Dear Editor and Authors,

Please find attached my second-round review of Mutter and Holschuh (manuscript number: egusphere-2024-2450) with manuscript title "Advancing interpretation of incoherent scattering in ice penetrating radar data used for ice core site selection".

In general, I would recommend this paper to be published in *The Cryosphere* with *minor* revision, and I very much look forward to seeing the updated version soon. Please find below a series of additional comments.

With best wishes,

Julien Bodart

Additional comments

− Figure 1: Could I suggest an alternative to the "core length" diagram on the right-hand side of Figure 1? I was suggesting in my first review to add the age of the ice cores in a figure in the main text as I think it would help the reader who might not be familiar with the age-depth at each core. The authors mentioned that this information was now in the supplementary materials, which is great. However, I believe that perhaps a good compromise (or opportunity) would be to order the cores in the diagram of Figure 1 by oldest to youngest (or reverse), so 1 in this scenario would be Dome C (currently 9), etc. This is motivated by the fact that there is no current logic behind the ordering of the cores in Figure 1, so ordering them by age would be an easy change that could address my comment (and the editor's).

Also, and maybe this is unfair at this stage of the manuscript, but do the authors think that the new WACSWAIN (Wolf et al., 2025; https://doi.org/10.1038/s41586-024-08394-w) ice core at the Skytrain ice rise site (WAIS) could be added to the manuscript? There is evidence of disruption in the depth-age scale in the deepest part of the core which remains unresolved. I know this is very early results and comes in late in this paper's process, but perhaps this could be interesting to add if there is time. This is not a strict requirement though and I understand that such addition at this stage is slightly unfair. I leave it up to the authors.

− Line 223: "of to" – rephrase

− Conclusion: I appreciate the observations made in this paper and believe that the conclusions from the analysis of the radar data made here is valid; however, I would like to see another sentence in the conclusion which highlights the potential subjectivity of the analysis and acknowledges that conclusions are not entirely independent from the acquisition or processing of the underlying radar presented here. I leave it up to the Editor and other reviewer(s) whether this step is a pre-requisite for acceptance of the manuscript, but in my opinion, it is an important caveat to add.

Indeed, if I am an ice-core person with little knowledge of radar, I might assume that this type of analysis/conclusions (especially Figure 5) are independent from things that can affect the quality of the radar data in its current form (i.e. acquisition or processing, whether pre-

processing or post-processing to enhance layering for example) and that such strict classification can be made without potential subjectivity, when in fact, and as acknowledge by the authors in their response to the reviews, there is some subjectivity when it comes to this analysis. This is not just subjectivity from the human eye, but also from the pre-processing or acquisition frequencies which affect the strength of the reflectors which are used to make the conclusions in this analysis. I take, for example, Figure 5a "Dye-3" sub-panel, where one could argue that there is some layering below the boundary that the authors put as "no signal", or Figure 5b "Dome F" where different conclusions could be made as to what each section represents. The addition of a simple gain function to these data could likely alter slightly the exact location of these boundaries and the type of scattering they represent, but I believe that the conclusions of the paper do not really acknowledge this caveat clearly.

I think the addition of Lines 125-127 earlier in the manuscript was really useful, thank you for that. But potential ice-core experts might skim through this and focus on the conclusions, and I think it would be worth adding a sentence here to acknowledge this caveat. I do note your addition of paragraph 443-449 in the Conclusion which might be read as an acknowledgement of such caveat, but in my opinion, it lacks a clear acknowledgment of the dependency of acquisition and pre-processing steps which could affect the "strict" classification made in e.g. Figure 5. A simple sentence, as in Lines 125-127 would be enough for me. As already mentioned, I leave it up to the Editor to decide whether this point is a bit too harsh. In any case, I really enjoyed reading the paper, I learned a lot, and I am sure it will be read by many others in the community for the right reasons, so I thank the authors for their work on this and sharing their findings and insights with the community.