

Response to Reviewer 1

In this second version, some aspects are better explained e.g. modelling approach. The model itself is interesting, as is the application to firn aquifers (particularly evaluation against SMAP data). However, several comments from the earlier review have not been taken into account, and some additions are not clear. Further questions have also arisen after clarification of the methodology.

Thank you for reviewing the revised manuscript. We really appreciate your effort. We shall address the questions in this round.

We want to make a clarification first. In this paper, we are not trying to justify or falsify the hypothesis of “water table position lowers as spring comes”. The major contribution of this paper is that this paper is the first modeling work that can correctly simulate the . We use the hypothesis together with our model to explain the time series of the brightness temperature. This is a possible explanation to the phenomenon.

We add the following sentence to the introduction:” **Such a large difference in the V and H channel would make the parameters used to explain the data much different from each other, hence introduce uncertainties in retrieving properties of firn aquifer. ”**

We revise the lines in the introduction “In this paper, a radiative transfer model based on 3-dimensional firn profiles [Tsang et al. 2001, Xu et al. 2023] and an effective permittivity of the aquifer[Huang et al. 2024], predicted from a full wave approach is used to explain brightness temperature time series and their polarization dependence. Changing of the water table location is a hypothesis used in this work.” Into “Noticing the limitations of the current models, this paper aims to provide a radiative transfer model that can explain the V and H pol brightness temperature with a same set of parameters. Unlike previous approaches, which cannot match the observed V- and H-polarized brightness temperatures concurrently, the proposed model achieves polarization-consistent simulations, thereby eliminating a major source of uncertainty in the future retrieval works. In this model, Three-Dimensional random media is used to describe the random fluctuations of the dry firn density[Tsang et al. 2001, Xu et al. 2023] and the phase matrix used in the radiative transfer model is developed based on this description. Solution based on this model is provided. Solution based on this model is provided. Time series simulation is also performed based on the hypothesis of decreasing water table depth starting from the end of summer. Such a hypothesis comes from the observation over land glacier firn aquifer [Christianson et al. 2015]. Simulation of the time series is not to justify or falsify the hypothesis but to show the ability of the new model and provide a possible explanation to the brightness temperature time series. ”

We add the following reference:

Christianson, K., Kohler, J., Alley, R. B., Nuth, C. and vanPelt, W. J. J. (2015), Dynamic perennial firn aquifer on an Arctic glacier. *Geophys. Res. Lett.*, 42: 1418–1426.
doi: 10.1002/2014GL062806.

Also add:” We use the results from [Huang et al. 2024] for the values as the aquifer is a complex structure of ice-water mixture.”

The following is also added to the manuscript:

” The radiative transfer model in this work tries to provide a tool that can provide simulations to the observed brightness temperature and help with the retrieval of the firm aquifer properties in the future work. Some of the inputs are tuned empirically(e.g. density fluctuation parameters) which are not the goals for retrieval.”

General comments:

A table is needed to summarize parameters used, state whether these are derived from observations or assumptions and the source. This was requested in the first review.

Sorry for missing this questions. The table is provided below to summarized the parameters used in the simulations for Greenland and Antarctica. And is inserted into the manuscript.

parameter \ type	Measurement	Fitting
Mean density	√	
Temperature	√	
Water table depth (April)	√	
Water table depth (other)		√
Standard deviation of density		√
Vertical correlation length		√
Horizontal correlation length		√
Liquid water content	√ (derived from pore space in Koenig etal 2013)	

Table 1 summary for the parameters used for forward simulation of time series at FA-13

parameter \ type	Measurement	Fitting
Mean density	√	
Temperature	√ (derived from MODIS surface temperature and water table location)	
Water table depth (Dec)	√	

Water table depth (other)		√
Standard deviation of density		√
Vertical correlation length		√
Horizontal correlation length		√
Liquid water content		√

Table 2 summary for the parameters used for forward simulation of time series at Wilkins Ice Shelf

Better connection with the physics is needed. In the sensitivity studies the water content is varied: as the permittivity calculation assumes only water inclusions in an ice background i.e. the medium is fully saturated with no air, more water implies less ice. Any discussion of temporal changes (lowering of water table and increase / decrease of water content) needs to be accompanied by a consideration of physical realism. Or is the discussion here related to natural spatial variability? Some context as to how this relates to interpreting SMAP observations is needed, and it may be better to frame it as a 10K reduction in brightness temperature between observations may be due to x, y or z.

