The authors would like to thank the reviewer for their comments and feedback. Our responses are presented in blue.

Reviewer #1

Synopsis:

In this paper, the sensitivity of microwave brightness temperature to snow liquid water content at different frequencies from L-band to Ka-band has been investigated. Considering a recent interest in multi-frequency snow monitoring, authors have done a good job in drafting this paper. However, there are some issues that need to be addressed before this manuscript can be published in this journal. Please see below for my major comments.

Comments to the Author:

1. In line 66, where you mentioned that "... 4) when the surface becomes extremely wet (i.e. a saturated water layer, running water, surface ponding), the microwave signal is affected to the point that melt detection may become impossible despite the surface being obviously wet.", can you please explain why it becomes impossible? When it is very wet to the point to the point that a microwave signal is saturate (depending on the frequency of operation), a melt event for that specific season would be detected, but there may be a delay in detection (depending on frequency), or even uncertainty in the correct absolute value of liquid water content, as one frequency signal gets saturated, it cannot retrieve any further excessive wetness. And here is when multi-frequency detection becomes valuable as you mentioned. That said, please correct me if I'm thinking wrong.

At this stage of the paper (the introduction) we can not address in full detail this complex topic, the reason why the sentence is a bit vague. We will slightly reformulate the sentence to give a better but still brief explanation, considering that the topic is addressed in detail further in the paper, in Section 4.5 about Experiment 4 and in Figure 15. "When the surface becomes extremely wet (i.e. a saturated water layer, running water, surface ponding), the microwave brightness temperature decreases -- because open water surfaces have a low emissivity (Comiso and Cavalieri 2003) -- up to the point that melt detection may become impossible despite the surface being obviously wet."

Furthermore, we do not understand the comment well, because the sentence is here about the "water saturation" (water is so abundant that no air remains in the snow) or pure water which is different from "microwave saturation" addressed by the reviewer (i.e. the brightness temperature at V is stable from some small water amounts). The former situation occurs for very high water content and in such a case, the signal at H polarization is low at all the frequencies (as on the ocean), the penetration is extremely short at all the frequencies. We don't see the benefit of the multi-frequency direction in this case.

2. In line 80, where you mentioned "This is only possible when the snowpack is dry, before or after the melt season.", isn't it different to use before or after melt season as a good dry snow condition? A snowpack that went through a longer winter season, will become closer to a more ideal dry snowpack; hence, a snow condition more to the winter season (further away from last day of melt season) would

be a better dry snow condition. Can you please explain this a little bit here and where it's necessary in the paper?

We agree with the reviewer that the snowpack is different before and after the melt season. The 'ideal' snowpack for our purpose is probably the winter snowpack for the earliest stages of the melt season and the autumn snowpack for the late stages when the snowpack is ripped. In practice we mostly use the winter snowpack in this paper and do not consider the progressive changes over the course of the season, which could be a further refinement of our study.

However, it seems that the reviewer's comment is raised because 'dry snowpack' is sometimes used for a snowpack that has never been subject to melt. Here and throughout the paper, we use "dry" to strictly mean "actual absence of liquid water" and we use the more subjective terms "winter / autumn snowpack" in the following to distinguish the typical snowpacks before and after the melt season. We propose to change "dry snowpack" into "dry snow" here but we keep "dry snowpack" in the following.

3. In line 153, where you mentioned "A third regime described by Colbeck ...", can you please add this sentence to the end of paragraph? It's too small to be a paragraph on its own.

It is indeed small, but it does not fit well in the previous paragraph which is about the funicular regime. We propose to completely remove this non-essential statement.

4. In line 234, you mentioned "Water was always added by filling the air pores, which means that the ice mass (i.e. the dry snow density) is kept constant.", can you please why it has been decided to keep the density constant? Shouldn't the snow density change with snow melting, at least for the tope layers, where usually undergo metamorphism?

It is correct that the total snow density is likely to change during melt because of the physical processes of metamorphism and densification. Other properties of the snowpack are also likely to be affected, adding more variations on Tb on top of what we have simulated in this paper. We have not taken these physical processes into account because 1) we want to investigate the variations of Tb as a function of a single effect, the addition of liquid water only, all other properties (density, grain size, …) being constant for the clarity of the analysis, and 2) these processes are complex and would require running an advanced snow models (e.g. Crocus or SNOWPACK), adding complexity to this study. Here all the presented results are only due to the liquid water, not to other concomitant structural and microstructural changes in the snowpack.

5. In Figure 2, please , mention in the caption the period (months of year) observed brightness temperature.

This will be added.

6. In line 254-255, where you mentioned "For instance, at H-pol, the brightness temperature is low and close to that at 6 GHz on Amery and Larsen C, whereas it is much 255 higher and close to that of 37GHz in the other sites. The reason for this is not clear.", first of all, it is very important when these measurements have been performed. For example, some years, some ice shelves can undergo more metamorphism, which introduces more distinct vertical layer boundaries, which in turn decreased the

brightness temperature drastically, mainly in H-pol. Also, surface scattering can decrease TB if it's comparable to the wavelength, and maybe that's why L-band is close to 6GHz in Amery and Larsen C. In general, as frequency increases, the applied electric field changes polarization faster and forces the dipoles in the dielectric to change directions faster as well. Hence, dielectric loss increases. In addition, as the frequency increases, E-field changes polarization faster than the dielectric dipoles' relaxation time, which in turn decreases the displacement electric field in the material and decreases the dielectric constant. Hence, TB increases (less reflection). Perhaps, by comparing different measurement during different periods of year (or even same months but different years), you may be able to demystify why L-band behaves like this.

