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.

Part 1

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.

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.

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

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

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.

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

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

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 a-s interface determined by ALS and the radars **and are** likely caused by …"

Part 2

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 2

so 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?

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)

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.