The different parameters used in the sensitivity analysis is to discuss the possible cases in the spatial variability.

We add the following: “This decrease is majorly due to the increased dry firn volume and thus the increased volume scattering. As water table becomes deeper, the permittivity difference between dry firn and aquifer gets smaller. This would increase the emissivity from aquifer. However, such an increase is overwhelmed by the increase of dry firn volume as indicated by the decreasing trend of brightness temperature.”

We add the following sentences into the related paragraphs: “The first case where water content decreases assumes that the aquifer is very wet after the melt season, then it dries up as the low temperature at the surface gradually taking heat out of the firn. The second case where water content increases as the water table lowers assumes that water gradually accumulates into a deeper position. Noticing that the temporal changes of the firn aquifer are only assumptions, their correctness would need to be justified or falsified by future direct measurements of the firn aquifer.” And add a sentence “ This implies that the decreasing of SMAP brightness temperature is a complex process, decreased water table location and changes in the firn aquifer liquid water content can both contribute to such changes. ”

Hypothesis: ‘Changing of the water table’ is not a hypothesis, but is in the direction of a good one. Rephrase this into a clearly-stated, testable hypothesis and prove / disprove it: this will make a very strong paper. As it stands it’s not clear whether the paper is a model validation paper, a

process understanding paper, a paper to quantify spatial variability in firn properties or gearing towards satellite retrievals of firn water content.

Thank you for this suggestion. However, the goal of this paper is not to justify or falsify the hypothesis of the changing water table. The major contribution is to provide a model that can simulate the V and H brightness temperature and give a possible explanation to the change of brightness temperature. We have made changes to the conclusion:

We delete the lines of “*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. Greenland and Antarctic sites may have different drivers. More research work needs to be done.*” And add: “**The SMAP brightness temperature time series is also modeled by the radiative transfer model with the hypothesis of a decreasing water table from December to April. The hypothesis itself needs to be further validated by the direct observations of water table location over the seasons.** “

Specific comments (line numbers relate to non-tracked change version):

- Line 38. ‘Logistic-like’ - what does this mean (as per previous review)

A better word would be “sigmoid function”. This behavior is used by Miller 2020 to describe the shape of the temporal changes of the SMAP brightness temperature.

The brightness temperature is first normalized according to (3) in Miller 2020:

$$T_{B,N}(t) = \frac{T_B(t) - T_{B_{\min}}}{T_{B_{\max}} - T_{B_{\min}}},$$

They then find that the sigmoid function in the form of :

$$T_{B,N}(t \in [t_{sfu}, t_{mo}]) = \frac{1}{1 + \left(\frac{1}{T_{B,N}(t_{sfu})} - 1 \right) e^{-\zeta t}}.$$

Can fit the transformed SMAP brightness temperatures. In the above expression, t_{sfu} is the surface fully frozen time, t_{mo} is the melt onsite time.

- Line 48. What is meant by ‘coupling between V and H...’ Also line 347.

