Editor comment

Your paper has received two reviews based on your revisions. The second reviewer has raised some concerns on how the discussion section has been structured in a speculative manner owing to the lack of in situ data for validating snow depths on Antarctic sea ice. While I understand your limitations and challenges to justify your discussions, I feel the second reviewer has their rationale related to how the different frequencies behave in terms of its penetration on a diverse range of snow covers especially during the summer season where snow undergoes geophysical and thermodynamic changes drastically causing snow covers to become even more complex and complicated to be tracked. I suggest you go through the reviewer comments and think about how the speculatory discussion can be reduced (I understand it cannot be avoided in this context). For that reason, I recommend major revision for this round of review.

Response to editor and reviewers

Reviewer comments are provided in bold, and responses to reviewer comments are in *italic blue.*

We thank the editor and reviewer for their valuable comments and feedback. Indeed, the summer season noticeably changes the physical and thermodynamic conditions, however, the summer impact is less noticeable for Antarctic snow cover compared to the Arctic, where we can have snow cover surviving the melt season, although with a complex structure, and studies of the Antarctic snow cover using satellites have been applied for the entire year (for passive microwave radiometry, but also from altimetry using the same methodology). Thus, while we acknowledge that the impact of the summer season cannot be neglected, and discuss this more in the updated version, the results themselves speak for the possibility of extracting snow depth (or at least that some microwave penetration occur and appears to reflect at some clear inter-faces) from some of the radars during this period. And overall, this manuscript presents a unique collection of data sources inter-compared at different scales, which has not been published before.

We have taken reviewer and editor comments into consideration, and have made the following amendments to the papers (Part I and Part II):

- *Updated figures (updated Fig. 5 in Part I and included a new figure in Part II) and text based on Reviewer #1 comments.*
- *Inclusion of new references either suggested by Reviewer #1 (Barber et al. 1998; Mallett et al. 2024), or to support new additions (e.g., Jutila et al., 2025; Farrell et al., 2012).*
- *Included a short paragraph on the impact of snow conditions and instrument specifications for satellite observations in Part II, which was missing (Reviewer #1 suggestion), with a strong reference to the snow conditions discussion already included in Part I.*
- *Updated the discussion (in response to Reviewer #3) to be less speculative where we saw it needed.*

We hope that these amendments are to your satisfaction. We look forward to your response.

Reviewer #2: John Yackel

I think the authors have made a wise decision splitting the original manuscript into two parts. The two-part manuscript now reads succinctly and fluidly. The authors have done an exceptional job at reorganizing this work. It makes good sense that Part 2 is a slightly shorter paper than Part 1 because the authors can reference Part 1 frequently, particularly wrt to some introduction, study site and some methods, towards reducing redundancy. I have only a few technical issues and comments for consideration.

We thank the reviewer for their efforts in reviewing our manuscript, the positive feedback, and the great suggestions. Please find a detailed response to each point below.

<u>Part 1</u>
A really small technical thing. Figure 5 caption and legend have slightly different terms. In the legend which appears above the figure, the delta (ha - i) term is different than the delta (ha-s) term. I believe the 'i' in the legend for this expression should be an 's'. Also, I don't see the need to extend the x-axis range out to 800 kg m-3. Why not just extend to ~ 550 or 600 and perhaps start at 150 instead of 100. Then your inset plot of the 280-380 range would expand in size a bit, which would be nice.

We thank the reviewer for the comment and the keen eye. We will correct the Figure 5 caption and legend. We have originally expanded to 100-800 based on papers which showed examples of this variety of densities. Based on the reviewer's comment, we will change the x-axis from 150-600 kg m-3 instead and expand the inset plot, as shown below.

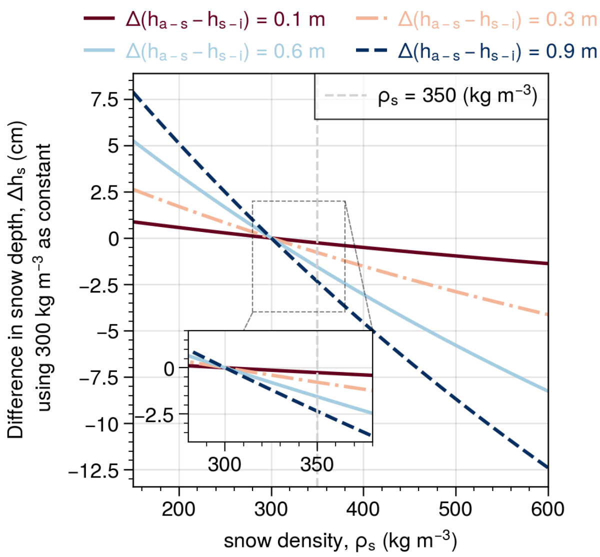


Figure 6b (axis above figure). Should the along-track distance be in km? (not meters). 0.07 degrees latitude corresponds to 7.77km so going from slightly less than 603 to slightly larger than 611 km would make sense.

