

# Evolution of Antarctic firn air content under three future warming scenarios

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*Submitted to The Cryosphere*

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## General comments

Veldhuijsen et al. update the highly used IMAU Firn Densification Model to account for changing climate conditions, apply a new ice slab permeability expression, and ultimately simulate firn air content (FAC) across the Antarctic Ice Sheet in three future warming scenarios. The authors provide a detailed description of the new model configuration as well as an evaluation of the model. This allows them to explore the evolution of FAC in a changing climate with more confidence.

This paper provides valuable information about how Antarctic firn could behave in different warming scenarios, which is critical for accurately quantifying the ice sheet's future contribution to sea level rise and assessing the stability of ice shelves. Moreover, this paper has a clear motivation and provides an important advancement for semi-empirical firn modeling. As such, this research fits within the scope of *The Cryosphere* and will be an asset to the firn modeling community.

While the science, methodology, and key results of this work are well done and valuable, improvements to the presentation of this work need to be made for publication. This paper is somewhat difficult to follow—even as a reader with a background in firn modeling. A lot of information presented feels disconnected from the key findings related to future Antarctic FAC evolution. My primary suggestion (in more detail below) is to remain more focused on the applications of the model throughout the paper and reconsider what information is pertinent to the key results that are outlined in the abstract.

## Specific comments

### Excessive Information in Sections 2 and 3

This paper represents a phenomenal amount of work, and because of that, it reads as two separate papers in its present form. Sections 2 and 3 contain a detailed description of the model adjustments as well as results of the new methodology. Because these sections have methods, results, and five figures, they feel like their own separate study. It's clear that an evaluation of the new model is needed for the results presented in the rest of the paper to be trustworthy. However, Sections 2 and 3 contain so much information that it's difficult to take away the key points. From the rest of the paper, it seems that the main tool being used to calculate future FAC evolution is IMAU-FDM v1.2AD-C, but four model configurations are listed in Table 2 and evaluated in various combinations (e.g., Fig. 4 shows results of three of the model configurations, and Fig. 5 shows some comparisons but not all).

As a reader, I want to walk away from Sections 2 and 3 knowing (1) why a new configuration was needed, (2) what the new configuration is, and (3) how much it improves upon the existing configuration. Points (1) and (2) were well executed in this work, but (3) is where the study falls short. There's so much additional information about four separate model configurations that the reader cannot easily understand what is being compared and why. In my opinion, these sections distract the reader from the main results concerning the application of the new model configuration.

To remedy this, one suggestion is to rewrite Sections 2 and 3 with a very clear focus on the development, calibration, and evaluation of IMAU-FDM v1.2AD-C. Anything important but not directly pertinent to IMAU-FDM v1.2AD-C could go in the supplement. Using Fig. 4 as an example, it does not seem necessary as a reader to know about the MO ratios and fits of model configurations that are not used to create the key results of the study. Perhaps the IMAU-FDM v1.2AD-C results in Fig. 4 could remain in the main text and the other data could go in the supplement.

### Reasoning for Parameter and Methodology Choices

There are a handful of places throughout the study where a parameter or methodology choice is made but the reasoning is not provided or not adequately explained. For example, lines 135–144 contain several values for model parameters but little explanations as to why they were selected. A few other places that could use further reasoning are: the choice to use a sigmoid function (line 218), the choice of applying the permeability factors by multiplying (line 228; is there a mathematical or physical reasoning for why firm permeability would be multiplicatively dependent on the overlying layers?), and the selection of different time periods for analysis. This final point is important since it provides the reader with context as to why certain times are being compared. It's clear that a lot of thought has been put into this research and the choices made were intentional, but without explicitly telling the reader why a method/parameter was chosen, it can seem arbitrary.

### Full ice sheet vs ice shelves

It's unclear whether this study is focusing on FAC on the entire ice sheet or just the ice shelves. The abstract and conclusions seem to exclusively focus on the ice shelves, but at least half of the figures are showing results of the entire ice sheet. Perhaps including a few sentences in the abstract/conclusion mentioning changes to the rest of the ice sheet beyond the ice shelves could remedy this.

Also, the limitations (Section 5.2) should mention that the calibration data mostly come from non-ice shelf areas (Fig. 2). We are of course limited by the availability of observations on ice shelves, but it should still be mentioned since there's such a heavy focus on the results over ice shelves.

Consistency in accumulation and melt units – Both “mm w.e. yr<sup>-1</sup>” and “mm yr<sup>-1</sup>” are used throughout this paper. I recommend using just the former if possible.

