REFEREE 3: ERIN PETTIT

This work presents and overview of how Greenland's firn porosity might change under slight warming or cooling scenarios.

The modeling and results are insightful and interesting – I really just want more! But in a shorter, more concise structure, and figures that really draw the reader to the main points. I agree with several of the points of the other reviewer – showing the model reaching equilibrium would be great. And testing another magnitude of change. But I realize that is effort. Also, as a physicist, I really want to see the results tied back to the physics of firn, since this is what would allow us to take these results and transfer them to other places…

We thank the referee for their thoughtful feedback and insightful comments that have helped us improve the manuscript. In particular, we appreciate the suggestions to make the writing more focused and specific. We have addressed the comments below and have noted the changes we have made to the manuscript text and figures. Please find below our responses to comments in blue text, and specific changes made to the text in ***blue, bold, italic font.*** Line numbers refer to the original manuscript.

General comments:

- the abstract suggests that the work will focus on the physics of the nonlinear relationships, then discuss this in the context of Greenland, but the paper jumps immediately into the large scale integrated Greenland response without showing much of the physical reasoning. For example, how much does a steady trend in air temperature affect the firn compared to an individual warm event (one hot summer, for example)?

Regarding the abstract, we noted our modifications in specific comments below (see lines 88, 134 of this document). We have also made the abstract more specified and precise by modifying lines 9-10 to say:

To better understand the nature and timescales of firn's response to air temperature change on the Greenland Ice Sheet, we use a physics-based model to assess the effects of atmospheric warming and cooling on firn air content in idealized experiments.

Regarding the questions and suggestions for new experiments (e.g., trend, extreme events) posed by the reviewer, we agree that they are very interesting and likely could be answered with a tool like SNOWPACK. However, they are beyond the scope of this work. For this work - we take a simple and reasonable approach (1 °C temperature perturbations) that allows us to place our results in the context of previous research (Kuipers Munneke et al., 2015) but add new exploration of idealized temperature–firn relationships over the entire Greenland Ice Sheet. Additionally, we aim to isolate the system's response to just one variable. Our experimental design adds new insights on timescales and magnitudes of response that make physical sense. We agree more work in this idealized framework could provide additional insights, and fit well within the research efforts of the firn-climate community.

- at the end of the abstract/introduction, I was still trying to figure out where this paper was headed. It was unclear to me what the goals of the paper were and what methods would be used or what the findings were (other than big general statements). It would be nice to be able to extract from the abstract and introduction what is really novel about what this paper is offering and how it is getting there. I think this could be helped by just making each sentence, each paragraph a bit more specific (not longer, just more specific).

We appreciate this suggestion and we believe the changes we have made in response to other comments have resolved this issue (see lines 27-32 and 134 of this document)

- The methods section is a bit limiting from my perspective. Perhaps this is because I like to know what physics is going on! More information is provided about the surface boundary condition than to the internal physics of the firn model - a summary of the key assumptions made by the model would be really helpful. The model also uses the same parameters as a previous paper (but I don't have time to go read that paper) and then is compared to the results from that paper to suggest that the model is working well. I'm a bit confused as it seems to me that the statement implying validation (Line 133) of the model is just comparing the model to itself.

In this work, we are building on previous work using this exact model set-up. Thus, an in-depth model description including assumptions and physics included is not needed as this exact model that has been used and described (Bartelt and Lehning, 2002; Lehning et al., 2002a, b), including for the Greenland Ice Sheet (e.g., Thompson-Munson et al., 2023). As a result, we have aimed to provide enough model description for readers in Section 2.1. In this section, we provide a list of studies that have used SNOWPACK (lines 67-70), information about how the model calculates density for new layers within a Lagrangian framework, and how melt is calculated with a surface energy balance scheme. We also provide key parameters here that could be applied to other firn models (e.g., the bucket scheme, the spin-up period) in Section 2.3. Please see lines 161-167 of this document for further comments.

In line 133, we agree with the reviewer that the original text was confusing. Our intention was to show that our modeling framework and choice of 1991 made sense by falling within the range of previous simulations using a more realistic climate. As such, we have changed this sentence to the following:

We ensure that the idealized atmospheric forcing can produce firn air content values within the range of those previously simulated, and we find that the mean of 19.9±8.3 m and spatially integrated firn air content of 35621 km^3 are consistent with results from the SNOWPACK study that uses the same parameters over Greenland (Thompson-Munson et al., 2023).

