Response to Reviewer 1

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

By Alex S. Gardner, Nicole-Jeanne Schlegel, Eric Larour

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?

Please see heavily revised manuscript that now addresses these concerns.

- 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. These have all been addressed in the updated 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 of the pore volume after Colbeck, 1974. If runoff is restricted, water can be retained within the model to form aquifers. Both these questions are now addressed in the main text.

- 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. This sentence has been modified for clarity.

"If the layer into which the meltwater flows is permeable and the capillary effects can retain the incoming meltwater, the water is held within that layer and is combined with any pre-existing liquid water. "

- 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 will 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 have significantly modified the manuscript which now includes the most relevant equations.

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 2019 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 2019, 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. To address this we have included the following sentence at the end of Section 9:

"Calonne et al. (2019) show that the quadric relations between effective thermal conductivity and density, that are often used for snow (e.g. Equation 4 & 5), do not perform well for firn and porous ice. Instead, they propose an alternative empirical relation to more accurately model effective conductivity for the full range of densities from 0 to 917 kg m^{-3} (see Equation 5 in Calonne et al., 2019). We plan to include empirical relations proposed by Calonne et al. (2019) in a future version of GEMB. "

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.

Sections on compaction have been heavily revised to be more explicit, with the addition of many equations. We believe this addresses the reviewer's concerns.

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 believe the reviewer is referring to Section 9, not 6.

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

"While differences in mean temperature are only ~0.8 K, seasonal differences can be as large as 5 K (Figure 13). A 5 K increase in snow/firn temperature increases the compaction rate by a factor of 1.6-1.8. Since compaction rates are strongly dependent on temperature, seasonal biases in temperate have a large impact on rates of firn compaction, even if annual biases are small. Therefore future work should prioritize improving near-surface atmospheric temperatures over glacier surface and thermal diffusion within snow and ice.

Calonne et al. (2019) show that the quadric relations between effective thermal conductivity and density, that are often used for snow (e.g. Equation 4 & 5), do not perform well for firn and porous ice. Instead, they propose an alternative empirical relation to more accurately model effective conductivity for the full range of densities from 0 to 917 kg m^{-3} (see Equation 5 in Calonne et al., 2019). We plan to include empirical relations proposed by Calonne et al. (2019) in a future version of GEMB. "

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.

We have added the following paragraph at the end of Section 1:
"GEMB is currently run independent of the ice flow model. The goal is to eventually couple the ice flow model with GEMB. In this situation the flow model would provide the 3-D surface displacement vectors to GEMB to inform the migration of the column nodes relative to the

climate forcing and to allow for solving of longitudinal stretching of the firn. Currently the major hurdle for coupling the models is the implementation of downscaling routines for the climate forcing."

In addition we also now state that:
"Currently GEMB is a stand-along module that is responsible for the calculation of ice sheet and glacier surface-atmospheric energy and mass exchange and firn state within ISSM. GEMB provides the ice sheet flow model with near-surface ice temperature and mass flux boundary conditions."

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.
Please see heavily revised text.

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. Since it is not clear to us that mentioning the other model would lead to clarity, or add more confusion, we have decided not to include this suggestion.

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?
Changed to "The introduction of melting decreases the number of ice-air boundaries which reduces scattering and enhances absorption of shortwave radiation."

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 and densification replaced with compaction in most places

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.
Please see major changes to manuscript equations.

- 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.)
Nearly all occurrences of "grid" have been changed to "nodes" for constancy

Section 4: How do you interpolate the RACMO fields from the RACMO grid to your custom grid?
We have added the sentence: "RACMO values are bilinear interpolated to GEMB nodes."

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 have included the following sentence for transparency:

"Like GEMB, IMAU-FDM is an uncoupled firn model (i.e. not coupled with the atmospheric model). IMAU-FDM represents the most widely used firn model results. We note that there has been a recent and rapid increase the number of ice sheet wide firn modeling studies (c.f. Brils et al., 2022; Dunmire et al., 2021; Keenan et al., 2021; Medley et al., 2022; Veldhuijsen et al., 2022). An in-depth comparison between all modeled results would be highly valuable but is not done here."