Thanks for the comment, what is suggested could be very relevant but if one wants to accurately estimate the temporal Tb trends in a specific site where accurate information (or hypothesis) of snow structure is available. Nevertheless, this is out of the scope of the paper which intends to contribute to a better understanding of melt detection using an approach that balanced precision and simplicity. In our case the precision is not required because our simulations with wet snow do not depend too much on the dry snow properties. The Tb winter inter-annual dynamics, as evoked by the reviewer, is a different and more elaborated topic and it would certainly require a different paper, with different goals and approach. This is the reason why the section on the dry snowpack is no more than 1 page long and that we do not address the dynamics of the snowpack.

7. In line 256, it was claimed that V-pol suggests that scattering increases with number of melt days. Can you please explain why? This is contradictory with the statement in line 262, where you mentioned that H-pol and V-pol are controlled similarly by scattering. Your claim in line 256 could be right, but it definitely needs more proof and validation.

We propose to extend the paragraph in the following way:

"In general, V-pol brightness temperature at Brewster angle is mainly driven by volume scattering and snow temperature. The observed decreasing trend with the number of melting days (e.g. a 40\,K decrease at 37\,\unit{GHz}) suggest that scattering is increasing with the number of melting days" and similarly change the paragraph on the H-pol, as addressed in the next comment (8).

8. In line 266, where you mentioned H/V ratio becomes larger mainly due to the layering, can you explain this? Assuming that there is no attenuation, and no surface scattering, are we still seeing this effect? Usually, H and V observations becomes close to each other when there is enough surface scattering to make the signal to loose its coherency, then it becomes polarization- independent.

We will reformulate this paragraph in the same way as the previous one about V-pol (see also Reviewer #2 comment).

"In general, the H-pol brightness temperature is more complex because it is in part controlled by snow scattering and snow temperature (exactly as V-pol) and in addition, it is sensitive to the surface density and the vertical density fluctuations in the snowpack (layering). The ice layers decrease the brightness temperature at H-pol due to the reflections on the high dielectric contrast between snow and ice in the upper part of the firn \citep{montpetit_2013}. The variations in V-pol and H-pol are correlated and of similar amplitude only if the ice layer effect is negligible. "

The physical reason for this behavior of H/V with the ice layers is well documented (E.g. Montpetit et al. 2013).

9. Please also explain why in figure 2, V-pol decreases and then increases, while H-pol monastically decreases by increasing the number of melt days.

The two reformulated paragraphs in this section interpret the variations observed in Figure 2. However, we don't see the simple variations depicted by the reviewer, to us, the variations are more complex. Our explanations in this section are therefore limited to a few points that seem to us robust enough, but we do not understand all the variations in the Tb and in the retrieved parameters.

10. Can you please compare your retrieved snow density with that of other available methods, such as "M. Mousavi, A. Colliander, J. Z. Miller and J. S. Kimball, "A Novel Approach to Map the Intensity of Surface Melting on the Antarctica Ice Sheet Using SMAP L-Band Microwave Radiometry," in IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, vol. 15, pp. 1724-1743, 2022."?

As stated in our manuscript, "While it is tempting to analyze the retrieved properties of those snowpacks, it is important to recall that the problem is under-determined and the snowpack representation simplified. Many equifinal sets of parameters give similar brightness temperatures, despite they may depict quite different snowpacks from one another, and from the real snowpack as well \citep{beven_1992}."

It is not our ambition to retrieve snowpack properties that would be comparable with other methods or in-situ data. Here, our objective is to quantify the melt effect by using a simplified but realistic representation of dry snow pack but the aim is not the retrieval of snow density.

In fact, we don't believe it is possible to retrieve density (and the other properties) with a useful accuracy for geoscience using microwave data only. As explained in our paper, the retrieval problem is under-determined when considering four layers and three unknown properties per layer. To obtain a well-constrained problem, the snowpack would need to be oversimplified with 1 or 2 unknowns, that is using a single layer, or multiple layers but assuming some constant values from some properties and neglecting the ice layer. The consequence would be 1) easier retrieval of a density value, but 2) this estimate would be uncertain, carrying all the neglected effects (notably layering) and would be method/assumption-dependent. This is certainly the case for the study cited by the reviewer.

To avoid the oversimplification of the snowpack in our setup, we decided to keep the problem under-determined, but as a consequence, we don't obtain a single value for the density. Instead we obtain a probability of possible densities and in general unfortunately, the range of possible densities is very large... too large to be useful in a comparison or for geophysical applications.

This is why we think that our "realistic" snowpack is only realistic from a microwave point of view, not from a geophysical point of view.

