Comment on tc-2021-119

Anonymous Referee #2

Referee comment on "Cross-platform application of a sea ice classification method considering incident angle dependency of backscatter intensity and its use in separating level and deformed ice" by Wenkai Guo et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-119-RC2, 2021

The authors present results from a methodological investigation of the cross-platform transferability between training sets derived from two different C-band SAR platforms (namely Sentinel-1 and RadarSAT-2) for their joint use in providing improved spatio-temporal classifications of level and deformed ice. This is a very useful first look at this potential ! The authors have carefully and painstakenly considered and quantitified most of the relevant factors involved in level and rough ice signature differentiation using a combination of quantititative and qualitative analyses, including using expert knowledge from in-situ field personnel and data from the N-ICE 2015 experiment. The flowchart (Figure 1) is especially useful. I have one major concern and several minor editorial comments in my review. Overall, I recommend publication after minor revisions, including properly addressing my concern.

Major concern:

My major concern with this paper is with the treatment of the role of snow on sea ice and its potential influenece (both direct and indirect) on both surface and volume scattering at C-band for all FYI types. Most of co-authors are aware of the role of snow (ie. work of Barber, Yackel, Nandan, Geldsetzer, Mahmud, Gill and others, including Nghiem and Drinkwater) on C-band backscatter for ice types younger than one year which have snow temperatures warmer than ~ -5 C at image acquisition due to high dielectric basal layer snow brine volume effects (Barber et al., 1998 TGARS; Barber and Nghiem, 1999 JGR-Oceans). While the authors reference the SAR scattering season work of Barber et al., 2001 (originally Livingstone et al., 1987; 1991) and the polarimetric scattering characteristics from Gill et al., 2015, they have not provided convincing evidence of dry and cold snow conditions for some of the data used in their classifications (ie. an April 30, 2015 image north of Svalbard). N-ICE-2015 was characterized by frequent warm, southward originating storm events. Many of these warm storms would have warmed the snow and upper ice surface considerably, thereby altering the snow volume scattering properties on seasonal sea ice types. I would ask the authors to present a time series of air temperature data from N-ICE (likely to be the most representative observation of air temperature for the region of image acquisition) to confirm that the air-snow **AND** snow-ice interface temperatures were below -5 C for all images used in their classifications.

Minor typographical and grammatical:

L16 - using the word 'thus' reads awkwardly. I suggest removing it.

L48 - I suggest adding the reference to Tschudi et al., 2020 TC for the NSIDC ice drift product ... like you did for the OSI-SAF latter on in that sentence.

L106-108. The sentence beginning with ... " This study mainly examines ...' is redundant from previous mentioning. Please remove.

L126. Remove 'covering' and replace with 'collected during'

L138. Remove 'of' before lower

L139. **in** the marginal ice zone

Figure 4 caption: 'red' not 'read' ; Classification CA's ? ... how about just CA's ... otherwise it reads 'Classification classication acurracies'.

Table 2: Mahmud values should be negative ... not positive. Also, Gill et al., 2015 **time** should be 2008 ... not 2018.

There are several typos in the reference list due to cutting and pasting ... causing characters to get formated incorrectly.