- Also related to methods - the goal of the paper implied trying to figure out how firn responds. But given that the results are only provided in the context of large scale changes on Greenland, I don't know how to take the results and apply them to a mountain glacier or to Antarctica. I would suggest an approach of sweeping through a range of parameters and looking at 1D results for each, to show the changes in the snowpack that are possible. Or at a minimum, spending much more time on results such as Fig 7/8 - where a few modeled points with different characteristics are compared.

This paper and the results in it are Greenland-focused; we specifically want to know how *Greenland* firn responds. As a result, taking these results and applying them to a mountain glacier or Antarctica may not be appropriate and also is beyond the scope of this work. We agree it is helpful to clarify this goal. We have added the word "***Greenland***" to several sentences throughout the paper (namely in the abstract, discussion, and conclusion) to clarify our intentions. Additionally, there are certainly implications of this work that could be applied to other ice sheets or glaciers, but fully diving into those and describing them is outside the scope of the present study. We acknowledge that your suggestions could make a very interesting additional paper, but we hope that this manuscript lays the groundwork for further research into more general atmosphere–firn interactions.

- this paper is long and quite wordy in ways that I don't think are necessary to get the message across. Many general statements that could be deleted or condensed. Figures made more focused on the

We have made multiple revisions to improve clarity and reduce length.

- There are a lot of figures that to me seems to re-inforce the same conclusions and I got a bit overwhelmed trying to figure out what new I was learning from each figure. Each figure should have a clear message that is easy to grasp even if the reader is skimming the papers.

Thank you for expressing this concern. We selected figures to fully show our results. We use the text to describe what is in each figure and why it is important. That said, we understand this concern. In response, we reduced the number of figures in the main text. Specifically, we have moved the original Figure 6 to the appendix. This figure reduction enables us to communicate our results with fewer figures. Figure numbers have been updated to reflect this change and the main text now includes only 9 figures while the appendix has 2.

- I really like the idea that this asymmetry might affect the way the ice sheet grows or shrinks, but I feel like that message got a little lost along the way.

We’re very glad to know this idea was appreciated. We believe this is a powerful implication of our results, but we do not believe the data support making overly strong claims about the role of firn in ice sheet growth/decay. An interesting next study could be to attempt to quantify the contribution of firn air content changes to overall ice sheet mass changes.

Specific comments:

Abstract:

- In the first sentence I’d suggest saying that the porous layer of snow stores meltwater and limits **the rate at which Greenland contributes to sea level rise**.

Done.

- the abstract states that the the paper demonstrates that fir air content is more efficiently depleted… but doesn’t provide any information to the reader as to the specific findings.

We have added the following sentence at line 12 to provide more specific findings while keeping the focus on the big-picture results.

One hundred years after a 1 °C temperature perturbation, warming decreases air content by 9.7 % and cooling increases it by 8.3 %.

Introduction

Line 24 - check wording, should be contribute, instead of contribution

Fixed.

Lines 32-40 - I understand the authors are trying to be succinct, but much of this paragraph is written in fairly general statements and the citations do not seem appropriate. For example the idea that meltwater changes firn was discovered much before 2015. If the authors wish to cite the more recent papers, then please be specific about what new knowledge those newer papers contributed.

Following this suggestion and those of another reviewer, we have removed “(MacFerrin et al., 2022)” in reference to general statements about overburden stress and “(Kuipers Munneke et al., 2015)” in reference to meltwater altering firn properties.

Methods

The model - In general I would like to seem more details. The reader should be able to read this paper and get at least the basic elements of the model the parameters used and any necessary details without having to go read another paper or papers first.

