Interactive comment on "Modelling perennial firn aquifers in the Antarctic Peninsula (1979–2016)" by J. Melchior van Wessem et al.

Anonymous Referee #2

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The manuscript by van Wessem et al. describes the formation and persistence of Antarctic Peninsula perennial firn aquifers in two models (IMAU-FDM and SNOW-PACK). Overall, both models show strong similarity in the location and timing of firn aquifer formation with minimal differences driven by various model parameters. These results suggest that careful consideration of model parameters and further investigation into firn processes are necessary to resolve the admittedly limited differences between the used models. However, the general similarity suggests that near surface hydrology has potentially played an important role in long-term ice sheet and ice shelf dynamics and further model development including meltwater transport is warranted.

Overall, the manuscript is generally reasonably written (see notes) and confirms many of the modeling and observation results initially developed in Greenland. Despite this,

there are several areas where the manuscript would benefit from additional improvements.

This manuscript is first and foremost a limited inter-model sensitivity study. While, I do believe it is relevant to more carefully and systematically examine the role of different model parameters in controlling firn aquifer formation and evolution. However, this is likely untenable at this point in the process. Therefore, it is strongly recommended that the manuscript very carefully expand the discussion on factors contributing to the differences in the models, with perhaps a table clearly laying out model differences and whether those differences are tunable/changeable, etc. Right now, it is only a few lines. But the authors can easily make an effort to better describe model differences (including irreducible water content) and how they would theoretically affect firn aquifer formation.

The manuscript makes a number of compromises that add to uncertainty in the results. These compromises may be warranted, but they do need to be clearly justified. First, the spin up of SNOWPACK leaves something to be desired. I understand issues with computational availability, but in light of this there needs to be a clear note as to how this with affect the inter-comparison with IMAU-FDM. Next, the comparison of the model results to firn cores from before the modeling window needs to be better scientifically justified – there are some notes to this point in the line comments. Finally, there is the issue of using RACMO melt for SNOWPACK, when it generates its own melt. Here, the argument presented is simplification of figures. This is a silly reason to make a change that will increase the uncertainty in the SNOWPACK results, and I strongly suggest that the authors reconsider this choice – it will make a minimal difference in the figures, but provide more accurate results.

The text could use a through tightening and focusing in some areas and expansion in others. This includes an improvement in the citations, which now are quite limited and narrowly focused on Antarctica; the clear separation of the results and discussion; and an overall focus on only the relevant components of the story. The results section has

quite a bit of discussion related material but lacks quantitively analysis of the results - there are some cases, like the range of conditions where firn aquifers form and persist where numbers would be beneficial to the community and future research. The discussion generally lacks a robust integration of these results within the current body of literature and instead seems to focus on things that need to be added to the models as suggested by others. In addition, the authors need to decide if the in-built RAMCO firn model is important to also consider and if so, the results need to be fully incorporated into the manuscript and figures.

Minor comments Page 1 4-5. What does adequately mean? Some quantitative assessment would be useful – even simply the direction of the bias in each model. 12. Quantify 'most' with a percentage. 12. Quantify 'large part' with a percentage. 16. The word 'timing' doesn't adequately describe what is meant. It's more like intra-annual variability or relative variability between SMB loss and gain. Consider more carefully framing this sentence more carefully and clearly. 24. The phrase 'as well as precipitation rates' should be changed to fit with the form of the other phrases in the sentence.

Page 2 9. There are tons more references to the discovery and behavior of GrIS firn aquifers. In the least, there should be a couple more citations and an 'e.g.' 11. The Bell paper is a perspective, while 'peer-reviewed', there are much better articles to cite here and, in the line below, including, but not limited to Bell et al. (2017 – Nature). 13. Latent heat release is only relevant when the FAs refreeze. This point should be clarified. I will also note again, that there is a broad body of literature about 'cryohydrologic warming' on the GrIS. 14. This line should simply be removed. It isn't necessary for the manuscript to be successful and frankly, unclear if it is true. There are AGU abstracts (which depending on the Journal, considered published, e.g. Miller et al., 2019 – AGU abstract 2019) and papers about supraglacial lakes and subglacial ponding also generally have discussion about firn aquifers. And, in all possibilities, there could easily be a paper in revision, review, press or published during the publication process of this manuscript. 20. This line somewhat implies that there is 'significant' melting during the

winter on the AP. Consider rephrasing. 26. 'observational datasets' across Antarctica or expand references. 26-27. So technically, Forster et al. (2014) utilize the firn model integrated with RACMO, which is, as indicated, different than the IMAU-FDM used in this study. And both citations use previous RACMO versions.