### **Line-by-line comments**

#### Abstract

7 – Consider specifying what makes these climate scenarios different. Suggestion: “three climate scenarios” → “three climate emissions scenarios”

8 – Since “accessible FAC” has not explicitly been defined yet, I suggest adding a short explanation: “To estimate the accessible FAC” → “To estimate the accessible FAC, which is the pore space meltwater can reach,” (or something along those lines).

9-10 – This sentence could be more impactful if the reader knows the timeframe to which these results are referring. > 50 % depletion by what year? Or over how many years?

10 – “strong and intermediate mitigation scenarios” is somewhat vague and unclear. If referring to SSP1-2.6 and SSP2-4.5, perhaps denote that: “strong (SSP1-2.6) and intermediate (SSP2-4.5) mitigation scenarios”. Perhaps the lack of clarity comes from the idea that these scenarios are often described using their emission strengths rather than their mitigation strengths. The authors could also reword this as “low (SSP1-2.6) and intermediate (SSP2-4.5) emissions scenarios”.

13-15 – This is a great sentence with high impact. A small suggestion is to change the word “viability” to something else. Perhaps “vulnerability”, “longevity”, “stability”, or even “instability” could work instead.

#### 1 Introduction

19 – “Both reduce their buttressing effect” → “Both reduce the buttressing effect of ice shelves”

28-29 – The sentence beginning with “Currently” is a little unclear and could use some rewording to help the reader understand. It’s unclear how “(i.e. where sufficient tensile stress is present)” is related to the language used before.

32 – “impermeable ice slabs” → “low-permeability ice slabs”

39 – “While runoff is a measure of firn saturation” feels a little like a bit of a leap, especially since firn can be unsaturated at depth but have near-surface ice slabs that drive runoff. Suggestions: “While runoff is indicative of the firn saturation”, “While runoff is strongly related to firn saturation”, or something along those lines

39-43 – In addition to the above comment, these sentences could use some re-wording or re-organization. Specifically, the sentence “The main advantage of using an offline firn model...” feels very out of place. It’s unclear if the sentence before that one was describing the alternative to an offline firn model. Also, the final sentence in this paragraph could be moved to Section 5.2.

46 – “forced by outputs of regional climate models” feels redundant and can probably be removed since it’s written verbatim in the previous sentence.

51-53 – Consider splitting the sentence “These ice slabs...” into two sentences since there is a lot of information packed in here and there is a natural break in the information flow before “can impede...”

## 2 Methods

74 – “referred as” → “referred to as”

74-76 – If possible, it could be helpful to briefly summarize the findings from the evaluation in Veldhuijsen et al. (2023).

89 – Should the accumulate rate units instead be “ $\text{kg m}^{-2} \text{s}^{-1}$ ”? It seems that the “-1” superscript is missing for the seconds.

98-101 – The description of the variable  $D$  is unclear and needs to be rephrased to help the reader understand. Is it saying that  $D = 0.03$  for  $\rho > 550 \text{ kg m}^{-3}$ , and  $D = 0.07$  for  $\rho < 550 \text{ kg m}^{-3}$ ? Also, splitting this sentence into two could help with clarity and flow.

132 (Eq. 9) – It’s unclear how this equation represents the local long-term mean accumulate rate. The units of the variables in this equation have not been explicitly stated, but pressure typically has units of  $\text{kg m}^{-1} \text{s}^{-2}$  and age should have units of time. Therefore, the accumulate rate from Eq. (9) would have units of  $\text{kg m}^{-1} \text{s}^{-3}$  instead of  $\text{kg m}^{-2} \text{s}^{-1}$ . It seems something is either incorrect with the equation or the units, but regardless, a clearer explanation is needed for defining this long-term mean accumulation rate.

142 – What is the significance of the thickness range? It’s also unclear how the layer thickness explicitly affects the densification of the freshly fallen snow.

Figure 1 – The use of shared  $y$ -axes makes this figure feel overly complicated. The language used for the melt and accumulation axis label makes its meaning ambiguous (i.e., it seems like it’s saying melt *plus* accumulation when that is not the case). An alternative label could be “mass flux”, or it could be “Surface melt ( $\text{mm w.e. yr}^{-1}$ ) [line break] accumulation ( $\text{mm w.e. yr}^{-1}$ )”, or just something to convey that these aren’t added together. The fact that the temperature axis spans  $-45$  to  $-15$  °C is somewhat misleading as

well since the temperatures actually only span  $\sim -39$  to  $-29$  °C. Finally, having temperature in between accumulation and melt is confusing since accumulation and melt share a  $y$ -axis. Please note, most of these are just suggestions that would make the figure easier to interpret, but they are not absolutely essential to change.