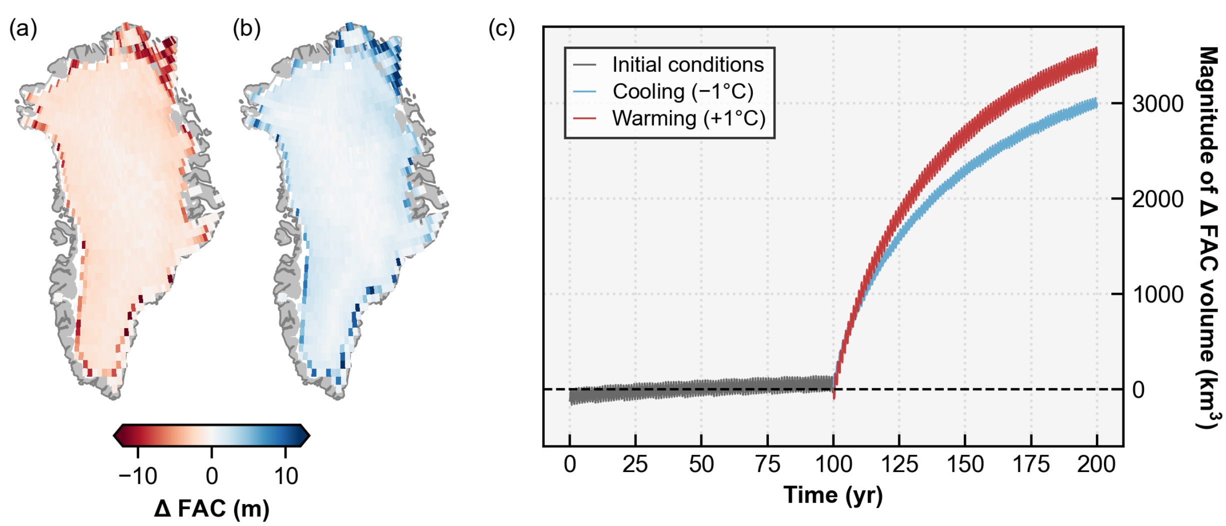
Given the length of the paper and the goal of remaining on message, we chose to only describe the general workings of the model. Several other papers have used and thoroughly described the SNOWPACK model on ice sheets and specifically on Greenland, and we have cited those papers in lines 67-70. In the present study, we use SNOWPACK as a tool to help understand firn–climate interactions. Other firn models should be able to reproduce similar results, so detailing the inner workings of the chosen model would not contribute to the present focus of the paper. See lines 62-70 of this document for more.

The experiments - Line 100 - “we require” - why? I see why you want no trends, but our real world does have noise at all timescale, so even in a steady climate there will be noise. I would suggest repeating the experiments with constant climate but added noise. Perhaps that is a different paper (if the authors want to contact me for more information on this please do, I’ve done some not-yet- published work on firn thermal structure in noisy versus “pure” steady climate conditions).

This study builds off existing work such as that of Kuipers Munneke et al. (2015). While we do not use the same model or even the same forcing, we appreciated their study design that used an idealized forcing. As such, we chose to take a similar approach in the creation of our forcing data. We acknowledge your point that the real world does have noise at all time scales, and that is certainly an idea that we discussed in the design of this study. However, we came to the same conclusion that you did: it could serve as a study on its own. For this paper, we chose to start as simply and reasonably as possible and explain firn responses in an idealized framework. We also note that since we used a full year of MERRA-2 data, there is noise included on the sub-annual scale. We also found our overall results are not sensitive to the year of MERRA-2 data we use to define the base climate.

Line 104-105 - We verified - how? What do you consider negligible? I am assuming you ran the same model using a different year’s climate and the results on warming/cooling were less than 1% different or something?

We ran the model for additional years and found that warming always has a slightly stronger (a few percent) effect than cooling. The exact firn air content values changed, but the asymmetric relationship remained. 1991 is a good choice of year because there were no significant changes to the firn air content that would affect our spin-up period. As an example, we used data from 1980 (the first year available) and created the following figure in our preliminary work. We found that the same warming/cooling relationship existed, but firn air content increased during the year of 1980, which resulted in a trend in the control data. We avoided this trend by using a year with little change (1991).



Finally some of the limitations of the model and the experimental design are not noted until the very end of the discussion section - and it seems like it would be nice to put those right up in the methods because they are choices that have been made in designing the experiment and the analysis.

We have chosen to keep the study limitations in the discussion so that we can discuss them in the context of our results and other papers. Moving this text to the methods would introduce topics that do not appear until later in the study, and we would prefer to not split up the discussion of limitations.

Results

Lines 120-135 - this entire paragraph is presented as results, but to me it seems like a confirmation that the model mostly does what we expect. Maybe just rewording this paragraph? statements like “melt occurs where the temperatures are highest” seems so obvious that I wonder what the authors want me to learn from this paragraph.