Page 3 4-5. The inclusion of the in-built firn model in RACMO isn't really justified or integrated into the discussion or abstract in any way. The authors should carefully consider whether it provides useful information. If so, it should be more clearly incorporated in the latter parts of the manuscript. 7. This section should focus on relevant atmospheric characteristics. The firn component should be included in the IMAU-FDM section, where it parses how the in-built model is different from the model primarily used in the manuscript. 23. 'Low' relative to what? Other models, observations?

Page 4. 18. Is the weaker densification due to chosen tunable variables, the used densification parametrization, or something else (or some combination)? 20. Perhaps mention that these are on the Plateau and do not affect the areas discussed herein (if this is the case). i.e. emphasize that the model didn't crash in areas analyzed in this paper. 26. Clarify this is because the forcing data is only available from 1979. 27. This statement (Using earlier. . .) should be expanded upon, essentially, this paragraph should clearly and strongly justify why using validation observations from a completely different timeframe can be used. Particularly in light of the recent, rapid atmospheric changes in the area. I think it is possible (e.g. at depth firn temperatures evolve slowly in response to surface forcings), but a careful, well-cited justification should be presented since this is the primary validation method of the manuscript.

Page 5. 10-11. r values should be accompanied by p-values or some measure of statistical significance. 12-13. The inclusion of the RACMO firn results are somewhat ad-hoc, either include them completely with a through discussion as to why the results are different from the other models or do not include them. 10-21. There are some discussion points here that should be moved to the Discussion. 23-4. There is a lot of better vs worse discussion here that heavily rely on Figure 3, which is simply a

qualitative comparison. Care should be taken to quantitively justify 'better' vs 'worse' statements.

Page 6. 1-4. These two sentences don't make a whole lot of sense and should be revised to be clearer and focus on the point of the manuscript. Plus, it is unclear what methodology the previous statement 'confirms' 5. This and the previous section title should be reconsidered. Something more descriptive like "Model characteristic inter-comparison" might be more useful in guiding the reader. 6-7. This should be clarified in the model descriptions above. 31. 'shelve' is the verb

Page 7. 9-10. It would be nice to have a volume comparison too, since there are observational estimates of GrIS PFA volume. 19-20. If they only last 1 year, are they really perennial? 20-22. See main note. 29. Missing an 'I' 30-31. I believe that this is an unnecessary simplification which introduces unnecessary and unaccounted for uncertainty. For the most part, this would mean 3 instead of 2 lines. See main note.

Page 8. 4-9. This is probably going to be a main take away from this manuscript. I would consider emphasizing this by placing the ratio of melt to accumulation on Figure 7. 5. This sentence indicates there is clear relation, but the previous sentence indicates that there is no clear relation. This conflict should be resolved. 7-10. This seems like a symptom of the different irreducible water content values in the models and should be discussed further here or elsewhere. 20. Top right corner isn't quite the right description. 31. What is positive?

Page 9. 2. Leave the speculation until the discussion. 8. Somewhat confused about this reference and Alley et al. (2018) in association with page 2, line 14. Generally, an aquifer is a water baring medium, so if the firn has liquid water stored, it is an aquifer.

Page 10. Inter-model differences: This section should be expanded to systematically assess the model differences as best as possible. In an ideal world, this would involve a sensitivity study, but because the focus of the manuscript is on model inter-comparison with single model runs, an effort should be made to clearly delineate the differences

and how the differences are related to model characteristics vs tuned parameters.

Page 11. 10. Reference for 'other regions as well'? 11. It reflects the lack of lateral water flow. The relevant mechanisms of heat transport and release should be discussed because this is not the only mechanism that could result in cold firn. 23. Odd reference location 30. It would be useful for readers to expand this section.

Page 12. 23-24. Citations may be relevant.

Figures Figure 1. Topography should also be mentioned in main text. Figure 2. See comment about in-built firn model inclusion. Figure 3. The figure text is too small to read. Either make the figure bigger or move the text to a table. The figure should also indicate visually what cores have high melt rates vs low melt rates. Figure 5. Would love to see a difference map too Figure 6. I like this figure, but it would also benefit from a difference map and a color bar that had both % and number of years (since both are used in the main text). Figure 7. I'd made this two (or 3) panels and also include the ratio of melt to ablation for both models over time Figure 8. Is the melt used here for SNOWPACK its actual melt or the RACMO melt? See previous point on this. The caption could also use some refinement to more clearly indicate what each of the panels is. Figure 11. What exactly is the shaded spread?