Response to Reviewer Comments on TC paper

Reviewers' comments

Responses to the comments

Proposed revisions to the manuscript

Responses to Reviewer 1

General comments

The paper is interesting and shows a theoretical explanation of passive microwave time series collected over aquifers in Greenland and Antarctica. The text is well written and easy to understand. It opens by introducing the scientific problem, the test sites and relative geophysical parameters, the electromagnetic models used for the analysis. Then it keep on with a suitable description of the results along with a fair discussion about these achievements and the uncertainties in the process. The workflow is fine although I found some major issues in the many assumptions made due to the lack of ground data. Assumption that often are unreferred and somewhat strong. In my opinion this point must be stressed clearly both in the abstract and in the introduction, in order to provide the reader with a clear overview of what will follow. Provided this, the paper is not a breakthrough but a first attempt to understand the relationship between aquifers evolution and microwave signatures although the many assumptions made can weak the reach of the work.

Specific comments

- The main issue of the paper is the lack of ground measurements to be used both in the modelling phase and in the verification of results. This lack has led to many assumptions and has weakened the work. For instance, density and temperature profiles at site 2 are not known and assumed as site FA-13 (line 59), temperature profile and liquid water content are not known for the Antarctic test site (line 64). The aquifer's liquid water content is let range from 5% to 25% but no reference is provided (line 188, it seems that these values comes from other papers or simulations, not from ground measurements). Also, temperature profile at Wilkins Ice Shelf test site is derived from a model but no references are given (lines 213-215 and 219-223).

Thank you for the comments. In this paper, we want to provide a forward model to simulate the SMAP brightness temperature observed over the aquifer region. We have made use all of the in situ measurements that are now available over Greenland and Antarctica. For the properties in Site 2, it is not far away from the FA-13 site, thus we assume the same properties. The properties may be different but current assumption can explain the time series. The liquid water content values are from the measurement values from (Koenig et al, 2014):

Initial in situ measurements of perennial meltwater storage in the Greenland firn aquifer, GEOPHYSICAL RESEARCH LETTERS, VOL. 41, 81–85, doi:10.1002/2013GL058083, 2014. The temperature profiles and the liquid water content are derived from the paper's data. Temperature profile is from measurements, and liquid water content is calculated from measurements. For Wilkins Ice Shelf, we use the surface temperature from MODIS and relate the surface temperature and aquifer temperature with an exponential like function as has been done in (Tan 2016).

We agree that the temperature profiles used from this paper lack measurements. We tried to use the temperature profiles from geophysical model, the Community Firn Model(CFM)(Stevens et al 2020). The modeling results showed that it has a major difference between the in situ measurements from the in situ measurements we have. This may due to the fact that CFM currently cannot handle the percolation process very well.

The depletion trend of the aquifers is just assumed, and no ground truth is available but the one on April (lines 258-259 and 297) for Greenland and December for Antarctica. Given the work found that the water table level is one of the main drivers of Tb timeseries trend, the water table level must be derived in a more robust way. Maybe from a geophysical model.

We are not naming it as the depletion trend of aquifers but the position change of aquifer. Such a change can be due to many factors. We strongly agree with the reviewer that the change of water table needs a better way to characterize. The change of water table of the aquifer is a hypothesis. We will add the following in the introduction to clarify this point:

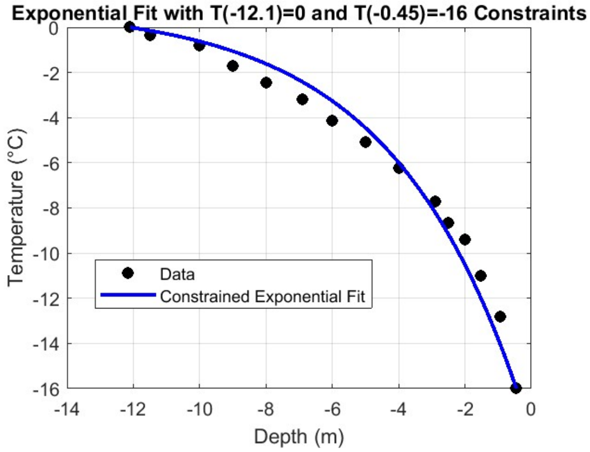
The change of water table location used in the analysis is a hypothesis.