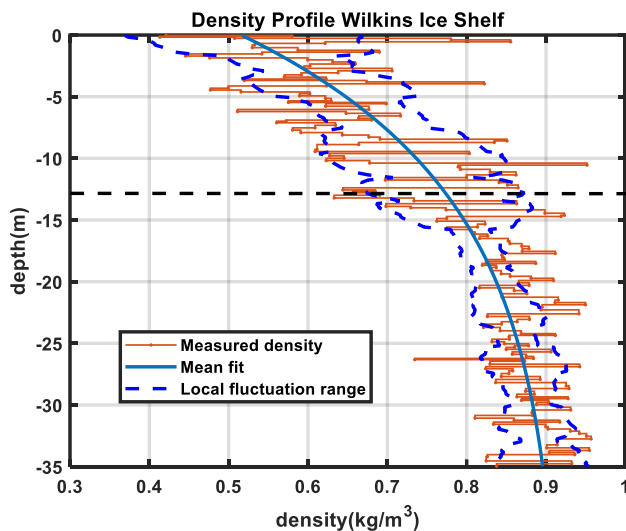
This is an electromagnetic wave scattering concept. In the layered medium structure, V or H polarized EM wave would be reflected/transmitted at the flat boundaries. The V polarized wave

and H polarized wave would only reflect or transmit according to their own rules. If we look into the cross pol component of the phase matrix in such a kind of media, their values are zero, because flat surface would not make a V-polarized wave into H polar or H polarized wave to V-polarized wave. However, due to the introduction of 3D variations, phase matrix in the radiative transfer has non-zero cross pol components. Such non-zero values would let V-pol wave become H-wave and vis versa. That's why we call it "coupling between V and H".

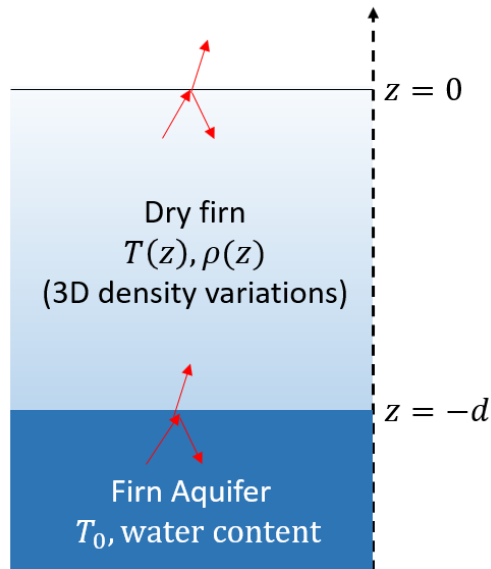
- Figure 1. Add colour bar to show limits of density. This should reflect the general densification to the base. Caption: 'blue's' -> 'blues'

In this figure, we are not trying to strictly show how the density changes. We are just providing a figure to show the regions in this passive microwave problem we will solve using the radiative transfer equations. The regions are air, dry firm and aquifer from top to bottom. The color is only to show how the 3D variation looks like, the color itself is not important.

Also, from the density measurements in figure 4 and 5, since this is a percolation zone, the density could be increasing from surface to the water table or keep at a constant value. The density fluctuations gradually lowers at a deeper region below the water table(see the Density profile form Wilkins Ice Shelf).



To avoid such confusion, we would use the following figure instead, to better show how the regions are assigned. The new figure is more focused on the regions.



The caption:

“A two-layer model for the firn aquifer region in the early spring before surface melting starts. Properties of dry firn change with depth. Three-dimensional varying profiles characterize the firn region. Deep blue’s means relatively lower density while yellow means relatively higher density. The aquifer is characterized as a mixture of ice and water. Due to the permittivity contrast of dry firn and aquifer, the interface is a reflective boundary.” is changed into **“A two-layer model for the firn aquifer emission problem in the early spring before the surface melt starts. The top region is air($z > 0$), middle region is dry firn with changing properties. The density fluctuation in this region is considered as 3 dimensional. The lower part is firn aquifer with a constant temperature of $T_0 = 273.15$.”**

- Line 89. Explain the rationale for exponential vertical correlation but Gaussian horizontal correlation: how does this relate to observations / physics?

We are following the tradition in the random media theory for choosing the random process as in Tsang 1978, Tsang 1979. In such studies, we would not claim what we are assuming in the process in always the ‘truth’ but a theoretical tool helps to explain the observations, just like researchers do in the surface scattering problems.