Corrected, thanks.

L521 - ….'we note the presence of liquid **water** in the snowpack would likely …"

Corrected, thanks.

L522 - ….'which **could** have produced …'

Corrected, thanks.

L541-555. I appreciate the addition of this section to explain the potential dielectric and scattering complexities which can arise at these temperatures. However, I suggest replacing Barber et al., 2003 reference on L545 with Barber et al., 1998 The role of snow on microwave emission and scattering over first-year sea ice | IEEE Journals & Magazine | IEEE Xplore as the better place to find the description of how brine is expelled upward into the basal snow layer. Furthermore, a recent assessment of the process is described here https://doi.org/10.1017/aog.2024.27 and would be worth citing behind Barber et al., 1998.

We thank the reviewer for suggesting these references, which have now been included in the paper.

L548 – "Importantly, **from a** remote sensing perspective …"

Corrected, thanks.

L650 – 'This is expected caused …." reads awkwardly. Please revise.

Corrected to:

"This is expected since PEAK can identify peaks before and after MAX as the s-i interface provided the peak fulfils the requirements"

L654 – I suggest revising the sentence 'Several inconsistencies are observed between the a-s interface determined by ALS and the radars likely caused by … " to "Several inconsistencies are observed for the as interface determined by ALS and the radars and are likely caused by …"

Corrected, thanks.

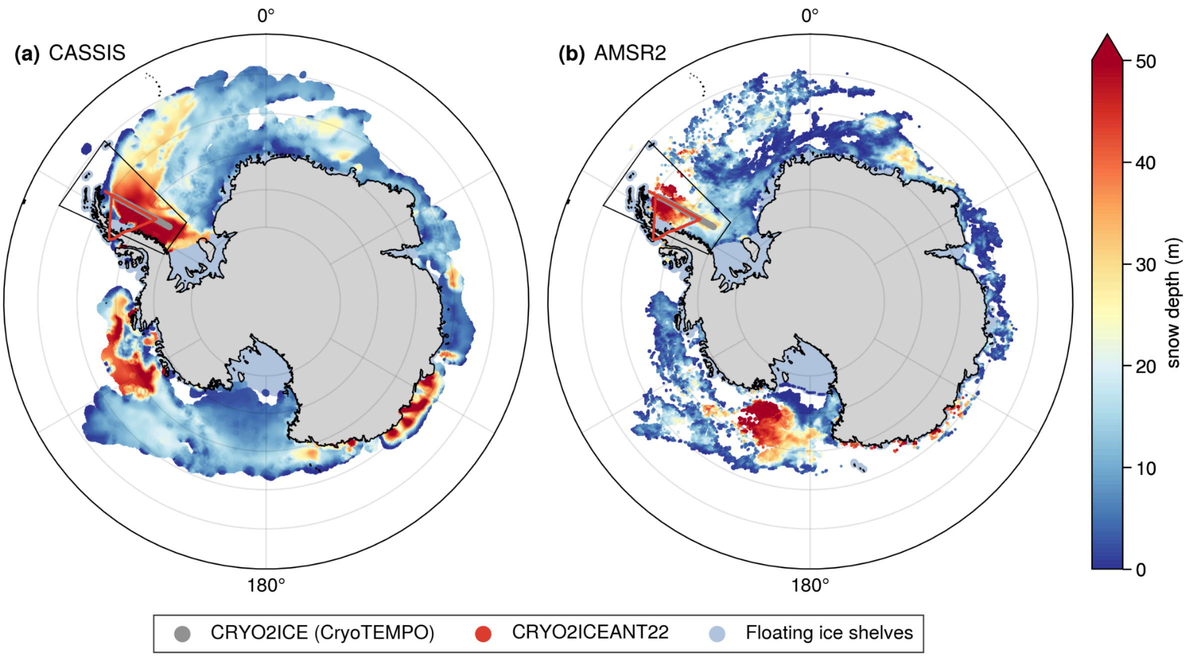
<u>Part 2</u>

Here, I have two comments for consideration.

