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Reply on RC2

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Author 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-AC2, 2021

General remarks on the revision and the reviewer's comments:

Dear reviewers,

the authors appreciate your comments, and we thank you for your valuable work and the provided suggestions and comments on our contribution. We have addressed all your remarks and a point-to-point response (authors responses are in italic) to your questions/comments is provided below. The revised version of the manuscript is ready for submission, once requested.

From your reports we have identified the following major issues. Before the point-to-point response, we like to point out how these were addressed:

(1) Use of one orbit and potential use of EW?

An explicit goal of our investigation is to provide small-scale estimates of SC and related parameters and to study the temporal evolution of the SAR signal in relation to high-resolution in situ data, which provide snow cover fraction estimates. As such we have not considered the use of EW data as these (i) will not allow to study the small-scale SC heterogeneity due to their coarse spatial resolution and (ii) are not suited for the rather small sizes of the test sites (42 and 7 km² covered by the in situ cameras). We have further just used one relative orbit (IW), as acquisition geometry needs to be constant throughout the time series to ensure a comparability of the measurements. Nevertheless, we point out in the discussion that additional orbits (IW), if available, might be used to densen the time series; however, different orbits need to be analyzed separately, due to differences in local incidence angle and acquisition time. Nevertheless, we believe that EW data could be used with our approach for snowmelt detection and snow cover depletion mapping on larger scales with coarser resolution in future studies.

(2) Decrease in spatial resolution might lead to better results?

The decrease in spatial resolution might in fact lead to better results, as a generalization will cause a better signal to noise ratio (reduction of variance) and a better radiometric stability (less speckle noise when increasing the number of looks). However and similar to the first answer, we wanted to make use of the high-resolution in situ camera imagery and the Sentinel-1 IW data. As such, we explore if it is possible to estimate SC and related parameters on comparable small-scale using S-1 time series and to infer effects related to the SC fraction cover during the melt, i.e. a high-spatial resolution is inherently required to observe/characterize this processes due to the patchiness of SC during the depletion.

(3) Limited transferability ☐ use derivative instead of fixed thresholds?

Thanks for your suggestions on this. We have now included an approach that uses the derivatives of the time series and, therefore, operates more adaptively. It is presented along with the threshold-based approach. Results point out that accuracies similar to the ones achieved from the threshold-based method can be realized. As now discussed, an approach using derivatives is believed to be less sensitive to signal-differences caused by different conditions of the snowpack or the land cover. Therefore, the derivative-approach favors transferability.

(4) Limited transferability ☐ add another site to test the capabilities?

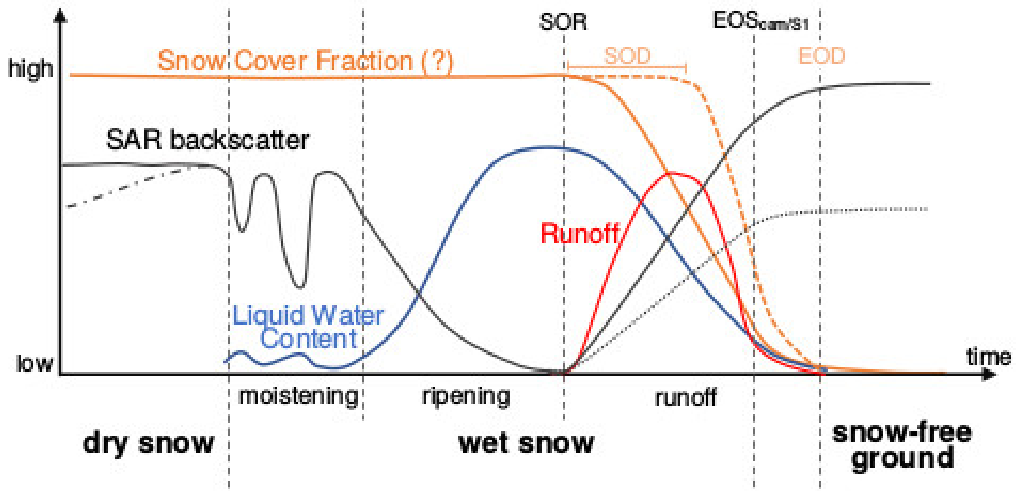
We have included a second test site and now show results also for the Kobbefjord region (Western-Greenland close to Nuuk). The Kobbefjord research area is, like Zackenberg, part of the Greenland Ecosystem Monitoring programme. Therefore, it offers a similar setup and also time-lapse camera imagery of the valley is available. As indicated in the revised version, we have repeated the entire processing of the camera imagery and of the Sentinel-1 time series for the Kobbefjord test site and we present results of both regions. Note that the environmental setting in Kobbefjord (low Arctic) is different to the setting in Zackenberg (high Arctic), which is also evident when studying the SC and its temporal evolution. Even though, the presented methods (threshold- and derivative-based approaches) perform well for both sites and produce reliable estimates, which compare well with the in situ measurements. For sure this is not a proof for a truly “global applicability” (which is also outside the scope of the contribution), but results confirm that the general design of the approach is not over-fitted but transferable.