The observation is that realization of Gaussian processes would provide smooth random profiles while exponential process would provide ‘high frequency’ changes in the profile. In observing the high resolution density profiles, we believe the exponential is suitable for vertical changes.(refer to the figures below). Horizontal variations, as it means, tries to include the effects of horizontal inhomogeneities. This can be due to the effects of melt or or other temporal effects. We would expect such changes are ‘low frequency’ . Also, we noticed the presence of ice structures, we want

to use such a simplified model to include the effects of such structures in stead of using many other parameters(such as, size, density, orization). This is preferred because if we choose the detailed description, the number of observations would be less then the parameters that need to be retrieved in the retrieving work later.

- Lines 150-163. Demonstrate where the real part of the permittivity appears in the equations. Similarly for the bicontinuous media parameters: how do these influence the permittivity?

I would recommend the reviewer to find the details in Huang 2024 where we show the details of the approach. The real part of the media and also the structure parameters of the bicontinuous media cannot be explicitly shown in those equations or expressed with analytical expressions. This is because we are using Computational Electromagnetic Methods(CEM) to obtain the wave behavior of such a complicated media. Basically, the real part of the permittivity and the structure parameters are influencing the scattering amplitude $f(\theta, \phi)$ outside the medium and the mean field inside the medium. The advantage of such an approach is that CEM would provide accurate wave solutions than the analytical mixing formulars, which would need to assume shapes of the inclusions, approach in Huang 2024 would not need such requirements.

*- Line 170. Formatting of section title header. In this section, I would recommend using subheadings for the two sites and restructuring accordingly. As written, it is hard work to continually switch between the two sites. E.g. line 190 'temperature profiles are obtained by the following steps' – this is only applicable to Wilkins, but the following sentence relates to FA-13, and I think is a description of the observations i.e. no steps followed.
- (in lots of places: cross-referencing has not been done properly, with lots of Error messages)*

Thank you for the suggestions, the subheadings are added and also the cross references are fixed.

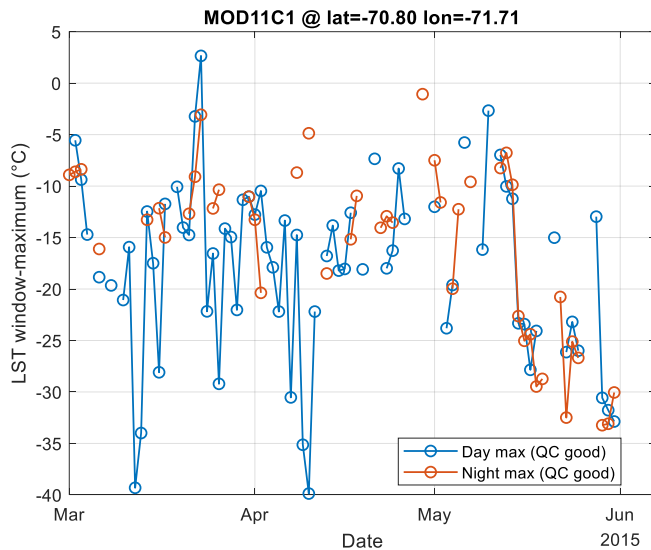
- Line 179. Need to reference Figure 18 here?

Cross reference to the Figure 1B(new numbering of Figure 18.)

- Line 194. Where does the assumption of mean surface temperature come from and is it realistic for Antarctica? What is the MODIS-derived mean surface temperature?

The location we are looking at is on the Antarctic Peninsula and it is an ice shelf. The location is very close to ocean such that a warm temperature in May is expected. The surface temperature in mid May of 2015 is high as shown in the figure below, this is from the nearest valid data of MODIS, which is further from the ocean. Thus we use a little higher temperature of -5 degree. This is what we can do for now as in situ measurements are not available. Given the fluctuating nature of the surface temperature, and compare to what is seen by the measurement in Koenig etal 2013, the

subsurface temperature is more stable after some depth. We can of course choose a lower surface temperature and obtain another temperature profile to run the simulation, but doing so would only result in a different set of parameters used in the forward simulation. As mentioned before, we are not trying to provide a comprehensive study of the L-band brightness temperature over the aquifer region combining the EM and geophysical models, which is too ambitious given the geoscience picture known and the ability of the current geophysical models. We are only providing a better brightness temperature model and doing a forward simulation of brightness temperature with a hypothesis.