In the conclusion, we add the following to the manuscript:

In this paper, we analyze the brightness temperature with a radiative transfer model. Liquid water and the change of water table content (thus increase total volume scattering) may drive the observed time series brightness temperature signatures. Other alternate or other drivers are also likely. The Greenland and Antarctic sites may have different drivers. More research work needs to be done.

To our knowledge, the Community Firn model is the best model for the firn available at the time. However, it is designed to model the accumulation zone. Its ability to model the percolation zone and the physical process of firn aquifer needs to be evaluated. Notice that there are no geophysical measurements that can validate the real changing of aquifer water table location at this time, it is a hypothesis that we use.

The exponential fit T=273.15-aexp(cz) of the temperature profile is shown by the following result:



The fitting difference is less than1 degree for FA-13. So we used this method to get the temperature profile of Antarctica using the surface temperature.

The snow temperature profile changes in time due to changes in water level and thermal forcing from above, no details are provided about its modelling (at line 261 is cited just a "squeezed"). For FA-13 the firn permittivity is set to a fixed value corresponding to a given liquid water content (line 279), however no references are provided to justify this geophysical value.

We are not trying to use a geophysical model to explain the physical changing process of the firn aquifer but to provide a possible explanation for the time series brightness temperature change. Again, modeling work for such a process is not available now. What we have done is based on the hypothesis that the water table position will change.

Surface temperature swing would affect the total brightness temperature observed. If we use an exponential fitted temperature profile and use a lower surface temperature in December than the temperature value used in April(-21 C), we can explain the time series using a shallower water table location. For example, if we let the surface temperature in December be 5K lower than the current case, we can still reproduce the brightness temperature observed by SMAP by moving the water table 0.5m higher as shown in the table below. We will add this result to the discussion section and state that a shallower water table location would be a possible situation when the surface temperature becomes lower.

	V	H
Use April surface temperature	238.2	212
Use 5K lower surface temperature and move water table 0.5m higher	237.8	211.7

Again, our goal is not to provide a geophysical and electromagnetic coupled model to explain the time series and draw a conclusion.

We are trying to explain the phenomenon using the hypothesis and also tries to show the brightness temperature would be sensitive to the depth of water table and the volume fraction of water in the aquifer.

The liquid water content values and temperature profiles are from the values from Koenig et al., 2014

For the second Greenland site, the assumptions are similar to FA-13 but in this case the liquid water content of the aquifer is set to 10% (line 303). No justification is provided for this value.

I apologize for the typo in table 2. FA-13 is using 20% of LWC rather than 10%. The permittivity values are the same if you look at table 2 and table 3. Since the results are based on assumptions and also it is actually in the same effective resolution of FA-13, we will delete this result from the manuscript.

For the Antarctic test site, the water table level is "adjusted as shown in figure 10" (lines 315-316).

Overall, it seems that the work relies on too many assumptions, in several cases without proper reference.

- In Fig.4 and 5 it is possible to see two temperature profiles, but nothing is said about the day of the year on which they were collected. Considering that the paper analyzes a time series of 4 months, the temperature swing in the snow/firn is appreciable and not considered.

  The temperature profile is collected in April 2013 in FA-13 (Koenig et al.,), it is interpolated to be a function of depth.

  The effects of surface temperature swing is discussed in the previous point.

- Ice permittivity models from Mätzler 1996 and Tiuri 1984 are appreciably different (line 191). More details should be provided about the use of these models.

  We use Matzler's model for the real part and Tiuri's model for the imaginary part following the previous radiometry work in Greenland and Antarctic(Tan et al 2015)

In the simulations it is not cited the inclusion of the temporal temperature swing in the upper layers that, given the shallow thickness of the snow (about 10m) can have an impact on the Tb. And maybe contribute to the "cooling" trend of SMAP Tb.