Thank you for pointing this out. As you noted, we did intend for this paragraph to show that the model behaves as expected. However, we also included it to make the results approachable to a wider audience that may be less familiar with ice–atmosphere interactions. To give this paragraph more context, we have changed the first sentence to:

We first present the modeled ice sheet and climate mean state during the control period prior to any atmospheric warming or cooling to confirm the model behaves as expected (Fig. 1).

We also removed the following sentence as we agree that it adds very little new information:

During the control period, melt occurs where the temperatures are highest.

Line 136-139 - these statements could use specifics. “Idealized warming causes depletion of firn air content” - by how much, how measured. The first specifics offered are for the total spatially integrated air content.

We have changed this sentence to reflect that we mean everywhere on the ice sheet. It now reads:

When perturbed from the control state (Fig. 1), idealized warming causes depletion of firn air content (Fig. 2a) while idealized cooling causes generation of firn air content (Fig. 2b) in all modeled grid cells in Greenland.

I think overall this section could be condensed down and focused on comparing what was learned to what was expected.

We have condensed this section down by removing the last two sentences about reaching equilibrium.

Line 151 - timescales of response to a perturbation are typically defined by an e-folding time, I’m not quite sure I understand the need to use the percentages.

We chose to use percentages because using an e-folding time assumes the data behave exponentially. While the ice-sheet-integrated curves appear to be related exponentially/logarithmically, many individual grid cell time series are not. The original Figure 7 provides a few examples (namely, panels e and f) of where assuming an exponential change in firn air content would be a poor choice. The times to percent changes instead do not make any assumptions about the shape of the curves and are more robust quantifications of the rates of change.

Moreover, the equation that would best fit the data in, say, Figure 2 (the ice-sheet-integrated firn air content) would be in the form $y = A - Ae^{-t/\tau}$ where τ is the e-folding time. The data do not reach an equilibrium within the experiment time frame, so we would have to make assumptions about the value of A to get at an e-folding time.

Line 167-169 - again very general statements.

We have removed these statements.

Figures

- most of the figures really could use contours instead of just colors. I can’t tell dark from darker colors and the authors point to various areas where they expect us to see color variations (such as SE

Greenland coastal areas). Also - many of the figures contain the full spinup period - this seems like a lot of wasted space, perhaps modify it to zoom in on the results that are the most useful for telling the story.

We appreciate this suggestion and have made modifications to several figures to address this. We have updated Figures 1, 2, and 3 using filled contours rather than smooth shading. (Since the original Figure 9 shows discrete categories of responses that are spatially unrelated, we have not added contours here.) As an example of our updates, below is the modified Figure 2 and its caption.

