Glacier Energy and Mass Balance (GEMB v1.0): A model of firn processes for cryosphere research
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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. We will make sure to add a statement to this effect in the revision.

(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. We'll look to see if there is anything intelligent we can add here.

Specific Comments

1. Line 31: Are “spatial gradients” referring to vertical gradients? Please clarify.
Good catch, we mean vertical… we will change this.

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. We will modify this sentence in the revision.

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 point, we will add this in the revision.

4. Line 95: Spell out DAKOTA.
Well do.

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’ll add this in the revision.

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 will clarify this in the revision.

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 will include this in the manuscript.

10. Line 150: Assign equation numbers here and throughout.
Will do… this was also pointed out by Reviewer #1.

11. Line 211: Should this be "gray body" rather than "black body"?
Good points… a dark, dark, almost black body ;-). We will update.

12. Lines 227-231: It would be helpful to reiterate which of these parameters are model inputs here.
We will make this clearer in the revision.

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 and will be remove… thanks for spotting.

14. Line 244: Specify that "initial" refers to fresh snow here.
Yes, good point. We will change this.

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 will clarify this in the revision.

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 will clarify this in the revision.

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 will make a note of that in the revision.

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?
This was also requested by R1. The revised manuscript will include the relevant equations.

20. Line 316: Please explain the b and m parameters.
See response to 19.

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 will clarify this in the revision.

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. We will mention this in the revision.

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. We'll make sure to address this in the revision.

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 will take your suggestion and mention it briefly to keep the figures together.

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 porus firn for the meltwater to freeze within.

32. Line 403: Clarify that this is for GEMB.
Will do.

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

34. Lines 417-418: I suggest using parentheses rather than slashes to avoid confusion with division here. Also, what explains why these scaling coefficients end up different between the two models?
We will update with parentheses in the in the revision. 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”.
Will do.

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.

37. Line 452: Add “change” after “larger rates of FAC”.
Will do.

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. We will make it clearer that there were only 4 model runs conducted with pairs of dmin and dztop.

40. Lines 524-525: Add “(GEMB)” after “Glacier Energy and Mass Balance”.
Will do.

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

42. Lines 858-859: Revise to “dztop is the maximum near-surface layer thickness”.
Will do.

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".
Will do.

2. Line 73: Change "persists" to "persist"
Will do

3. Line 297: Change "increases" to "increase".
Will do

4. Line 337: Change "pacing" to "spacing".
Will do

5. Line 355: Remove italics from "Medley et al., 2020".
Will do

6. Line 385: Change "concentrated to" to "concentrated in"
Will do

7. Line 414: Replace comma with semicolon after "higher FAC".
Will do

8. Line 431: Add "between" before "1975 and 2005"
Will do

9. Line 441: Change "perform" to "performs". Start a new sentence after "1978".
Will do

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

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