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?
No FAC is added as the bottom domain

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).
Done

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

434-447: This paragraph has several run-on sentences and grammatical errors that make it difficult to follow.
This paragraph has been heavily edited.

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?
This section has been heavily modified for improved clarity.

-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 section has been heavily modified for improved clarity.

- 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.

Response to Reviewer 2

Glacier Energy and Mass Balance (GEMB v1.0): A model of firn processes for cryosphere research
Alex S. Gardner, Nicole-Jeanne Schlegel, Eric Larour

General Comments
The authors present a new 1 dimensional snow/firn model to be applied to glacier studies. The model is of intermediate complexity relative to state-of-the-art existing models, but retains high computational efficiency suitable for long-spinup periods and sensitivity studies. It is therefore well-suited for these applications as well as inclusion in ice flow models such as ISSM.

The paper is quite well written and generally very clear. I think it can be published with relatively minor corrections discussed below.

Thank you kindly to the reviewer for taking the time to provide such a constructive and thorough review of our manuscript. We greatly appreciate all of their efforts. We agree with nearly all of the reviewer's comments and suggestions and will address them in a revised manuscript. Please see detailed response to each comment below.

Some general comments are:
(1) The model is evaluated over ice sheets, but temperate glaciers are not discussed. Perhaps the authors can comment on applicability to temperate glaciers.
All of the physics within GEMB are applicable to temperate glaciers but GEMB does not include parameterizations for debris cover and climate forcing in mountainous regions is much challenging to simulate. We will mention this in the revision.

(2) The relative advantages and disadvantages of GEMB relative to existing models could be more explicitly stated in the abstract and introduction (e.g. its suitability for running within ISSM or in conjunction with other glacier/ice sheet models).
Good point… personally I think the biggest strength is having multiple models and groups simulating firn processes. GEMB was also designed with uncertainty quantification in mind with many parameterization options to explore the uncertainty envelope. GEMB also facilitates the assimilation of altimetry into the ISSM ice flow model.

(3) The computational efficiency is mentioned briefly but not quantified in the manuscript. It would be interesting if some metrics could be provided regarding this.
While we can't compare to other models, we could mention how long it take to spinup a single column of a fixed number of layers. After thinking about this more we were unable to come up

with an informative benchmark as run time is highly dependent on model setup and we have not runtimes to benchmark against. As a rule, C++ is 10x to 100x more perfomant than native Python.

Specific Comments

1. Line 31: Are “spatial gradients” referring to vertical gradients? Please clarify.
Good catch, now changed to “vertical”

2. Line 47: Can the authors specify how these changes affect the net energy balance? Are these all positive feedbacks?

These feedbacks are mostly positive, with the exception that impurities can sometimes decrease thermal gradients that will slow further grain-growth. But in general feedbacks are positive leading to increased energy absorption. This paragraph has been modified slightly for improved clarity.

3. Line 75: Discussion of perched ice layers could be added here to clarify that it is not necessary to completely fill up pore space to enhance runoff (

e.g. Culberg et al., 2021; Miller et al., 2022; Macferrin et al., 2019) Culberg, R., Schroeder, D. M., and Chu, W.: Extreme melt season ice layers reduce firn permeability across Greenland, Nature communications, 12, 1, 1-9, 2021.
Miller, J. Z., Culberg, R., Long, D. G., Shuman, C. A., Schroeder, D. M., and Brodzik, M. J.: An empirical algorithm to map perennial firn aquifers and ice slabs within the Greenland Ice Sheet using satellite L-band microwave radiometry, The Cryosphere, 16, 103–125, https://doi.org/10.5194/tc-16-103-2022, 2022.
MacFerrin, M., Machguth, H., van As, D., Charalampidis, C., Stevens, C. M., Heilig, A., Vandecrux B., et al.: Rapid expansion of Greenland’s low-permeability ice slabs, Nature 573, 7774, 403-407, 2019

Good suggestion. We’ve added the following sentence:

“Further, impermeable ice layers formed at depth within the firn can support perched aquifers or enhance metlwater runoff (Culberg et al., 2021; Miller et al., 2022; Macferrin et al., 2019).”