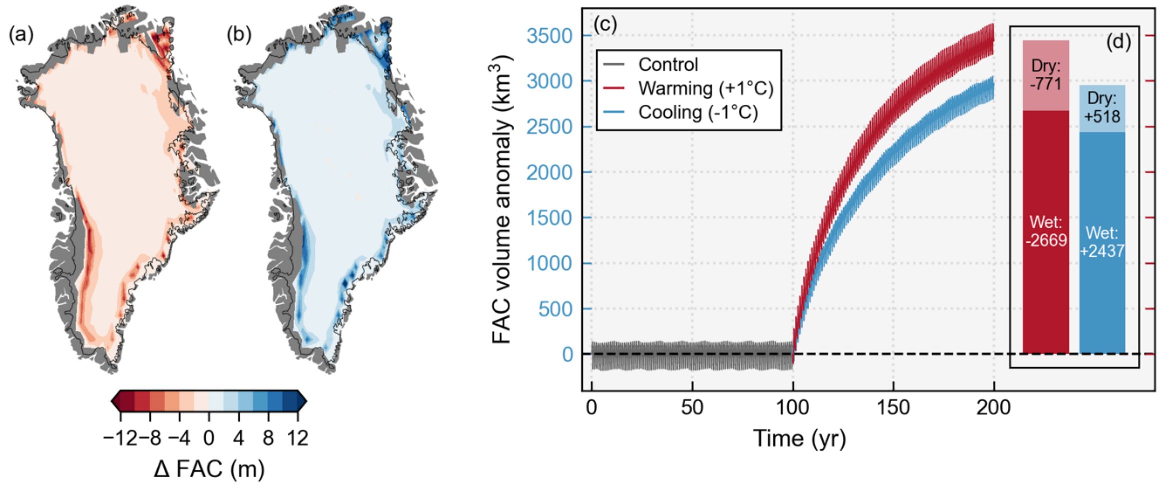


Figure 2: Change in firn air content (FAC) for the Greenland Ice Sheet calculated as the final firn air content minus the mean of the control conditions for (a) warming by 1 °C and (b) cooling by 1 °C. The thin black line represents the ice sheet outline. (c) Time series of the firn air content volume anomaly integrated over the full ice sheet for the control (gray line), warming (red line), and cooling (blue line) periods. The control period and cooling experiment use the positive values on the left y-axis and the warming experiment uses the negative values on the right values on the right y-axis. (d) Changes in spatially integrated firn air content in km^3 at the end of the experiments partitioned into wet firn (melt is present) and dry firn (melt is not present) areas for warming (red) and cooling (blue).

While we acknowledge that showing the spin-up period in the figures takes up room, we are choosing to show it in order to support our claim that the control period is stable. It can be valuable for equal-length time periods to be shown when comparing the perturbed states to the control state, as it makes for a more direct comparison.

- is there a reason not to use an e-folding time to express the timescales, Figure 3, for example, could be just one plot instead of 6. Or maybe 2 if the authors want to emphasize how much change happens in the first 2.5% of time.

Please see our earlier response regarding e-folding times, as we believe we have addressed this comment in that response (lines 244-254 of this document).

- Among figures 7 an 8, a few things could make the data more digestible - In fig 7 for example, All 6 could be plotted on the same curve as relative change in FAC. Because of the different scales, it is hard to compare them. Also Fig8 take a while to digest - similarly the “control climate” takes up a lot of ink, and then what my mind really wants to see is a diagram what a firn core might look like in each situation. Also Fig 8 only shows two locations - it would be helpful to find a way to express this information for all of them.

The original Figures 7 and 8 do contain a substantial amount of information, but we feel that it is necessary for supporting our conclusion that the temperature–firn relationship is highly complex and to show all of the physics that is included in this modeling (as summarized in the original Figure 10). Plotting all six examples from the original Figure 7 on the same curve (even if normalized) would result a loss of valuable information and arguably a more confusing figure. For instance, plotting panels (e) and (f) on the same axis would result in (f) being very hard to see since ~3 m of change occurs in (f) while ~8 m occurs in (e). The intention of this figure is not to directly compare each of the examples, but rather compare the signature of warming and cooling *within* each panel and showcase the wide variety of responses we find in just 6 out of 1724 cases.

As for Figure 8, we have in part addressed this in a previous comment, but we also acknowledge that a similar type of figure is shown in Kuipers Munneke et al. (2015) and we found theirs to be very informative. Finally, we use this figure to show one of the simpler cases and one of the most complex cases as end member responses. While the other example could be interesting, there are 1724 of these panels we could also plot and could be potentially interesting. We are choosing to just show two examples that help drive home our conclusions instead. However, we have supplied all the code and data (see the code/data availability section) so readers are able to explore the full dataset.

Again - lots to like in this paper, most of my comments are towards focusing the message and making it easier to read and digest.

Erin Pettit

References used in this response

Bartelt, P. and Lehning, M.: A physical SNOWPACK model for the Swiss avalanche warning Part I: numerical model, Cold Reg. Sci. Technol., 23, 2002.

Kuipers Munneke, P., Ligtenberg, S. R. M., Suder, E. A., and Van den Broeke, M. R.: A model study of the response of dry and wet firn to climate change, Ann. Glaciol., 56, 1–8, https://doi.org/10.3189/2015AoG70A994, 2015.

Lehning, M., Bartelt, P., Brown, B., Fierz, C., and Satyawali, P.: A physical SNOWPACK model for the Swiss avalanche warning Part II. Snow microstructure, Cold Reg. Sci. Technol., 21, 2002a.

Lehning, M., Bartelt, P., Brown, B., and Fierz, C.: A physical SNOWPACK model for the Swiss avalanche warning Part III: meteorological forcing, thin layer formation and evaluation, Cold Reg. Sci. Technol., 16, 2002b.

Thompson-Munson, M., Wever, N., Stevens, C. M., Lenaerts, J. T. M., and Medley, B.: An evaluation of a physics-based firn model and a semi-empirical firn model across the Greenland Ice Sheet (1980–2020), The Cryosphere, 17, 2185–2209, https://doi.org/10.5194/tc-17-2185-2023, 2023.