Response to Reviewer 1

First of all, we would like to thank the reviewer for their time for reviewing and editing our work. We appreciate the constructive feedback we received. Following the feedback we have clarified and revised the structure of the text. We believe that the given suggestions will improve the presentation of our results and improve the readability. Responses to the comments of the reviewers are written in **red** and citations of the manuscript are written in **blue**.

Kind regards, Sanne Veldhuijsen

General comments

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.

Thank you for these comments. We agree that the paper contains a lot of information, and we can imagine that it can therefore be difficult to follow. We followed your suggestion by focusing on the applications of the model and only showing the most relevant results in Sections 2 and 3. We have for example moved some information of Figure 4 and 5 to the Supplementary materials, and we have rewritten Section 2 and 3. Section 2 contains the model development description, experimental setup, and the calibration and evaluation results. Section 3 contains the accessible FAC calculation description (see Sections 2 and 3 and the supplement of the revised MS). Point (3) is not one of the takeaways. We explain with Point (1) why the new configuration is needed. However, for the historical period, which

we need to use for evaluation, the performance of the models is comparable, which is also logical, since there are no trends in temperature and accumulation.

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.

The FDM v1.2A merging and splitting boundaries already existed and were not chosen in this study. To approximate densification of freshly fallen snow, we keep the upper layer to be 1 cm instead of 15 cm (boundaries 0.8 and 1.2 cm – to avoid layer adjustments each timestep), we expand our explanation: "Accumulation is low in many parts of Antarctica and therefore 0.15 m of snow can consist of snow from multiple years." and: "We use this small range around 0.01 m to avoid mass needing to be constantly added or removed to the upper layer to keep it exactly at 0.01 m thickness."

A few other places that could use further reasoning are: the choice to use a sigmoid function (line 218), We clarify this in the revised MS: "We use a sigmoid function because it has a characteristic S-shaped curve, representing that thin ice lenses are very permeable, while even very thick ice layers only approach complete impermeability at a large scale."

the choice of applying the permeability factors by multiplying (line 228; is there a mathematical or physical reasoning for why firn permeability would be multiplicatively dependent on the overlying layers?), Yes, if an ice slab has a permeability factor of 0.5, it means that only 50% of the water can percolate through. If there are two ice slabs both with a permeability factor 0.5 overlying a specific firn layer. It means that only 25% of the water can reach that specific firn layer. So, you have to multiply it with 0.5 twice.

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. We agree that it is indeed important to explain the periods which we use for analysis. We add this in e.g.: "Figure 5f shows the difference in FAC over the AIS between FDM v1.2AD-C and FDM v1.2A-E for the period 1979-2014 (the overlapping historical period)." And see our responses to comments L363, L292-293 and L302-305.

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. We agree that this is indeed confusing, to remedy this we decided to change our title to: "Firn air content evolution on Antarctic ice shelves under three future warming scenarios." We still show the figures containing the entire content, as it would also be difficult to only show the ice shelves area, so therefore it does not take up more space to include the grounded ice. In addition, this gives more insight into the how firn responds to a changing climate.

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^{-1}" and "mm yr^{-1}" are used throughout this paper. I recommend using just the former if possible. That is indeed true. We have added this in the discussion section as follows: "In line with this, the in situ observations used for calibration and evaluation of the model are mainly situated in non-melt regions." In addition, we will use mm w.e. yr-1 consistently throughout the MS.

Line-by-line comments

Abstract

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

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). It is defined in L5: “accessible FAC (i.e. the pore space that meltwater can reach)”.

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? Thank you for noticing this, we have included “by 2100”.

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”. We have reworded this by “low (SSP1-2.6) and intermediate (SSP2-4.5) emissions scenarios”. We also changed this in other parts of the MS.

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. We change this to “vulnerability”.

1 Introduction

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

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. To clarify, we rephrase this as: “Currently, 60 % of the ice shelves (by area) both buttress upstream ice and are vulnerable to hydrofracturing if inundated by meltwater (i.e. where sufficient tensile stress is present) (Lai et al. 2020).”

32 – “impermeable ice slabs” → “low-permeability ice slabs” We agree and have also adjusted this in the abstract.