11. In line 318, what do you mean by "pixel.day"? Can you please explain this unit to avoid confusion?

This unit is frequent in studies on melt and passive microwave (cumulative melt area) but we will reformulate to avoid it:

“In reality, such high brightness temperatures are rare, they can be found only 347 times in the full daily AMSR2 19\,\unit{GHz} records gridded at 12.5\,\unit{km} over 9 summers (2012--2021). For comparison the number of times melt is detected on this same grid and for this same period is 2.5×10^6.”

12. Please specify incident angle in Figure 4, and wherever it’s needed.

The value is now indicated in the method section and is the same for the whole paper.

13. Can you please compare your result for snow wetness with other algorithms? You can convert it to liquid water column using density and thickness of wet snow layer.

It is not clear to which part of the manuscript this is referring to. The aim of the paper is to better understand melt detection at the different frequencies, we do not intend to retrieve snow wetness.

14. In Figure 5, please specify in the caption that solid curve is V-pol and dashed one is for H-pol.

Done.

15. In line 404-407, it was claimed if the total liquid water content is fixed, wet snow layer thickness has little effect. First of all, can you please write an expression to relate volumetric liquid water content to the total liquid water content? If the thickness is normalized in the total liquid water content, the thickness should have zero effect with fixed total liquid water content, in other words, as thickness increases one would expect the signal to lose more energy with exponential term depend on the thickness and the imaginary part of propagation constant. However, if the imaginary part is normalized by depth, and kept constant, the exponential term would be constant. That said, I believe a bit more explanation with some equations would avoid any confusion.

We will reformulate the paragraph and add the equations, as follows:

“The previous results were obtained with a fixed wet snow layer of 10 cm. Figure \ref{fig_aws19_experiment2} reports a few results for different thicknesses of wet snow h_w (for Roi Baudouin and at 6 and 19\,\unit{GHz} only) considering two predictor variables (i.e. the variable on the x-axis): the total amount of liquid water TLW or the volumetric fraction of water θ_i. These variables are related by: $TLW = \theta_i \rho_\textrm{water} h_w$ where $\rho_\textrm{water}$ is the water density.”

16. In line 418-420, where it was claimed that total water content has more effect than volumetric water content, it is very confusing. It looks like there are two different parameters. This needs to be clearly explained.

We acknowledge that “main/second driver” was misleading, our purpose was to explain why the total amount of liquid water is a better predictor than the volumetric water content if the wet layer thickness is not known or is varying. We reformulate the conclusion to fit closely to the paper’s aim.

“In conclusion, plotting brightness temperature variations as a function of the total water content (\unit{kg\,m^{-2}}) allows a better generalization of our results for other thicknesses than if the volumetric water content was used. For this reason, we generally present our results in total water content.”

17. In line 470, where you mentioned “... saturated layer exceeds a quarter of the wavelength ...”, why quarter wavelength? It should depend on the penetration depth. Can you please explain where does this quarter wavelength come from?

This is the (approximate) value from which the “coherent effect” disappears and when the reflection of the layer can be calculated with the (incoherent) Fresnel’s coefficients. Under this limit the reflection is not as strong. We will add a reference to Wiesmann and al. 1998 which is specific to snow, but this effect is very common in electromagnetism and has many names (see e.g. https://en.wikipedia.org/wiki/Thin-film_interference).

18. In Lines 519 – 521, where you mentioned “This is in apparent contradiction with the results in Fig. 17 showing the brightness temperature at L band with a 30 cm thick wet layer at increasing depths (Experiment 3).” If there is a very wet layer, such that the thickness is greater than the penetration depth of the frequency of operation, it becomes a very reflective layer, and drops the TB, no matter what depth this layer has been resided. S, it is very confusing that you are claiming it is in apparent contradiction with (Mätzler, 1996; Macelloni et al., 2016, 2019) for L-band. For example, if there is 100m snow with some but small wetness, L-band can detect aquifer as penetration depth is higher, while higher frequencies cannot see the wet aquifer layer.

Our simulations show that for wet snow with a moderate amount of water, this layer behaves as a black body, not as a high reflector. We will change “wet layer” to “wet snow layer” everywhere it was confusing and we will add the total amount of water (6.5 kg/m2 corresponding to the maximum of Tb H-pol in Fig 16) which was not explicit.

The reviewer describes the situation of a saturated layer of water, and indeed the theory suggests that such layer behaves as a reflector (lowering Tb) and would be seen at much greater depth (~100m at L band). This is the case of the sea water under ice-shelves for instance, visible for thin ice-shelves or buried lakes. We have not included this case in the paper because we have not found real observations for this situation caused by melting.

19. The results need to be compared with in-situ values or other algorithms. There are some other algorithms using L-band (refer to my comment number 10). Same authors, I believe have done a similar thing for Greenland.

It is not clear which part of the manuscript this comment refers to. Unless we misunderstand what is meant by “algorithm”. Our paper is not about retrieval algorithms, the topic is a sensitivity analysis of brightness temperature to liquid water. Its intent is to be useful to build better algorithms in the future, but we do not propose any precise retrieval algorithm here.