As we have calculated, the temporal surface temperature swing can have effects on the brightness temperature. The temperature itself does not show the same trend as the brightness temperature changes over the period since over the period of Dec to April, the overall trend in increasing. So, its direct contribution to the cooling is questionable. We know that the surface temperature can be one of the drivers of the lowering water table location, but the interaction is rather complicated. Again, we are not presenting a geophysical and electromagnetic coupled model to explain all the physics behind this process but to provide a possible explanation based on the hypothesis that the water table gets lower from Dec to April. We are not trying to discuss what causes the changes in water table location.

- Having the model parameterized in section 3.1, a sensitivity analysis is provided in section 3.2. However, given the many assumptions often not referenced, the representativeness of the trends found seems at least questionable.

The change of water table is a hypothesis that needs to be validated in the future work. We are providing a possible explanation to the brightness temperature change over the period of time.

At line 397 the paper says "This model eliminates the ambiguities.....where different parameters ae needed to explain ....". Actually, given the number of assumptions made, this sentence sounds too optimistic.

We want to clarify this point. The parameter difference is about the different parameter used for density variation properties and firn aquifer permittivity value. In previous work of (Miller 2016) where DMRT is used, the V and H pol brightness temperature time series show major differences of 40K using a layered medium model while the SMAP brightness temperature time series have difference around 20K. Temperature profile would not affect the difference between the V and H pol data. The layered medium model could not match the V and H pol time series data with a same set of density and liquid water content parameters. The statement of

"This model eliminates the ambiguity in the previous 1D random layering structure model, where different parameters are needed to explain the different polarized brightness temperature data"

is changed to

"This model potentially provides an explanation to the difference between V and H polarized brightness temperature data"

Minor points

- line 26, there is a red "s" in aquifers.
- line 56, here the second test site is 5km far from FA-13 while at line 298 is 6km. It is better to use a single value for coherency.
- What is the Tb reduction? It is not a common parameter and should be described in the text, not in a table (line 289).
- section 2.1. This section is redundant and the formulation can be found in many books and papers. I strongly suggest removing this section that adds nothing to the discussion and leave just some proper references.
- Figure 3 must be improved, for instance by using inset with large-scale maps, a clearer (lat, lon) grid, etc.
- section 3.1.2 seems misplaced and should be moved close to the model description.
- in line 332 the first "H pol" should be "V pol"
- the panels in figures 10 and 11 should be represented in a unique figure each to ease the comparison of the curves.
- In figure 12, the different limits of y-axes makes the comparison of the two different cases difficult. Better to merge the two panels into one.
- What "the increased water content contributes constructively to the decreased water table depth" means at line 367?
- lines 388-394 are redundant and can be shortened or deleted at all.

Thank you, we will address these issues when revising the manuscript

References:

Mapping Greenland's Firn Aquifer using L-band Microwave Radiometry,J Miller, A Bringer, KC Jezek, JT Johnson, TA Scambos... - AGU Fall Meeting Abstracts, 2016

Tan etal 2015: S. Tan *et al.*, "Physical Models of Layered Polar Firn Brightness Temperatures From 0.5 to 2 GHz," in *IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing*, vol. 8, no. 7, pp. 3681-3691, July 2015, doi: 10.1109/JSTARS.2015.2403286.

Koenig etal 2014: Initial in situ measurements of perennial meltwater storage in the Greenland firn aquifer, GEOPHYSICAL RESEARCH LETTERS, VOL. 41, 81–85, doi:10.1002/2013GL058083, 2014.

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Miller, J. Z., Long, D. G., Jezek, K. C., Johnson, J. T., Brodzik, M. J., Shuman, C. A., ... & Scambos, T. A. (2020). Brief communication: Mapping Greenland's perennial firn aquifers using enhanced-resolution L-band brightness temperature image time series. *The Cryosphere Discussions*, *2020*, 1-17.

