

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-157

Anonymous Referee #1

Referee comment on "Antarctic snow-covered sea ice topography derivation from TanDEM-X using polarimetric SAR interferometry" by Lanqing Huang et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-157-RC2>, 2021

Summary. This is an interesting, detailed, well prepared paper that has important implications for

deriving sea ice topography using a unique approach, with single-pass interferometry. I have some requests on clarifications that I will write down in Details. The methods are largely clearly described.

I think I have three main points about the paper as a whole that I suggest the authors consider. First, the paper is presented as being applicable to both polar sea ice covers. However, particularly in the Introduction and Basic Concepts, they really don't distinguish sufficiently between the main differences in the sea ice between the Arctic and Antarctic. I have added some suggested references to include. So please add some more text about differences in ice type and snow layer. Next, it's not clear to me that a two-layer model is sufficient, with both layers considered to be uniform, to correctly identify the phase center. In Arctic first year ice, at the snow-ice surface particularly for young first year there is often a significant layer of high salinity slush ice that may also include frost flowers. There will be some penetration below this thin layer where the salinity is much lower. Then of course in the Antarctic, flooding at the snow-ice layer occurs due to relatively deeper snow loading on the generally thinner ice layer, as compared to the Arctic. This flooded layer has a higher salinity than the ice below but is still not likely sufficient to minimize further penetration. Plus of course there is increase in salinity near the ice-ocean boundary in all winter ice growth conditions. The slush layer is referred to in the paper, but I maintain that it is not sufficient to dismiss the possibility of a 3-layer without demonstrating otherwise which I suggest they do, as it may impact correct estimation of the phase center and therefore a modeled-derived height. Finally, last main point has two components – first while they compare with a DMS height as a narrow 2D transect, which is what is available and appropriate, they also show 3D output (Figure 13). However, there isn't much discussion about the 3d output – do these appear to be representative of what might be expected or compared to other possible data or studies? There are papers on ridge/sail characteristics plus a nice example in Tucker chapter 2 in Carsey sea ice microwave book. Second part of last point is that these 3D maps are really unique because they are not just narrow transects. How often for example could these 3D maps be generated with Tandem-X, spatially and temporally? I think Tandem-X is pretty limited by its duty cycle at least and perhaps storage/downlink too. It would be good to

hear about longer term capabilities for deriving this product and what might be required to validate Arctic products, for example.

Detailed comments.

- Lines 16-18. Add references that discuss ridge characteristics, etc in addition to Rampal reference for both poles, for example for Antarctic, Lytle et al. *Annals Glaciology* 1998, two Tin and Jeffries papers in 2003/04, plus Timco and Burden 1997 for Arctic.
- Line 24, Tucker et al reference is for first year ice only. please clarify.
- Line 25. Petty reference discusses both FY and MY and differences. Please mention in text. Also following Toyota paper, there is a really good chapter on Snow by Sturm and Massom in Sea Ice book edited by Thomas and a recent chapter by Webster et al *Nature Climate Change* 2018.
- Lines 30-31. Add journal papers that utilize DMS data in addition to Dotson and Aversen references.
- Line 53. Substitute 'deficient brine' for 'reduced brine'
- Line 61. Substitute 'obtain an' for 'obtain a more' accurate.
- Lines 77-83. This paragraph should be expanded to discuss thin salinity layers at snow-ice interface as mentioned in the summary with references
- Figure 8. I guess I really don't understand these figures. I looked and looked at how one might determine that these graphs suggest phase centers of 6-7cm and 15-33 cm as described in the text – Lines 312-315. I would appreciate an explanation of what information they are using from these figures and how they are deriving the phase centers. I also hope the editors are getting a review from another person who has a lot more INSAR and radar modeling expertise than me.
- Figure 14. I think the grey lines for the removed sections are too distracting from both a) and b) and perhaps just not included in the graph. Are the grey removed sections the same segments as described in Figure 4 as being mis-registered and set at 0 height? If so, not sure why they need to be included in Figure 14 at all.

- Lines 474. Snow depth is not well correlated with thickness or even FY or MY ice and in both polar regions.

- Lines 509-511. This is a good sentence and touches back at some of my points in the summary and the need to clarify some references in the introduction and Basic Concept section, the idea of model that may need to improvement etc as well as how these type of products could be expanded in Tandem-X acquisitions and products.

- Basic concepts or Model Development or Discussion. I really do think it's important to consider a third thin high salinity layer at the snow-ice interface, whether on thin first year ice or flooded ice. I realize this might be a lot of extra work and at this stage of your study, it may not be of primary importance. This could also be added to the Model section or Discussion section too at minimum, as a topic for further research and what you think the impact might be on the model. Of course, the authors could tell me that they don't think it's a worthy topic at all and won't make any difference. I do think firmly that their two-layer is not universally applicable to all the major ice types and conditions for both polar regions, based on my understanding of their model. New and young ice are often the trickiest anyway to deal with any radar algorithm. Throughout the paper as I was reviewing it, I kept thinking about those two thin salinity layers and differences between first year and multiyear etc in both poles and how this should all be considered in a model of radar penetrating sea ice.