192 – Please specify which version(s) of the firm model is being compared to the observations. Based on Fig. 2, it seems that it's FDM v1.2AD-C, but no explanation is given as to why that version is being used here.

Figure 2 – Consider changing the color and/or of the star since yellow does not stand out with that color map. Cyan or magenta may work better.

206 – “3–5 cm”  $\rightarrow$  “0.03–0.05 m”

203-207 – This argument was hard to follow and could benefit from clearer language. It's not immediately apparent whether the results of Samimi et al. (2020), (2021), and Clerx et al. (2022) are in agreement or not.

211 – This final phrase is confusing; please elaborate or clarify.

215 – It looks like the citation has not been added in the correct part of the sentence.

Figure 3 – The use of an inverted  $y$ -axis for FAC is not intuitive. A quick glance at this figure makes it seem that density is increasing and FAC is also increasing.

### 3 Calibration and model performance

Figure 4 – Should “FDM v1.2A-C” instead be “FDM v1.2AD-C” in the caption?

244 – What is the “( $< z_{550}$ )” referring to?

Figure 5 – Flip “(e)” and “(f)” in caption. It could be useful to explicitly state how the difference in (d) and (f) is calculated, or at least say “positive values indicate greater FAC due to  $x$  model, negative values indicate greater FAC due to  $y$  model”. Also, why does panel (e) only show two of the models? Why not also include FDMv1.2AD-C?

### 4 Results

273-274 – What is meant by “most ice shelves”? Are the values reported not referring to all ice shelves?

274-279 – Should all of these percentages have negative signs? It seems that the sign of the change is the same for all but only some are reported as negative.

287 – It seems another negative sign is missing on “76 %”.

292-293 – The sentence, “For 42 % of those...” could benefit from being reworded. It's also unclear why a range of years is reported rather than a single year. Another sentence at the beginning of the paragraph could help set up the reader to understand why that range is being evaluated.

294 – There is an inconsistency between the date range in Fig. 7 (2005-2014) versus what’s reported here (2005-2015).

302-305 – In the description of Fig. 7c, it could be useful to mention that differences between total and accessible FAC are being calculated over 2060-2100 because that is (presumably?) when they begin diverging. Additionally, please note either here or in the Fig. 7 caption how the difference is calculated. Is it the mean over the 2060-2100 period, or the difference in the final FAC values at 2100, or the time-integrated difference?

328 – “arial” → “areal”

329 – What is meant by “only limited meltwater storage capacity left”? Is there some kind of threshold prescribed here?

Figure 8 – Please consider making the gray grid lines lighter or thinner so as not to distract from the actual data. If possible, please make the lines in the legend thicker so they are easier to see. This figure has some really important information but it’s difficult to visualize in its current presentation.

348-351 – Consider rewording or splitting up this sentence to make it easier for the reader to follow.

Figure 9 – The description of panels (a) and (b) in the caption is vague. It needs to be clarified (as it was in the main text in line 345) that one is showing absolute and one is relative. Also, why is the 25 % threshold applied here? Is there some reason why not all areas are shown? As for panels (c-h), there is a lot of information packed in here and the use of so many  $y$ -axes is hard to follow. The main text primarily discusses MOA (lines 352-360), so perhaps the surface melt and accumulation could be removed from this figure, especially since they are used to calculate MOA. If those variables are retained, check that the units are correct (should they be  $\text{mm yr}^{-1}$  or  $\text{mm w.e. yr}^{-1}$ ?)

363 – Why this date range?

378 – Change units of K to  $^{\circ}\text{C}$  to remain consistent.

Figure 10 – Would it not be more useful to see a 1:1 comparison of the two models? In other words, FAC from FDM v1.2A vs FAC from FDM v1.2AD in panel (a)? This is just a suggestion and can be ignored.

## 5 Discussion

406-408 – Citation needed for this sentence: Jullien et al., 2023 perhaps.

423-424 – Consider rewording this to make it easier to read.

458-460 – Has this sensitivity been tested and reported somewhere?

## **References used in this review**

Jullien, N., Tedstone, A.J., Machguth, H., Karlsson, N.B., Helm, V.: Greenland Ice Sheet Ice Slab Expansion and Thickening, *Geophysical Research Letters*, <https://doi.org/10.1029/2022GL100911>, 2023.