Long, David G., Mary J. Brodzik, and Molly Hardman. "Evaluating the effective resolution of enhanced resolution SMAP brightness temperature image products." Frontiers in Remote Sensing 4 (2023): 1073765.

Stevens, C. M., Verjans, V., Lundin, J. M. D., Kahle, E. C., Horlings, A. N., Horlings, B. I., and Waddington, E. D.: The Community Firn Model (CFM) v1.0, Geosci. Model Dev., 13, 4355–4377, https://doi.org/10.5194/gmd-13-4355-2020, 2020.

Responses to Reviewer 2

This paper concerns the modelling of L-band brightness temperatures for dry firn overlying aquifers in Greenland and Antarctica. This is an application of the Huang et al (2024) permittivity model and extension of this work from backscatter modelling in the original paper to brightness temperature in this paper, and comparison of the radiative transfer model with enhanced resolution 3km SMAP data. The paper uses sensitivity studies to examine the impact on different parameters, but stops short of using the model to retrieve aquifer liquid water content. This is a potentially exciting application of this work. The paper would benefit from clarifications and further discussions to highlight the significance of this work, as detailed below.

Specific comments:

1. Representation of the dry firn layer. In this model framework, the dry firn layer, up to ~15m deep, is represented by a single layer that contains density fluctuations around the mean. Figures 4 and 5 show (as expected) vertical variation in density from 350-800 kg / m3. It would be useful to gain some insight as to how good the assumption of a single layer is. It seems some of the observed fluctuations in brightness temperatures are rather extreme e.g. at a depth of 4m in Figure 5, it looks like there may be an ice lens between two lower density layers of around 500 kg / m3. Existing multi-layer radiative transfer theory models (SMRT, LS-MEMLS, DMRT) could be used to test the impact of these layers compared with a single layer approach and thereby demonstrate whether the experimental set-up here is appropriate.

   Thank you for the comments. I would like to clarify that our solution to the radiative transfer function is not using a single layer with uniform properties but is using a layer that has varying properties along the depth. We are assuming a constant density fluctuation parameter because this is the simplest case that can fit to the SMAP data. The scattering properties due to the variation is actually changing along the depth since that the mean density is changing with depth.

   In the multi-layer radiative transfer theory models(SMRT, LS-MEMLS and DMRT), the phase matrices are considering the scattering due to the snow particles, the scattering is significant at higher frequencies such as C, X and Ku bands. Also, these models are considering a homogeneous horizontal layer. At lower frequencies such as L band, the major effects affecting thermal emission would be the change in permittivity, which is a function of density.

   Different from the multi-layer models, we include not only the variations vertically but also the horizontal changes. The phase matrix in our radiative transfer model is obtained by Born's approximation using the statistical properties of inhomogeneous permittivity profile. The phase matrix not only depends on the amplitude of variation but also the mean value. This indicates that although the density variation is the same, change in the background medium would also affect the phase matrix properties. In the formulation, the parameters in the radiative transfer equations change with respect to depth z this is why the final expression is so complicated with integration with z.

