was confused reading through this. When I read section 2.9, I read it as saying that you had included the IMAUFDM densification equations (Ligtenberg et al., 2011 for Antarctica and Kuipers Munneke et al., 2015 for Greenland) in GEMB. But, section 3.4 indicates that what you are actually doing is using the model calibration procedure that Ligtenberg and Kuipers Munneke used. Please clarify the language here. Presently, it reads as, 'we did what they did' using more formal language (relying on the reader to have an expert knowledge of the IMAU group's techniques). You could add more description directly in section 2.9 of what that calibration method entails, or in 2.9 you could add something like, "In addition to the previously published firn-densification equations, we include a new equation. To derive this new equation, we follow the method employed by Ligtenberg et al. (2011) and Kuipers Munneke et al. (2015) and add a tuning factor, MO, to the Arthern et al. (2010) firn densification equation." (And then add a few more details about what that tuning procedure is, including specific equations). Reference section 3.4 if you need to, but I think that it would be appropriate to give more detail in 2.9 because this is where you include the MO equation. (It is a bit tricky because you don't have an explicit 'methods section', but this new equation and its calibration procedure is probably more a part of 'model description' rather than 'study specific model setup'. Also, please also clarify if GEMB includes the Ligtenberg and Kuipers Munneke equations.

Ligtenberg and Kuipers Munneke do not employ novel densification equations, instead they employ a novel method for calibrating existing models. We will make this clearer in our revision.

4. In Section 6, you report that the average difference is 0.8K; however, there is a lot of temporal variability in that, including a ~5K difference between model and observations in the spring of 2014. Given the fact that firn densification has a non-linear dependence on temperature (a ~5K increase in temperature nearly doubles the value of the Arrhenius factor in the densification equation), this seems like it could affect the density profile substantially. I would appreciate a bit more discussion about the source of this model-data mismatch and potential implications. How can this be improved? You list possible sources of error; what information do you need to be able to definitively state where this error is coming from?
Also: Are you correcting for the fact that the thermistors are continuously getting deeper due to new snowfall? I.e., the first thermistor was much deeper than 20 after several months.

We will provide more discussion on the implications of the mismatch (primary modeled temperature vs in situ observations).. we will also discuss the limitations of our ability to make more definitive conclusions (we don't have above-snow aws data). And yes, data have been corrected for change in thermistor depth… we will also make sure to mention this in our revision.

5. This is a model within the ISSM framework, but there is very little description of how GEMB fits into that framework. Does GEMB provide a boundary condition for the ice sheet model? Is it fully coupled to the ice sheet model, or any other models within ISSM? Or is a standalone offline model? A bit more text contextualizing GEMB within the ISSM framework would be helpful.
6. The paper needs general editing throughout: there are numerous typos, run on sentences, etc. (especially in the introduction). I have highlighted several of these (but not all) in the specific comments. In some cases, these (run on sentences) obscured the meaning (or what I inferred to be the meaning) of the text, which made it difficult to assess the quality of the science. Please give a thorough structural edit to improve the writing and thereby clarity.

Specific Comments
- I would not expect you to change the name of you model on account of this, but there is also a glacier model called PyGEM (https://github.com/drounce/PyGEM); it may be prudent given the similar names to mention that they are not related?

We hadn't thought about that. I'm not sure if mentioning the other model would lead to clarity or confusion. We will thing about that in our revision.

31: Arthern misspelled.
Fixed

45: this sentence has subject/verb agreement issues. (e.g., 'results in increased absorption, modify thermal')
Fixed

48: 'things' is a vague word/colloquial phrasing – please be specific. Why is this complicating?
Fixed. It is complicating because it is very challenging to understand and simulate all of the feedbacks/

58: SNOWPACK is all capitalized
Fixed

64: I think that you mean that it is needed to predict delta age, which is the ice-age gas-age difference.
Good point, fixed.

Table 1: typo: Sturm et al. vs Strum
Fixed

Section 2.9 – perhaps change section header to 'Compaction', as densification also occurs due to

meltwater refreezing. (And, be consistent throughout the paper to use "compaction" when you are discussing densification due to strain.)
Another good point, section header changed.

355: The documentation for SUMup specifies that each core from the database that is used should be cited (citations are provided in SUMup), rather than just citing the database. I have seen this done by adding an appendix or supplement with those cores referenced (see e.g. Brils et al., 2022).
Yikes! OK, we'll look at adding this.

- Please ensure that units are specified on all parameters and quantities. Places I noticed them missing: C in your MO equation; specific surface area *S* " ; there will be more assuming you follow my suggestion to add pertinent equations to section 2.
We will make sure this is done in the revised manuscript. Thanks for catching that.

- Please clarify (and use consistently) the language with 'node' and 'grid' – initially I thought that node referred to the points in the x-y (ice-sheet surface) plane, and grid referred to the vertical component of the model (ie. layers of snow) – but section 3.1 refers to grid in an x-y manner I think? Perhaps add a sentence early on explicitly stating how you are using each term. (Admittedly this gets tricky in the firn model world.)
This is a good point and one that we'll make sure to address in the revision.

Section 4: How do you interpolate the RACMO fields from the RACMO grid to your custom grid?
Bilinear interpolation, we will include this in the revision.