We revise “*Assume that the mean surface temperature is at -5°C and the temperature profile follows an exponential-like function as $T(z) = 273.15 - 5\exp(Cz)$. The C value can be obtained by forcing $T(z = -13.5) - 273.15 < 10^{-3}$. The results show that setting b , being one-fifth of the water table depth, which is $b=2.65\text{m}$, would fit the need. The surface temperature in the forward simulation will be obtained from MODIS surface temperature product rather than assuming -5°C .*” Into “**The surface temperature of Wilkins Ice Shelf borehole location is chosen as -5°C given that the nearest valid surface temperature is in the range of -3 to -10°C in mid May. temperature profile follows an exponential-like function as $T(z) = 273.15 - 5\exp(Cz)$. The C value can be obtained by forcing $T(z = -13.5) - 273.15 < 10^{-3}$. The results show that setting b , being one-fifth of the water table depth, which is $b=2.65\text{m}$, would fit the need. Given that the subsurface temperature is more stable as observed from FA-13[Koenig etal 2013], a better approach to estimate the temperature profile is needed in the future retrieval work.**”

- Lines 193-199 be consistent in the nomenclature. ‘C’ is used twice for two different things. ‘b’ is used where ‘C’ is used previously.

Thank you for pointing this out. We have corrected this typo.

- Lines 201-203. *It's not easy to see the step change in density in figure 4/5. Please could you be more precise as to what to look at? I cannot see the critical density of 0.55 g/cm³ about 15m from the surface.*

There is a typo, it should be 5m below the surface rather than 15m. We want to show the mean density profile I obtained from the measurements and it will be used in later simulations.

- Line 208 *'positions of FA-13..' -> 'positions of the water table in FA-13...'*

Thank you for pointing out. We have revised accordingly.

- Line 213. *Reference to section 2.2 here: this section does not discuss the role of spheres / needles / bicontinuous (relates to earlier comments about use of these parameters)*

To make it more clear, we add the following sentence to section 2.2:

“The major reason for doing this is due to the limited kinds of shapes can be applied in the classical mixing formulas. In such formulas, the inclusions, for aquifer case, water, have to be assumed to be either needles or spheres such that effective permittivity can be evaluated. However, these shapes could not represent the complex structure of firn and it is shown later in **Error! Reference source not found. that the shapes are important in obtaining the effective permittivity. ”**

- Line 293-240 *' Density fluctuations used as tuning parameters' – how was this done, given the three parameters available?*

The following is added to the manuscript:” **The variance of density and vertical correlation length is first tuned such that the V pol brightness temperature is close to the observations. Then the horizontal correlation length is tuned to adjust the difference between V and H pol. Although the properties of density fluctuation can be obtained from the measured profile, tuning parameters are used instead. This is because the density measurement is usually performed over the drilled ice core samples. Such samples are usually tens of centimeters long and measuring the density of the sample would unavoidably smooth out the density fluctuations and suppress the spatial variations within the sample . ”**

- Table 2. *The concept of TB reduction i.e. relative to 273K is confusing. Remove from this table, and throughout the text: describing the impact on TB is much more intuitive.*

Thank you for the suggestion. I have made the changes accordingly. I have removed this section from the table

- Table 2. How was the time variation in firn permittivity determined? Is this from fitting, or from an assumption that the firn aquifer is higher earlier i.e. only the top 6m density profile is used in December, while keeping density variation parameters constant?

We are using the same profile and assume the firn aquifer is at a higher location in December while a lower location in April. Currently, we are assuming the density variation parameters are the same, we can of course change it to depth dependent but we want to keep the number of parameters as small as possible.