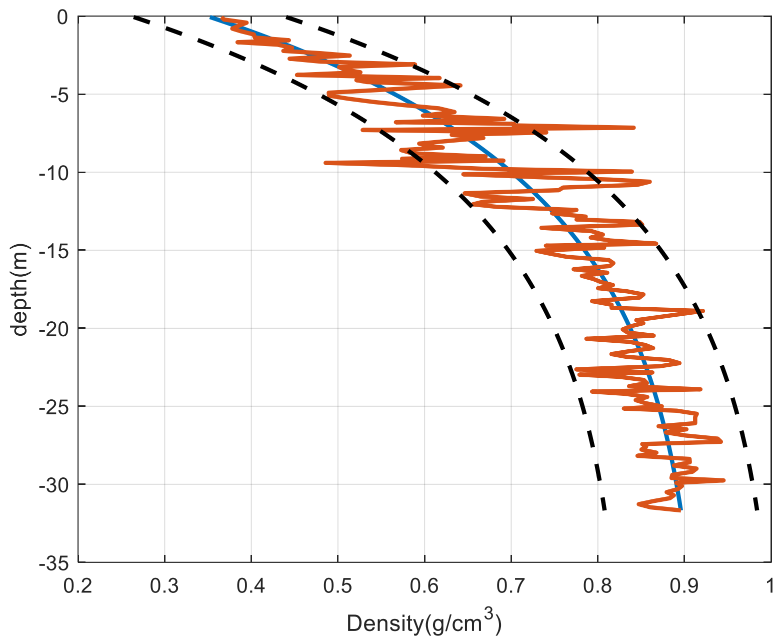
We agree that there might be ice lens at that depth. But the borehole measurement is a measured profile from the area, which is just a single realization of the density variation. While on the other hand, radiometer is measuring the overall effects within the resolution cell, that is actually many realizations. Radiative transfer equation is handling the averaged results from many realizations. Thus, we are treating the ice lens as part of the fluctuation profile.

If we calculate the effect of the ice lens in the radiative transfer model, we are assuming such a high-density layer extended across the resolution cell of the radiometer.

2. Fitted versus physically-derived parameters. It would be useful to highlight which of the parameters applied in the model have been derived directly from the measurements and which have been fitted to the SMAP observations. As far as I understand, the mean densities, temperature profiles and aquifer depths were from observations, whereas the density fluctuations, liquid water content, and vertical and horizontal correlation length were all fitted to the SMAP observations – is this correct? I would recommend parameters for all sites (Tables 3, 4, 5) be combined into a single table, and highlight (perhaps in bold) which ones were directly from the observations. It would be useful to overlay the mean density and a shaded region for fluctuations on to the density profiles to demonstrate how representative the model is.

   Yes, your understanding is correct. The density variation parameters are fitting values for all 3 cases. Liquid water content for Greenland is derived from in situ measurements. LWC for Wilkins ice shelf is a fitting parameter. The temperature profile for Wilkins Ice shelf is derived from an exponential like function (Tan etal 2015) ith the constrains at the surface and water table. Water table is 0C, surface is from MODIS temperature data. Water table location is also a fitting value for the time series except for the one in April. The values in April are from Accumulation radar measurements from

We will include a table to show all the fitting parameters used in the model when revising the manuscript. The figure of density is shown below, we use 2 dash lines to represent the region where the density is off from the mean by 1 std.



3. Difference in parameters between two Greenland sites. These two sites are 5-6km apart (different values given in different parts of the paper), leading to the assumption that some of the parameters (density, temperature) are transferable. However, the aquifers have different liquid water assumptions and consequently effective permittivity. What is the reason for the 10% difference? The SMAP data presented show rather different signatures, but are likely adjacent or near adjacent pixels within the same radiometer footprint. A scatterplot comparison between the two may be informative - what in the footprint is causing the difference in TB, given that it's likely coming from the radar? Is there something specific about the topography that could cause differences in the aquifer water content?

   Thank you for the comments. We were previously thinking that the two sites are different. Based on the findings in (Long etal 2023), they are in the same effective resolution. It is hard to know what is causing the difference in TB. We would delete this simulation results from the paper given the confusions caused by this result.

4. Aquifer liquid water assumption. How were the values used derived? Are they realistic for the regions?

   The values used in Greenland is derived from Koenig et al., 2014.

The value in Wilkins Ice shelf is a fitting parameter that can explain the time series data. We believe the values are realistic as they are calculated from insitu measurements.

Spatial extent of application. This model assumes dry firn above aquifers, which may form under 'certain conditions' (line 24). What are the conditions that cause this situation and how frequently / on what scale do they occur. This is important to understand how significant this problem is. Although potential contribution to sea level rise is given as 0.4mm for total aquifer drainage, it would be useful to understand whether this a few large aquifers or many small ones.