355: Shallow and deep cores: wording of 'reaching' is a bit confusing. Perhaps say, 'reach beyond the 550 horizon' and 'reach beyond the 830 horizon' to be a bit more explicit.
Done

367: Provide more info about this 'additional withheld subset'. E.g., are you just randomly selecting a subset to withhold, or are you doing so in a way to withhold data from numerous climate zones, or something else?
We will state this more explicitly in the revision

371: Be more specific with which SMB components you are comparing – I think (or am guessing at least) that you are using the RACMO rainfall and snowfall. I think this leaves any components that are determined by your surface energy balance module (e.g. sublimation, evaporation, deposition, meltwater production) – is that correct? And then also runoff, which is a function of your subsurface scheme?
RACMO has its own simple snow model so we are comparing all surface mass change within model (accumulation, runoff, sublimation/evaporation/deposition) to those estimated by GEMB. We will make this more explicit in our revision.

410: I am not convinced that IMAU-FDM (especially with the Ligtenberg and Kuipers Munneke densification equations) should be called the state of the art in firn modeling. The IMAU group has recently published updated versions of IMAU-FDM for Greenland (Brils et al., 2022) and Antarctica (Veldhuijsen et al., 2022); NASA Goddard has developed a model (Medley et al., 2022); and the physically based SNOWPACK model has been applied to both ice sheets (Keenan et al., 2021; Dunmire et al., 2021). The upshot is that the version you are comparing to has been widely used is therefore worthy for comparison but may not represent the latest and greatest.

Good point.. the landscape has changed a lot since we initially wrote (even submitted) our paper. We will make sure to mention these new studies in the revision.

404: Please specify what value you are using for ice density in calculating FAC. Is it the same as what the IMAU group uses? In the case that layers are added to the bottom of your domain (Section 2.8), are you potentially adding FAC?

418 and elsewhere: change instances of 'FDM' to IMAU-FDM (in the text and in the figures), as the NASA Goddard model also uses FDM in its name (GSFC-FDM).
Will do

431: do you mean "positive between 1979 and 2005"?
Oops, thanks for catching that.

434-447: This paragraph has several run-on sentences and grammatical errors that make it difficult to follow.
We will rewrite this in the revision

444: Why do you not show the detrended comparison? You just explained that the trends (or, at least the difference between them) are largely artifacts of model spin up, so it seems that the detrended comparison is actually the prudent metric to compare model outputs.
I think this could be argued either way. Ideally, we want modeled FAC that does not need to be detrended. This one I think we'll leave as is as we feel it's valuable to show the non-detrended comparison.

449: This (Antarctic) paragraph should also mention the spin up periods – were they the same for IMAU-FDM and GEMB in this case?
We will do this in the revision. we found that a longer spin-up (relative to IMAU) was needed to achieve model convergence (no residual trends in FAC).

-You identify that the spin up affects the output: why did you choose the spin-up dates that you did (vis a vis other firn model studies)?
This is not nearly as scientific as we would like due to the limited period of reanalysis data that we have to spin-up the mode. We will make this clearer in the revision.

- Since you are using the same calibration method as is done for IMAU-FDM, is any difference between the GEMB and IMAU-FDM outputs due entirely to differences in the SEB module? I would expect that if you used melt production and surface temperature from RACMO, your

calibration procedure would net MO factors quite similar to the IMAU values (from the new IMAU equations, at least, that also use a lot of the SUMup cores). Can you draw any conclusions about firn model uncertainty in general from this result?
I'm not sure there is much we can conclude. Our model is sufficiently different from IMAU-FDM, we use more cores for calibration than used by IMAU and we calculate the SEB differently. I think you'd need to run the models with the same forcings to really tease apart the differences.

Figure 12: labels/units are missing on colorbars. Left panel colorbar inside of panel a makes it difficult to find.
We will fix this in the revision.

Thanks again for such a thorough review… greatly appreciated.

References:
Brils, M., Kuipers Munneke, P., van de Berg, W. J., and van den Broeke, M.: Improved representation of the contemporary Greenland ice sheet firn layer by IMAU-FDM v1.2G, Geosci.
Model Dev., 15, 7121–7138, https://doi.org/10.5194/gmd-15-7121-2022, 2022.
Dunmire, D., Banwell, A. F., Wever, N., Lenaerts, J. T. M., and Datta, R. T.: Contrasting regional
variability of buried meltwater extent over 2 years across the Greenland Ice Sheet, The Cryosphere,
15, 2983–3005, https://doi.org/10.5194/tc-15-2983-2021, 2021.
Keenan, E., Wever, N., Dattler, M., Lenaerts, J. T. M., Medley, B., Kuipers Munneke, P., and Reijmer, C.: Physics-based SNOWPACK model improves representation of near-surface Antarctic snow and firn density, The Cryosphere, 15, 1065–1085, https://doi.org/10.5194/tc-15-1065-2021, 2021.
Medley, B., Neumann, T. A., Zwally, H. J., Smith, B. E., and Stevens, C. M.: Simulations of firn processes over the Greenland and Antarctic ice sheets: 1980–2021, The Cryosphere, 16, 3971–4011, https://doi.org/10.5194/tc-16-3971-2022, 2022.
Veldhuijsen, S. B. M., van de Berg, W. J., Brils, M., Kuipers Munneke, P., and van den Broeke, M. R.: Characteristics of the contemporary Antarctic firn layer simulated with IMAU-FDM v1.2A
(1979–2020), The Cryosphere Discuss. [preprint], https://doi.org/10.5194/tc-2022-118, in review,
2022.