1) In Part 2 you make the comparison of your CRYO2ICE derived snow depth to AMSR2, but you have left the AMSR2 background image in Figure 1 of Part 1 (because this was one big manuscript initially and you have decided to not alter the figure … I wouldn't either … it's a really nice figure). So, for all the AMSR2 discussion in Part 2, we are left to viewing a few AMSR2 points on Figure 2 Part 2. As a result, your CRYO2ICE tracks in Figure 1 of Part 2 lack a bit of spatial context to the broader AMSR2 estimates, especially if you are not going to refer to Figure 1 Part 1 in all of the CRYO2ICE to AMSR2 comparison discussion in section 4.2 of Part 2. So, I guess what I'm trying to say is, can you figure out a way of presenting Figure 1 from

Part 1 to show the AMRS2 background image needed for your discussion of AMSR2 in Part 2 and for the larger spatial context of your CRYO2ICE tracks shown in Figure 1 of Part 2. I get that you can't show Figure 1 from Part 1 in both papers, but can you possibly integrate a sub-image of the AMSR2 background image shown in Figure 1 Part 1 into Figure 2 Part 2so that AMSR2 image data presented in Figure 1 Part 1 doesn't go to waste, especially if you are not going to refer to it in section 4.2 Part 2?

We thank the reviewer for the comment. We include a reference to Figure 1 in Part I for a zoom in, and include the following figure of both products from 13 December 2022 showing the pan-Antarctic snow depth distribution in Part II.



2) So, brine wicking and its potential for altering dielectrics and scattering was thoroughly discussed in Part 1 (L541-555) and it is introduced in the Introduction of Part 2, but it is not mentioned again in the rest of the manuscript (Part 2). In additional to ice surface roughness, the brine wicking process and basal snow layer brine volume affects the snow dielectrics, and it affects Ka- and Ku-band scattering and attenuation as you rightfully acknowledge in Part 1. As such, I would have expected some additional discussion of its potential affect on CRYO2ICE penetration depth, scattering and attenuation. The fact that the Ka- and Ku-band sensors are on satellites (Part 2) as opposed to aircraft (Part 1) doesn't change the implications for dielectrics and microwave penetration, scattering and attenuation. I strongly suggest you add some additional commentary in either section 4.3 or 5. Or, make stronger reference in Part 2 back to your description in Part 1 (L541-55)

We thank the reviewer for the comment. Indeed, it does not make a difference whether the sensors are on an aircraft or a satellite in terms of the dielectrics and microwave penetration as such, however, the bandwidth and imaging geometries will govern what is detectable in the data as well as the central frequency of the instruments, and cannot be neglected - hence the impact of scale. Furthermore, for the satellites, we do not have a Ka-band sensor, but the photon-counting laser *altimeter instead, which has an even smaller footprint than the Ka-band satellite radars.*

We have now included the following in the discussion (Sect. 5) of Part II:

"Similarly to the airborne data (Part I, Section 5), the snow conditions also matter to the satellite observations, and the retracked scattering horizons are directly related to the instrument specifications of the spaceborne altimeters, as with the airborne. Hence, any snow conditions (e.g., saline snow, icy layers, snow grains, etc.) limiting penetration at airborne scales are also expected to be impactful on the satellite scales, although at different magnitudes, which one may assume when the footprints are larger and the resolution lower. "

Finally, as an overall comment on both Parts, I agree that there appear to be MAUP (Modifiable Areal Unit Problem) issues at play (L419 of Part 2). Openshaw https://www.uio.no/studier/emner/sv/iss/SGO9010/openshaw1983.pdf is a good reference for trying to understand the problem wrt measuring snow covered sea ice from various microwave sensor characteristics and ground resolutions/footprints as a function of height above the surface. These scale issues are no doubt a result of different altimeter processing techniques as a function of frequency but some which are likely as result of MAUP as a function of the length scales of snow thickness distributions and ice surface properties particularly wrt to ice surface roughness resulting from heterogeneous ice freezing/consolidation processes and dynamics+deformation, etc. I would be curious to hear the authors thoughts on how MAUP can be overcome (or at least minimized) as the sea ice community continues to use both airborne and surface-based multi-frequency microwave measurements towards either calibrating and validating satellite-based estimates.