- Line 255. There is some missing text here at the start of the sentence

There is no missing text here. It follows Line 242 "...with vertical and horizontal correlation length...". The "." Is removed at the end of Line 242

- Line 257. State where the assumption of 20% liquid water comes from. Is this from a fully saturated lower layer in April? Does this mean the application of this firn aquifer permittivity in December implies a higher liquid water content as the density is lower at 6m depth than at 15m?

The assumption comes from the measurement in Koenig et al 2013. It is assumed as a fully saturated layer. For the case where the water table is assumed at 6m below surface, there is no direct observations about what the physical picture is. We are not implying anything but to provide a controlled simulation and show how the water table depth along affect the brightness temperatures. Effects of the water table depth and water content in aquifer is discussed in the later analysis.

- Line 283. 'Assuming the changes are purely due to the changes in the water table'. Given the number of different effects here, could this be tested via a much simplified model set up (non-random density fluctuations, perhaps even constant density). This relates to the sensitivity study too – there are too many different variables to fully isolate any of these effects. This study could be restructured to start with a simplified sensitivity study, then demonstrate the combined effects with the density fluctuations, followed by evaluation against the SMAP observations, and finally a discussion considering whether the hypothesis has been proven or not.

We want to clarify that we are not trying to justify the hypothesis of the changing water table. The major contribution of this paper is that this is the first theoretical work that can provide brightness temperature modeling results that can explain the V and H brightness. With the given hypothesis, the model can provide results that are consistent with the measurements. Whether or not the hypothesis is true or not is beyond the scope of this paper and at the current stage, we cannot justify or falsify this hypothesis. In the conclusion, we change the last sentence " *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. Greenland and Antarctic sites may have different drivers. More research work needs to be done.” Into

“The SMAP brightness temperature time series is also modeled by the radiative transfer model with the hypothesis of a decreasing water table from December to April. The hypothesis itself needs to be further validated by the direct observations of water table location over the seasons. “

- *Figure 9: font is too small.*

We have make the words larger and more visible.

- *Line 295-298 Should be joined with paragraph in line 301*

We have let the paragraphs join together.

- *Figure 10. X-label is incorrect.*

The X-label is corrected to “Depth of Water Table(m)”

- *Line 333. This sentence belongs with the next paragraph*

This sentence is moved to the next paragraph.

- *Line 343. ‘As shown in ...Figure 14’: neither graph reflects the increase in std(rho).. Figure 14 does not show anything stated in this sentence.*

The cross references are corrected. Thank you for pointing out.

- *Line 360-362. I do not understand the point being made in this sentence – please rephrase.*

The sentence: *”If the liquid water content in the aquifer has a decreasing trend, the brightness temperature from December to April is much less than the case when the liquid water content increases with time and the change in Figure 7.”*

Is changed into *“Comparing the total brightness temperature reduction from December to April, if the liquid water content decreases as the water table lowers, the reduction is smaller than the case when liquid water content increases as the water table lowers ”*

- Figure 15. This is not adding much to the discussion – perhaps this figure can be moved to the appendix.

Thank you for this suggestion. We have moved the 2 figures into appendix.

- Lines 385-387. This does not add much to the discussion as it stands. Either delete or expand to be specific: what characteristic, how, and what would the purpose of the study be (why is this a research gap?)

The sentence” *As discussed earlier in the paper, the variation properties of the dry firn affect the emission. These properties need to be characterized. A possible approach is to use the data from higher frequency microwave sensors, such as the AMSR-2 multi-frequency radiometer data at C, X, Ku, and even Ka Bands.*” is revised as:” **As discussed earlier in the paper, the variation properties of the dry firn affect the emission. These properties would better be characterized before retrieving the properties related to the firn aquifer(e.g. water table depth, liquid water content). As higher frequency microwaves are more sensitive to the near surface properties, data from AMSR-2 multi-frequency radiometer(C,X,Ku and Ka) would potentially help with achieving such goals.**”

- Lines 389-394. These are new results and don't belong in the discussion. They should be moved to the results section, perhaps in a subsection of their own.
- However, these do illustrate the robustness / equifinality of the results: important to consider to answer the hypothesis.