4. Line 95: Spell out DAKOTA.
Dakota does not appear to be an acronym, we have modified the capitalization and added a link to their website (https://dakota.sandia.gov)

5. Line 105: Although it is touched on here, it would be helpful to have a description of the benefits and drawbacks of GEMB relative to other similar models, e.g. why it is particularly well suited for ice sheet model simulations in contrast with other 1D models.
See response to general comment.

6. Line 128: This “near-surface” region and why it is necessary have not been explained yet. Perhaps include a sentence prior to this explaining the nearsurface portion of the column.
Good point…we have added the following:

“Firn properties and energy fluxes have more vertical heterogeneity nearer the surface-atmosphere boundary than deeper within the firn column. Because of this the near-surface firn must be modeled using finer model layers than those deeper within the firn column, where gradients in energy fluxes and ice properties are more diffuse. To accommodate this, finer model layers are specified for the near-surface and increasing layer thickness with depth for layers below the near-surface.”

7. Line 130: Can the authors explain in a bit more detail how the scaling by depth works and why it is implemented this way?
The scaling is done make the thermal diffusion calculations more efficient and to reduce the numbers of layers carried by the model. We have added the following:
“Layer thickness is increased with depth to minimize the overall number of layers needed to simulate the firn column, improving computational efficiency without sacrificing accuracy.”

8. Line 173: Does this include an integrated snow/ice albedo as a function of snow depth for shallow snow?
That’s a good idea but our model does not account for that.

9. Line 197: How is the thermal time step determined?
We identify the minimum time step that’s needed to achieve a diffusion number > ½ for all layers. We have added the following sentence;
“See Patankar (1980) for a good overview of numerical heat transfer and stability criterion for finite difference methods.”

10. Line 150: Assign equation numbers here and throughout.
Done

11. Line 211: Should this be “gray body” rather than “black body”?
Good points… a dark, dark, almost black body ;-). Now updated.

12. Lines 227-231: It would be helpful to reiterate which of these parameters are model inputs here.
This sentence had been modified to make things clearer.

13. Lines 231-232: This sentence about longwave emissivity seems out of place in the turbulent heat flux section. Should it be mentioned in the previous section instead?
It should and it already is… this sentence is redundant we have removed it… thanks for spotting.

14. Line 244: Specify that “initial” refers to fresh snow here.
Yes, good point. We now state that :

"By default, fresh snow dendricity is set to 1 and fresh snow sphericity is set to 0.5."

15. Lines 248-258: Does this mean that the rain is assumed to refreeze instantaneously unless the layer reaches the melting point? Please clarify.
Yes, when the rain reaches the surface it is treated exactly the same as meltwater… we have added the following sentence:

"The approach of adding rain as ice plus latent heat of refreeze, and later converting back to liquid water if the top firn layer does not possess enough cold content to freeze the rain in place, is simply for computational convenience."

16. Line 269-271: I don't understand how there could be excess thermal energy that does not contribute to melting. Would this occur if the layer completely melts away? Please clarify.
Yes, excess thermal energy occurs when all ice in that layer is melted… we have modified the sentence to read:
"Beginning with the surface layer, the module determines if the local thermal energy is capable of melting any of the ice within the layer, and if it is, this portion of the layer is melted and any excess thermal energy (i.e. all ice in layer is melted) or melt water that cannot be accommodated in the pore space locally is redistributed to the layer below. "

17. Lines 293-294: However, addition or removal of mass from the bottom of the column would not be included in surface mass balance estimates, correct?
Yes, that is correct. We have modified the sentence to read:
"All additions and subtractions of mass are cataloged and accounted for in final mass change estimates (i.e. not included in estimates of surface mass balance)."

18. Lines 299-300: Some of these models use the mean accumulation rate as a parameter. Can this be specified by the user or is it determined during the spinup period?
Currently GEMB sets the mean accumulation used for the spinup period… but it would not be difficult to modify GEMB to take user specified values.

19. Line 310: Can the authors briefly explain how the c0 and c1 rate parameters are applied, or include the equations where they are used?
Please see heavily revised text and equations.