We thank the reviewer for the comment. Indeed, MAUP is at play in terms of different spatial distribution of datasets and the way they are aggregated (e.g., going from airborne to satellite comparisons, to comparison with gridded data). However, we would argue that some of the most important aspects relates to the sensor specifications and the surface characteristics retrievable using that specific sensor (so not only as a function of frequency, but as a function of beam width, range resolution, footprint and such). Thus, with future work, we hope to make some sensitivity studies to evaluate the impact of this on the scattering interfaces considering not only the frequency, but also change of resolution, area size (altimeter processing techniques - e.g., low-resolution-mode vs SAR), altitude (related to footprint) and such. In addition, further discussions on how field work could (or maybe "should") be performed to emulate what the airborne and satellite sensors will see - or how we can at least try to reach something comparbale, so that we can actually "validate" them under the same conditions. Some projects are preparing simulators to evaluate different validation scenarios, but we do believe that it will likely not be overcomed (or at least minimised) without a strong collaborative interdisciplinary effort including modelling of ground, airborne, and satellite observation and further exploration of the data we already have, to see if there are things we have overlooked in the process.

Reviewer #3
The paper 'Multi-frequency altimetry snow depth estimates over heterogeneous snow-covered Antarctic summer sea ice – Part I: C/S-, Ku-, and Ka-band airborne observations & Part II: Comparing airborne estimates with near-coincident CryoSat-2 and ICESat-2 (CRYO2ICE)' by Hansen et al. has been carefully reviewed. The paper disucsses how airborne Ka-, Ku- and C/S-band radar signals interact with summer Antarctic sea ice and how snow affects the radar signal propagation affecting snow depth retrievals, which has impact on CryoSat-2, AltiKa and upcoming CRISTAL missions. Although, the paper comprehensively (well-written) describes existing literature, theory of multi-frequency radar signal propagation through snow-covered sea ice and uses unique datasets to investigate its potential to retrieve snow depth on summer Antarctic sea ice, I have some reservations for immediate publication of this paper. Here are my general comments for this round

We thank the reviewer for their efforts in reviewing our manuscript, the feedback and suggestions. Please find a detailed response to each point below.

a) I am happy the authors acknowledged the lack of in situ snow depth and geophysical properties data on summer sea ice, but that is itself a downfall in the discussion part (mostly sepulations through lit review) supporting their results. Lines 135-140 show their dependence on ERA-5 reanalysis data of air temperature and precipitation to discuss their findings. This inherently amplifies the uncertainty in what is seen from the results (e.g. Figure 6 showing 'probable' air/snow and snow/sea ice interfaces). I do agree sometimes when there is no in situ data, we have to rely on literature review, but the discussions are way 'too speculative', considering the assumptions of snow properties used in the methodology.

We thank the reviewer for their comment. Indeed, it is difficult to be more than speculative when there are no in situ observations for comparison. Within the altimetry community, and especially when comparing with satellite data, the "snow radar" has been utilised as a "reference" for the snow depth (in lack of in situ observations, but with great comparison to in situ data nonetheless - although it has its own caveats, as explained in the paper), it remains the only dataset that resembles a reference (aside from the laser for air-snow interface), and it was therefore deemed a unique - and optimal set-up - to have all the radars and lidar on board to evaluate microwave penetration. However, you can argue that the snow-radar data is not sufficient on its own as a reference (even though the community generally agrees to use it as such). To limit the speculative discussion, we have critically re-assessed the text and minimised where we saw fit or referenced further to literature - since, as the reviewer also states, we do still have to rely on literature reviews when in lack of in situ data.

To support the comparison in lack of in situ data, we did also compare the airborne estimates and satellite estimates with both AMSR2 and CASSIS, which have both been compared with contemporaneous in situ and/or reference data, which is the next best thing when additional contemporaneous (beyond the airborne data as presented in this study) is not available for further validation. We are keen to hear what the reviewer might suggest of data otherwise to support the study.

2) Based on 1), the authors use 300 kg/m3 as the bulk snow density for Antarctic snow cover on sea ice, although field observations and past studies show presence of melt/refreeze layers, slush, refrozen snow-ice layers and complex snow layering both during

winters and summers, not just on sea ice that are flooded, but also for positive freeboards. Especially, with daily diurnal fluctuations in meteorological conditions, presence of all these complex snow properties discussed above are common. My problem is that the assumption of a 300 kg/m3 bulk snow density may not work considering these issues, instead, I suggest authors to conduct a sensitivity analysis based on changes in snow density (of course snow density is not the only one error source).

