The Cryosphere Discussions

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Comment on tc-2021-78

Anonymous Referee #2

Referee comment on "Sentinel-1 time series for mapping snow cover depletion and timing of snowmelt in Arctic periglacial environments: case study from Zackenberg and Kobbefjord, Greenland" by Sebastian Buchelt et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-78-RC2, 2021

General comments

This study has utilised 2 years of Sentinel-1 SAR data covering a small study area in northeast Greenland to develop a method for
mapping snow cover based on the temporal the radar backscatter during snowmelt and high resolution snow cover fraction observations from time lapse imagery. Traditionally remote sensing of snow cover has relied on the use of optical sensors to detect snow but these methods are limited by cloud cover and during periods of low solar illumination which is a problem in Arctic areas where polar night is present for a part of the year. As such, an approach that can offer snow cover mapping capabilities under such conditions would be advantageous as well as a providing a complementary data source.

The approach claims to be able to map both dry and wet snow cover but based on the fact that the method relies on the
relationship between SAR backscatter during the melt period (i.e. when snow cover is wet) and snow cover fraction information from high spatial resolution time-lapse imagery, I find it difficult to understand how the method can provide a solution for dry snow cover mapping. Detection of perennial snow and permanently snow free pixels suggests binary snow cover mapping, but this does not provide additional/improved information with respect to optical methods that can for example be used to derive snow cover fraction beyond the SOS/EOS period which is studied here.

Overall, the method seems very case study specific and it is unclear whether the same approach could be applied globally in other areas with seasonal snow cover. The time period of data acquisition (2 years)

is also limited and the data presented suggests large variations in SAR backscatter can occur from year to year. However, I do believe the results are worth publishing but the content should be revised to reflect that the method has until now only been applied to a limited dataset. Moreover I don't think the method will be useful as a standalone method for snow cover mapping due to the limited part of the year on which the method is based (i.e. snowmelt), but can certainly complement existing methods.

Specific comments

Abstract l.12:
"....enabling large-scale SC monitoring at high spatiotemporal resolution (20m, 6 days) with high accuracy" - this seems a somewhat bold claim given the small size of the case study area (45m2?) on which the method has been based.

Line 83: "...using adaptive thresholds" - the results would suggest that different thresholds have been tested but a fixed threshold of 4dB has actually been used to produce the snow cover maps using only the HV data. This statement needs revising.

Figure 4 illustrates the specificity of the method. The workflow diagram is very involved considering the small size of dataset and case study area. Moreover it is difficult to follow. A new method for estimating snow cover that uses SAR ought to be more generic to be of use elsewhere. The authors do not specify whether this was the objective of the work, or if the goal was simply to develop an approach which could be used solely for the area of interest.

Lines 280-281:
"... the t values for the best results dependent on the used polarization and the observed year" - suggests the need for an adaptive threshold to deal with the seasonal variations in backscatter due to for example, snow depth as alluded to in the discussion

Lines 366-367: "...could be used for a holistic hydrological monitoring of SC from the scale of a single catchment up to pan-Arctic observations" -

as highlighted earlier, I think this kind of statement/claim is somewhat bold, given that the method has been developed using such a small area of study and only two years of data. Furthermore the method has not been demonstrated on areas elsewhere.
This claim should be revised to something more realistic and which reflects the results of a dataset which is limited in both spatial and temporal dimensions.

Lines 386-387: "The optimal seasonal threshold value increases in accordance with snow depth" - I do not recall any snow depth data having been presented in the study, so this statement seems rather speculative until supported by data analysis.