We thank the reviewers for their supporting comments.

General

I thank the authors for carefully considering my comments and the extensive replies. Both of my general remarks are adequately addressed, despite I think figure 6 would benefit from showing the mean ± standard deviation for each frequency instead of 1000 individual, but this is up to the authors preferences. I have only a few minor comments left (see below).

We have changed Fig 6 according to the reviewer's suggestion, it is indeed clearer.

2 Specific comments

L87: (Comiso et al., 2003)) → remove extra)

Done

L304: when the e-folding depth is only 5.2mat Roi Baudouin. → frequency missing

We have added: “at 6 GHz, the lowest frequency used in the optimization”

L312: this increase in density... → I guess you mean ”increase in ice layer density”?

Yes, it was misleading to use only ‘density’. It is now corrected.

Figure 5: The authors showed the same results using the coated sphere permittivity model in the response letter to the reviews. I think this comparison should be (shortly and qualitatively) discussed in the main manuscript, since the reader gets an idea of the impact of the model choice.

We have added a sentence:

“The transition between the first, "absorption" regime and the second, "reflective" regime takes place around 0.75 and 1.75\,\unit{kg\,m^{-2}} at 37 and 6\,\unit{GHz} respectively. \cite{shi_1995} reported a slightly higher value of 3\,\unit{kg\,m^{-2}} for radar at C band (5.6\,\unit{GHz}). **Using a different formulation of the wet snow permittivity would change these values. For instance the coated sphere model (results not shown) has a higher imaginary part of the permittivity, which leads to even more rapid increases in the first regime, and a reduced decreasing rate in the second regime, because the absorption dominates even more than the scattering and reflection mechanisms.”**

Figure 11, caption: add ”The values of the dry brightness temperatures are marked by the triangles.”

This is added.

L517: ”or in on the eastern Roi” → remove ”in”

Corrected.

Referee #3: Angelika Humbert

Many thanks for the efforts the authors took for improving the manuscript. I can fully understand that taking a new way to estimate the temperature profile is too complex for the current manuscript, but I am still convinced that it is a minor effort (given the complex simulations!) and would be highly beneficial and I highly recommend to consider this for FUTURE studies.

We agree that it is a logical next improvement of our method, especially if the purpose is retrieving (accurate) dry snowpack properties.

My point on the performance is obsolete - this was a misunderstanding on my side.

However, to the melt rate is indeed a very relevant quantity for this manuscript, as the (total or volumetric) water content is highly influenced by the amount of water available for infiltration/percolation into the snow. The data is provided by RCMs and can easily be analysed. Only comparing to ERA2m temperatures (given the atmospheric inversion in the 2m above the snow surface) seems to me not sufficient.

This refers to a comment of the first review that was not clear to us, as indicated in our response. We agree that the amount of liquid water present in the snowpack is controlled by the melt rate, at least its maximum value, while the minimum value is also controlled by the refreezing capacity of the snowpack (cold content) and other processes. However, we do not understand how this fits in the present analysis, where we don’t try to predict the liquid water content, we “only” explore the sensitivity of the brightness temperature to prescribed amounts of liquid water.

Line 344: Banwell in preparation cannot be cited

We have removed the reference.