20. Line 316: Please explain the b and m parameters.
Please see heavily revised text and equations.

21. Line 318: Is there a module within GEMB that allows the user to include the observational data, or was this done independently?
Calibration is done independently; The model is run without calibration, the parameters are fit and the model is re-run. We have included the following text for clarity:
"Offset ($b_{550/830}$) and scale ($m_{550/830}$) coefficients are estimated by spinning-up uncalibrated

GEMB firn profiles at node locations that are closest to the location of firn cores. Offset and scale coeffects are then determined by minimizing the least-squares fit between modeled and observed densities. These calibration coeffects are then applied in the ice sheet wide simulations. "

22. Line 328: Can the authors explain why this initial smoothing was performed?
The smoothing is preformed to match the resolution of the ERA 5 climate reanalysis data, which is the forcing of choice when not comparing to IAMU-FDM. This statement has now been added to the manuscript.

23. Line 330: How is the bare ice extent initialized at the start of a simulation. I suppose this may not be important given the long spinup period.
There is no need to initialize the bare ice extent as the model builds up a glacier over time… if the glacier is located in an ablation are then the column is supplied with an infinite source of glacier from the bottom layer. (see section 2.8)

24. Line 349: Again I am curious as to how the model was initialized, though I suppose this doesn't have much effect given the long spinup period.
Ya, this doesn't matter. The spinup is so long that any effects of initialization are long-gone.

25. Lines 359-360: The reference to Figure 4 is misleading here. The description here seems to indicate that Figure 4 is showing MO550 and MO830 as a function of C. I think the equation on Line 315 should be referenced here instead. It can also be mentioned, perhaps at the end of this section, that Figure 4 shows modeled vs. observed 550 kg m-3 and 830 kg m-3 depths.
Good point. This paragraph has been updated.

26. Line 369: It would be helpful to have a brief description of the RACMO model simulations used here to have an idea of the inter-model differences.
Not a bad suggestion but for this I think we'll refer our readers to the original texts as reviewing all relevant details of the Greenland and Antarctic RACMO simulations would be a bit much.

27. Line 375: There might also be differences in subsurface components that contribute to differences, e.g. differences in snow density that contribute to differences in refreezing and thermal conductivity.
Very true. I think the number of processes and interactions is numerous which is why we list the likely top three sources of different followed by etcetera.

28. Line 380: Figure 6 is not mentioned until the following section. I suppose combining Figures 5 and 6 to match Figure 7 would make it difficult to see the details for the Greenland ice sheet inter=comparison. Perhaps Figure 6 can be mentioned briefly here, then described in detail later.
Yup, when we started out Figure 5 and 6 were one figure but because of the elongated nature of Greenland it didn't work all that well so we split it into two figures. We have now cited the figures in order, making sure that Figure 6 is cited before Figure 7.

29. Line 390: Any idea why fresh snow melt would be underestimated?

Good question. We suspect it's related to the surface albedo but we lack the necessary outputs from RACMO to diagnose further.

30. Lines 393-394: Is this sentence regarding albedo in reference to Antarctica?
Yes it is… if you look at Figure 7 you'll see that this really only applies to ice shelves in the Antarctic peninsula.

It would be interesting to see a comparison between GEMB and RACMO albedo for both Antarctica and Greenland either in the main text or as a supplemental figure.
Unfortunately, we do not have this data.

31. Line 402: Is this higher retention of meltwater due to differences in estimated porosity, or due to differences in thermodynamic properties?
I think in this case we're just seeing more melt at higher elevations in RACMO. At these elevations there is plenty of cold porous firn for the meltwater to freeze within.

32. Line 403: Clarify that this is for GEMB.
We have changed this sentence to read:
"In the Antarctic lower rates of sublimation and higher rates of meltwater runoff in GEMB result in a slightly more positive surface mass balance trend relative to those simulated by RACMO."

33. Lines 411-412: Clarify that this is relative to IMAU-FDM.
Done.

34. Lines 417-418: I suggest using parentheses rather than slashes to avoid confusion with division here.
We researched this a bit and could find no standard convention. Given that the '/' appears as a subscript we do not think there will be any confusion with division so we have kept this unchanged.