39 – “While runoff is a measure of firn saturation” feels a 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 We agree, and have used “indicative” instead of “a measure”.

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. We agree that this part is unclear. These sentences will be separated from the previous ones, and rewritten as en entire new paragraph.

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

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…” We agree and have rewritten this sentence as two separate sentences: “Ice slabs are common in

Greenland (MacFerrin et al., 2019; Culberg et al., 2021) and have locally been observed in Antarctica on Larsen C ice shelf (Hubbard et al., 2016). They can impede vertical meltwater percolation to deeper firn, limiting the fraction of the FAC that is accessible for meltwater. "

2 Methods

74 – "referred as" → "referred to as" Done

74-76 – If possible, it could be helpful to briefly summarize the findings from the evaluation in Veldhuijsen et al. (2023). Since we are including FDM v1.2A in the evaluation of this work, and to reduce the length of the paper, we decided to not summarize the evaluation.

89 – Should the accumulate rate units instead be "kg m^{-2} s^{-1}"? It seems that the "-1" superscript is missing for the seconds. Thank you for noticing this.

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 $\rho > 550$ kg m^{-3}, and $D = 0.07$ for $\rho < 550$ kg m^{-3}? Also, splitting this sentence into two could help with clarity and flow.
We have rewritten this sentence in the revised MS, also in line with different comments of reviewer 1 and 2: "in which D is a constant with different values above (0.03) and below (0.07) the critical density level of $\rho = 550$ kg m^{-3} to represent two distinct densification mechanisms (Herron and Langway, 1980)".

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 kg m^{-1} s^{-2} and age should have units of time. Therefore, the accumulate rate from Eq. (9) would have units of kg m^{-1} s^{-3} instead of kg m^{-2} 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. The gravity constant g is missing in Eq. 9 and will be added in the revised MS.

142 – What is the significant of the thickness range? It's also unclear how the layer thickness explicitly affects the densification of the freshly fallen snow. We clarify this as follows: "We use this small range around 0.01 m to avoid mass needing to be constantly added or removed to the upper layer to keep it exactly at 0.01 m thickness." In addition, this entire paragraph has been restructured and moved to the supplementary materials (See supplement Text S1).

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 (mm w.e. yr^{-1}) [line break] accumulation (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 ~-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. To improve the readability of this figure, we break it in subplots.

192 – Please specific which version(s) of the firn 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. We add that this is FDM v1.2AD-E, FDM v1.2A-E and FDM v1.2A-C.

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. We slightly enlarged the star and used turquoise.

206 – "3–5 cm" → "0.03–0.05 m" Changed.

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. To clarify, we reorder these sentences: "A small-scale field experiment conducted in Greenland shows that ice layers of 0.12 m can still be completely permeable for liquid water (Samimi et al., 2020). On the other hand, in another small-scale field experiment in Greenland, ice lenses of only 3-5 cm have also been found to be partly impermeable (Clerx et al., 2022). Samimi et al. (2021) assume that ice layers thicker than 0.5 m act as impermeable barriers and prescribe a non-linear decrease in permeability between 0.1 and 0.5 m." In this way it is indeed clear that Samimi and Clerx are not agreeing with each other, and Samimi et al. (2021) is stated afterwards as it presents permeability observations for a different thickness range.

211 – This final phrase is confusing; please elaborate or clarify.
To clarify we rephrase: "For ice layers to be impermeable on a large spatial scale, such as in IMAU-FDM simulations, this requires at least a larger thickness than the ones found to be impermeable in the small-scale field experiments, thereby we assume that lateral connectivity of ice layers increases with ice thickness."

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

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. We invert the y-axis for FAC.

3 Calibration and model performance
Figure 4 – Should "FDM v1.2A-C" instead be "FDM v1.2AD-C" in the caption? Thank you for noticing this, we have improved this.

244 – What is the "(< z_{550})" referring to? Z550 is defined in L249, the depth of critical density level rho = 550 kg/m3. For clarity, we rephrased this is "(below z550)".

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? (e) and (f) are flipped, and we add in the caption how the difference is calculated. FDM v1.2AD-C has a different forcing, and here we want to compare the relation between FAC and MOA for the different models. Since Fig. 5d suggests that there is a relation between those. The panels d-f will be moved to the supplementary material based on other comments by reviewer 1 and 2.