Previous researchers have performed the study on the spatial distribution of firn aquifers using SMAP radiometer data, please refer to the work: (Miller etal 2023, miller etal 2022, miller etal 2020)"Mapping Firn Saturation Over Greenland Using NASA's Soil Moisture Active Passive Satellite" J.Z. Miller JSTARS 2020. The conditions for the firn aquifers to form is still unclear, but classification through time series signature is possible.

5. Generalization of modelling. Borehole / GPR were used to parameterize the model – how could modelling be approached without these data i.e. extended to other locations? This also ties in with point 10 below: what would you need to know to be able to do the aquifer liquid water retrievals.

   This point would be answered together with point 10.

6. Credit to earlier work. In particular, this work leverages earlier permittivity work by Huang et al. (2024). Figures 2 and 6 in this paper are rather similar to figures in the Huang paper, which is understandable, but please be explicit about how the work in this paper builds on the work of Huang, particularly placing into context here the treatment of effective permittivity, which is studied much more comprehensively in Huang. There are also numerous other radiative transfer models available – please be explicit about how the one used here is different, and why it is used rather than any other. I would really like to have seen the impact of different permittivity model assumptions on TB – this would be helpful to the community and would again help demonstrate the impact of this work.

   Part 1: For the radiative transfer models. We are using a new radiative transfer model that (2) treats the dry firn above firn aquifer as a layer with inhomogeneous permittivity values due to the density variations. (2) The random variations of density profile (permittivity) is not only vertically changing but also horizontally changing. The horizontally changing effect is not included in the multi-layer versions of radiative transfer models. (3) This model is a low frequency model as the scattering from snow particles are not evaluated. The scattering is mainly because of the permittivity inhomogeneity. We have used this model to explain the SMOS angular and polarization dependence observed over Dome-C Antarctica.

Part 2: For the permittivity values from different models. Here the values are listed comparing the shallow water table case and deep-water table case:

	Needle $\epsilon_r = 10 + 0.8i$ TB(K)		Bic-model $\epsilon_r = 7.6 + 0.25i$ TB(K)		Spheres $\epsilon_r = 5.2 + 0.02i$ TB(K)	
	V	H	V	H	V	H
Water table at z=-6	227	196.1	231	200	236.8	204.9
Water table at z=-12.5	215.6	186.1	217.3	187.4	218.5	189

The reason for using this model is because this model is similar to the real physical case of the firn structure.

7. Apparent sensitivity to aquifer wetness in Figure 10. Please explain the physical mechanism for the decrease in TB with increasing wetness here. This is counterintuitive to the increase in permittivity to near blackbody behavior for wet snow.

   Physically speaking, in the simulation the temperature of firn aquifer is fixed at 0 degree. The change of wetness would affect the permittivity of the aquifer, as wetness increases, permittivity increases. Emissivity from such a medium would decrease as e=(1-r), where r=|R|^2 is the reflectivity and R is the Fresnel reflection coefficient. R is a function of permittivity values across the boundary. When the ice permittivity does not change, the increase permittivity of aquifer would increase the reflectivity and reduce the emissivity. This means the total emission from aquifer is reduced.

   The wet snow phenomenon can be explained by the 2 layer model in radiative transfer theory. Different from the aquifer case, the warm wet layer is above the cold dry layer. When wetness increases, with the increase of permittivity, the absorption of wet snow layer increases, which means it blocks the emission from the colder dry snow layer below it. Although the emissivity at the top boundary is decreased, the increase of the imaginary part of the wet snow makes the emission from this wet warm layer contributes more than the reduction. The radiometer tends to see more contribution from the relatively warmer wet snow due to the increased absorption.

8. Temporal resolution of simulations. Five data points are presented for each site, to represent the seasonal evolution over a year. What are the temporal resolution of

observations available for the in situ data and how were these particular points chosen? Is it possible to include other data points? It is not clear how this varied in time, and how the aquifer depth was inferred from these observations.