We thank the reviewer for this comment. Indeed, it is not the only aspect at play, but an important one. It is also why we already included a sensitivity study in Fig. 5 of Part I based on snow density, where it shows that depending on the density used (for most studies often ranging from 280 to 350 kg/m3), the snow depth will not change by more than approximately 2.5 cm for snow depths of 0.6 m (and less for thinner snowpacks of course) using the established assumptions for deriving snow depth from the differences when comparing to using the bulk density applied in our study. However, we apologise that it has not been more clearly presented in the paper, which we have now aimed to do in Sect. 4.3, Part I.

As Reviewer #2 previously suggested (in the first review round), other methods could be explored (e.g., what happens when there is slush, etc.), but the current community-agreed-upon method for estimating snow depth from dual-frequency (assuming dry and cold snow conditions) follows the methodology utilised in this study (and which has also been applied for year-round snow depth maps using CryoSat-2 and ICESat-2 in published studies, even with the caveats that the snowpack might not be dry and cold throughout). For now, we have utilised that method too and believe that it is out of scope to conduct radar backscatter modelling or additional sensitivity studies. However, we have planned for future work on radar backscatter modelling to support dual-frequency altimetry approaches for a variety of snow and ice conditions.

3) Your lines 369-372 'Traditional hypotheses of the radars include Ka-band primarily scattering at the a-s interface, Ku-band at the s-i interface (over cold and dry snow), and that C/S-band should reflect at the s-i interface at maximum scattering. Furthermore, airborne Ka- and Ku-band traditionally assume one surface to primarily contribute (thus, we only track one point of the waveform). In contrast, we assume C/S-band to be influenced enough by both interfaces to re-track both.' My question on this. How do you say C/S-band 'SHOULD' reflect from the snow/sea ice interface. If you have thin (pre-melt time) snow, these long wavelengths can scatter max from the ice volume or even from ice/ocean interface. Even otherwise, if you read my previous comment on snow properties such as slush and snow-ice formations, neither Ku-, or C/S-band will penetrate from the snow/sea ice interfaces or below. Because the density inhomogeneties and salt content from these layers are stronger causing the scattering to be the strongest from these layers. This issue ties up with my initial problem of speculating results without any in situ data. Also, how did you decide the location of the snow/sea ice interface from the '---->' line in Figure 6 labelled as snow-ice interface?

We thank the reviewer for the comment, which we agree with. In fact, in the manuscript (especially Part I), we discuss several times the impact of various snow conditions such as e.g., slush, snow-ice formation, brine, etc., which will alter the reflective horizon, but we apologise if it has not been more clear and has tried to make clearer in this revised version. It is also true that the C/S-band could reflect within the ice under specific conditions. However, C/S-band has long been used as a snow-radar system, and it has not been a *worry. Most of the time, the underlying ice interface is assumed to provide the maximum scattering (this assumption is also what the official OIB data product relies on). We have changed the wording to "tends to" instead, and edited parts of the discussion to include more of the considerations of the reviewer.*

Regarding the snow-ice interface location in Figure 6: The location of the snow-ice interface is taken from comparison to the C/S-band, which is re-tracked as the snow-ice interface with all of the re-trackers (CWT, PEAK, and MAX). The caption is now rephrased to: "In the Ku-band, what appears to be a s-i interface somewhat apparent based on a qualitative guess corresponding to the location of the s-i interface observed in the C/S-band ; however, it does not represent the maximum scatter or somewhere on the leading edge of Ku-band.".

These are my major concerns for now. I do understand and you have clearly acknowledged the lack of in situ data in the paper that inhibits what is 'really' happening. But I feel, there should be some sort of radar penetration modeling or a sensitivity analysis to atleast reduce speculative discussions in your paper. For that reason, I think, the authors should be given a chance to respond to this review and come up with a stronger analysis to rebutt the reviewer comments. For that reason, I recommend the paper to go through major revisions for this round.

We thank the reviewer for the review, and agree (along with the other two reviewers) that in situ data would be ideal. Nonetheless, it is not very often that in situ data is available from the airborne campaigns nor in a format that makes it comparable to the satellites - however, that does not mean that the airborne itself does not have merit, and should not be evaluated - especially when such a unique set-up is available with three different radars and a lidar system. In addition, it is very rare to have collocated in situ observations along both satellite and airborne tracks due to the drift and logistics, as well as weather, meaning that we often are not able to collect the required in situ observations, even if it was planned.

However, including a specific sensitivity analysis of radar penetration using a radiative transfer model and complex snow pack structures is out of scope and deserves a study in itself. For these manuscripts (Part I and Part II), the sensitivity analysis based on snow density (which is the only variable in terms of methodology) is included, and future work on such radar backscattering modelling is planned by the authors.