4 Results
273-274 – What is meant by "most ice shelves"? Are the values reported not referring to all ice shelves? Yes, they are referring to all ice shelves, so we remove "most".

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. Thank you for noticing this, all have negative signs now.

287 – It seems another negative sign is missing on "76 %". Added.

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. We rephrase this as follows: "On 42 % of those melt-free locations, FAC decreases by 2090-2100, and on the remaining 58 %, FAC increases." In the revised MS we consider the last year of the historical and future period instead of decades.

294 – There is an inconsistency between the date range in Fig. 7 (2005-2014) versus what's reported here (2005-2015). This should be 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? Thank you for these suggestions, we add in the caption how the difference is calculated and explain in the text why we select the 2060-2100 time period, which is indeed because ice slabs become more widespread after that: "We select this period as this is when total and accessible FAC start to diverge."

328 – "arial" → "areal" Changed.

329 – What is meant by "only limited meltwater storage capacity left"? Is there some kind of threshold prescribed here? We explain now in the method section how runoff is calculated: "Once meltwater has saturated the lowermost firn layer, we assume that it will leave the firn column as runoff instantaneously."

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. Thank you, we follow these suggestions.

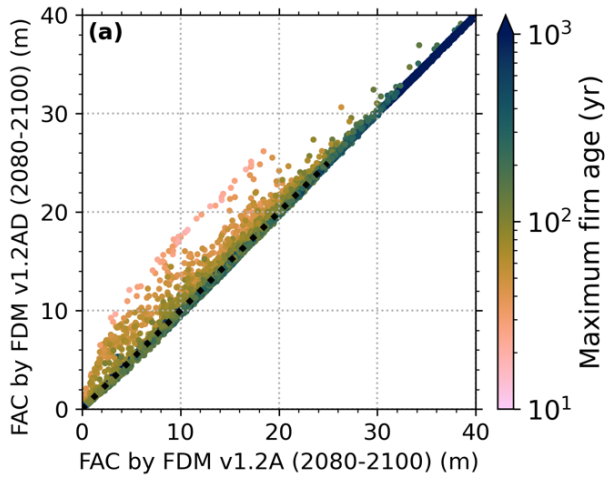
348-351 – Consider rewording or splitting up this sentence to make it easier for the reader to follow. We rephrase this as: "The differences are lowest in the Bellingshausen Sea region, on ice shelves as Wilkins, George VI and Stange and Abbot, and on the Getz and Crosson ice shelves (< 3.9 m and < 47 %), which are among the warmest and wettest ice shelves of the AIS (> -19 °C and > 600 mm yr−1). The absence of ice slabs under these conditions is also depicted in Fig. 7c."

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 mm yr^{-1} or mm w.e. yr^{-1}?) We rephrase this as: "The maximum (a) absolute and (b) and corresponding relative difference .." The threshold is applied here, because if there is only a very small increase, a relative increase of 100% does not say much. We change the units to mm w.e. in the figure. The absolute values of accumulation are also discussed in the text, therefore we decided to keep the variables as is.

363 – Why this date range? Here, we include two decades as "end of the century", because from 2090 the firn has already disappeared in both models in quite some regions (See Figure 8/Figure 6 of revised MS). We add this: "We select the period 2080-2100 as the firn layer disappears in 2090 on some ice shelves (Fig. 8b)."

378 – Change units of K to °C to remain consistent. Done

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. We used this suggestion in draft versions of this manuscript. However, in a 1:1 comparison it is hard to see differences as all data is close to the 1:1-line.



5 Discussion

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

423-424 – Consider rewording this to make it easier to read. We rephrase this as: "The AIS ice-shelf-wide runoff extent in Fig. 8, 25 % by 2100 for SSP5-8.5, is substantially lower than reported by Gilbert and Kittel (2021), who estimate a runoff extent of 98 % by 2100 using the regional climate model MAR forced by exactly the same CESM2 realisation."

458-460 – Has this sensitivity been tested and reported somewhere? No, this follows from the values mentioned, and therefore we think that presenting testing results is not necessary.

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.