We want to clarify that the temporal change of firn aquifer is not available from in situ measurements, so depth is a fitting parameter. FA-15 has this kind of information but it is not applicable to our model since a wet firn layer appears between the dry firn and aquifer. The time points are basically chosen one from each month. I think to include more data points, we would need to assume a decreasing rate of the water table, starting from the summer when the surface is wet.

The point we want make is that the brightness time series change can be explained by the change of firn aquifer water table solely or together with the aquifer liquid water content change as shown in the sensitivity analysis. The change of water table location indicates the total amount of change in the volume scattering. The lower the water table, the lower total amount of volume This is one of the possible explanations for the brightness temperature change.

9. Aquifer liquid water content retrievals. As the application of this paper is for aquifer liquid water content retrievals, it would be hugely beneficial to attempt this in the paper, and use the sensitivity study to indicate how well the parameters need to be known. In lieu of this, please could you suggest a methodology for liquid water retrieval, particularly how some of these unmeasured parameters may be estimated. As it stands, this paper does not support the last sentence in the abstract.

Thanks for the suggestion. We cannot retrieve the total amount of liquid water content but can get some information of the volume fraction of water in the aquifer. Actually, the next stage of the work would be the retrieval of liquid water content of the aquifer. However, I could not proceed due to funding issue. I would like to continue the work when new funding comes in.

Here is a way I think I can make use of to retrieve the liquid water content of the firn aquifer. (1) We will use the Sum-up data set for the density information that overlaps with the detected aquifer region from SMAP (2) For the temperature profile, we would use a fitting model $T(z) = 273 - C\exp(Dz)$ with the surface temperature from MODIS measurements and the water table .(3) We would use the brightness temperature from the nearby percolation facet(where no aquifer or ice slab exist) to bracket the density variation properties of the dry firn. (4) We create a cost function to retrieve the water content of aquifer and water table depth at the same time, based on the SMAP brightness temperature.

10. Structure of material. Site data from other studies should be in the methodology section, along with the map of the sites. These are other people's work with no new analysis in this study.

    We will move this part to the methodology section with the map of the sites in the manuscript when revising the paper.

11. Code and data availability. The authors are strongly encouraged to make the code publicly available. 'Code available upon request' does not conform to FAIR principles and has meant that it is much more difficult to interpret how the authors have undertaken their research. 'Upon request' is more or less unjustifiable and incompatible with how research is conducted in present times. Links download the data need to be provided in this section.

    Thank you for the suggestion. The code has been uploaded to the author's personal github website. Links to the data are also provided: https://github.com/Jokerleonxv/firn-aquifer

    We will add the links to the data used in this paper when revising the manuscript.

Technical comments:

Line 37: 'logistic-like': should this be logarithmic?

Line 44: Explain the polarization signal that is not captured by a single layer model, and why the single layer model here is different.

Section 2.1 A figure showing the geometry and nomenclature would be useful here.

Figure 3: needs to be in the context of a larger map (and in the methodology)

Figure 5: Show water table location

Line 215. Explain how this fitting was done.

Line 216. Figure 6 does not show this.

Line 258. Where does this assumption come from? Is it reasonable?

Line 260. Explain the mechanism for squeezing the temperature profile.

Line 261. Why tune the density fluctuations? Why not use the observations?

Table 2. Explain what is meant by 'TB reductions' i.e how these are calculated. How are boundary effects separated, given multiple scattering?

Figure 8, caption. How and why is the temperature gradient changed 'to have a slower changing speed' – please clarify what this means.

Line 288. 'As indicated in…' – please rephrase to clarify what this sentence means.

Line 314. 'higher aquifer water content' in Wilkins is inconsistent with values indicated in simulation parameter tables.

Line 383. The vertical correlation length lz controls the layer-like behavior, not the horizontal.

Thank you, we will address these issues when revising the manuscript

References:

Mapping Greenland's Firn Aquifer using L-band Microwave Radiometry,J Miller, A Bringer, KC Jezek, JT Johnson, TA Scambos... - AGU Fall Meeting Abstracts, 2016

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