Also, what explains why these scaling coefficients end up different between the two models?
Differences are primarily due to differences in firn cores (IMAU had access to less cores when they did their calibration) and differences in the models themselves. We will mention this in the revision.

35. Line 427: Change "Figure 8" to "Figure 8c" and "Figure 9" to "Figure 9c".
Done.

36. Lines 432-442: I'm a bit unclear on how the spinup affects the trends here. Are the authors saying due to the spinup GEMB is in a steady-state condition over the 1979-2005 period, while IMAU FDM is not? Also, could these differences also contribute to the spatial differences shown in Fig. 6?
If the forcing is similar to the spinup then there will be little change in FAC… therefore how closely the forward model climatology matches the spin up will determine if there is a change or trend in FAC. The spinup could affect the patterns seen in Fig 6 but we attribute most of the

observed pattern to higher rates of melt water production in IMAU-FDM in the percolation zone which leads to denser firn (lower FAC) at these altitudes. This is described in Sec 4.2 so we'll leave as is. This paragraph has been updated to make this clearer.

37. Line 452: Add "change" after "larger rates of FAC".
Changed.

38. Lines 453-454: Any idea why this difference occurs over Antarctica?
We really have no clue. Total GEMB and IMAU-FDM FAC are nearly identical until 2008 when they start to diverge (Fig 9c). We're not sure if something changed with an updated run of IMAU-FDM or if it just happens to be a difference in models that manifest during the post 2008 period.

39. Line 484: Figure 11 shows results for dztop but apparently not dzmin. Can the authors provide some discussion of those results?
For these experiments dmin = ½ dztop. This sentence has been modified as follows:
"To demonstrate model sensitivity to vertical resolution we run GEMB v1.0 for Greenland using the same model setup (Table 1) but for four different dz_{top} [2 cm, 5 cm, 10 cm, 20 cm] and dz_{min} [1 cm, 2.5 cm, 5 cm, 10 cm] pairs (i.e., four model runs each having dz_{min} = ½ dz_{top})."

40. Lines 524-525: Add "(GEMB)" after "Glacier Energy and Mass Balance".
done

41. Line 564: Remove "Glacier Energy and Mass Balance (GEMB) model" and replace with "GEMB" as this is already defined in this section.
done

42. Lines 858-859: Revise to "dztop is the maximum near-surface layer thickness".
done

43. Figure 5: In previous studies, red tends to be used for higher melt, while blue is for lower melt. It might be more intuitive to flip the red-blue color bars for the first two rows, if the authors agree.
I see what you're saying but we're a bit hesitant to invert the color bar for the same figure as row three would have red = negative and blue = positive, and row one and two would have the opposite if we adopted this change.

44. Figure 7: The same could be done as for figure 5 for melt, runoff, and evaporation if the authors agree.
This makes sense but same comment as above.

45. Figure 11: Again both color bars could be flipped if the authors agree.
I think we'll stick with red = negative, blue = positive throughout for consistency.

46. Figure 12: Note that units of temperature and temperature differences are in

K somewhere on the figure or caption.
Will do, good catch. Thanks.

Technical Corrections
1. Line 46: Change “modify” to “modified”, “enhance” to “enhanced”, “increase” to “increased” and “feedback” to “feed back”.
All done with the exception of feedbacks which in this case we believe should be one word.

2. Line 73: Change “persists” to “persist”
done

3. Line 297: Change “increases” to “increase”.
done

4. Line 337: Change “pacing” to “spacing”.
done

5. Line 355: Remove italics from “Medley et al., 2020”.
Done

6. Line 385: Change “concentrated to” to “concentrated in”
Done

7. Line 414: Replace comma with semicolon after “higher FAC”.
Done

8. Line 431: Add “between” before “1975 and 2005”
Done

9. Line 441: Change “perform” to “performs”. Start a new sentence after “1978”.
Done

10. Line 526: Replace “allows the model” with “allows it”.
Done

11. Lines 566-568: This is a bit of a run-on sentence. Please revise.
Done