Thank you for your suggestion. I have moved such results to the results section with subsection ”effects of temperature perturbation at top.”

- Lines 403-405. ‘This is because the permittivity...’ is not a robust conclusion as this study has not separated the effects of permittivity difference and scattering from deeper snow. A simplified sensitivity study could do this.

Thank you for your comments. We delete this sentence in the manuscript and add a sentence:”**This is shown in figure 10 by the reduced sensitivity to liquid water content as water table becomes deeper : The dynamic range reduces from 8K to 3K as water table decrease from 6m to 12.5m.**”

We also performed a study for non-scattering case, only changing the location of water table. This does show the reduction in water content sensitivity, however not as significant as shown in figure 10. This is due to the increase optical depth due to scattering reduces the sensitivity. We are not going to include these results in the manuscript.

- Lines 411-415. I would recommend reworking these after presenting a hypothesis: currently these

sentences are weak as they are non-specific and does the contents of the paper a disservice. Expand on alternate drivers, the difference between Greenland and Antarctic sites (in the discussion) and what further research is needed.

Thank you for your suggestion. The word “driver” is not appropriate. We are trying to say the increased volume scattering could explain the decreased brightness. It is a EM model perspective than a geoscience perspective. We are not trying to validate or dis validate the hypothesis, but to show our model can explain the SMAP TB time series with such a hypothesis.

We delete the lines of *“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. Greenland and Antarctic sites may have different drivers. More research work needs to be done.”* And add: *“The SMAP brightness temperature time series is also modeled by the radiative transfer model with the hypothesis of a decreasing water table from December to April. The hypothesis itself needs to be further validated by the direct observations of water table location over the seasons. “*

Response to Reviewer 2

My only observation is about the sentence " In the dry firn, the density profile is converted into the permittivity profile from the work of Matzler 1996 and Tiuri 1984." which is not clear at all and they have to report the reply to my comment "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)". Indeed, the use of Tiuri or Matzler model can have a strong impact on the simulations (e.g. see <https://doi.org/10.1016/j.rse.2016.02.037>) and mixing two models can be a bit arbitrary although done in previous papers.

Thank you for mentioning this point. The permittivity of firn and ice is a major parameter that affect the thermal emission. Currently, the Cryorad team(leading by Giovanni Macelloni) is measuring the permittivity of ice and want to eliminate this uncertainty. The current strategy has proven to be useful through the UWBRAD project. The Cryorad team would provide a conclusion for this problem soon.

Responses to Editor's comments

The referees have concluded their review and are generally positive about the changes that have been implemented. However, some important concerns remain, especially for referee 1. Please address these in an updated version of your manuscript.

Additionally, I would like to ask you stress the "empirical" nature of the work more (e.g., in the discussion section). It is a common practice that the data used in modelling are the most coherent as possible in order to have a faithful representation of the electromagnetic process, which is not the case here (for good reasons).

Also, please address the following point, which were noticed during the review file validation:

1. A "Short summary" system section contains scientific abbreviations. Please be aware that scientific abbreviations (excluding chemical elements) must have their full written explanations. However, do not forget that there is a limit to characters (not words!) for "Short summary": it must be < 500 characters.

2. Regarding the section "Author contributions", we kindly ask you to use rather initials of the authors instead of full names in this section with the next revision.

3. With the next file upload request, please number the figures in the Appendix as per our guidelines and adjust the references in the main text accordingly: <https://www.the-cryosphere.net/submission.html#manuscriptcomposition> => Appendices

Thank you for the comments. I have changed the author names, appendices. The short summary now is <500 characters. Thank you

We add the following sentence to the discussion : ” **The radiative transfer model in this work tries to provide a tool that can simulate the observed brightness temperature and help with the retrieval of the firm aquifer properties in the future work. Some of the inputs are tuned empirically(e.g. density fluctuation parameters) either because the measurements are not available or the parameters are not provided on the microwave scale. These parameters will be used as empirical inputs to serve the retrieval of firm aquifer properties.**”