(5) Factors influencing the threshold setting (vegetation, snow depth, soil properties)?

It is correct that factors influencing the setting for the threshold-based approach cannot fully be captured by the reference data available, as such their influence on the threshold setting itself cannot be studied, nor is it possible to explain the influence on the S-1 signal in detail. From the time lapse imagery, only the influence of the SC cover fraction on the backscatter can be compared and analysed, while information on snowpack properties is missing. This issue is now better addressed in the discussion and also points to future research needs. Note as well in this context that the derivative-approach favors transferability as it is self-adjusting and not linked to a fixed global (scene) threshold.

(6) Terminology and Abbreviations

According to your recommendations, we had a look at all terms used in our manuscript and redefined them to fit better along with terms used by other publications. We like to thank Reviewer 4 for the recommendation to use Fig. 1 for that. We added the parameters there and present the adapted graphic below. Besides, a table with all changed terms is shown below:



Old term	New term	Explanation
Start of snowmelt (SOS)	Start of runoff (SOR) (solely detectable by S-1)	Reviewer 3 correctly commented that this approach using the backscatter minimum as so-called start of snowmelt is actually not in line with other publications and the term might be ambiguous due to the different phases of snowmelt, i.e. SOS could also be at the beginning of the moistening phase / wet snow phase. As we use the backscatter minimum, the term is better described as SOR (start of runoff = point of time, where water is starting to leave the snowpack and either penetrates into the ground or causes surface runoff underneath the snowpack), which is in line with Marin et al. 2020.
End of snowmelt (EOS)	End of snow cover (EOS)	▪ This term was not criticised by the reviewers and there is no interference with

		other publications, as this is the new variable identified by our study. ▪ However, the term end of snowmelt is probably not optimal, because the time lapse images give only information about SC but not about melt. ▪ “End of runoff” (EOR) is also not fitting, because no corresponding validation is available from the time lapse images. ▪ What we actually try to detect is the “end of snow cover” and then visualize it in the snow cover depletion curves. Also only in that case validation with the time-lapse images makes sense. ▪ EOS definitions: (SC fraction falls below 50 % for time lapse imagery; S-1 time series meets threshold/derivative condition)
SOD (solely for time lapse imagery)	-	first observable decrease of SC fraction below 100 % in the time-lapse imagery for specific pixel
EOD (solely for time lapse imagery)	-	point in time when SC fraction in the time-lapse imagery of a specific pixel reaches 0 %
Perennial snow	End-of-season snow-covered	Reviewer 1 criticised the used terms, because they are not in line with the standard, as these areas might persist/be snow-free only for one single year. Hence, we renamed

them to better describe the state actual being observed.

Permanently snow-free Start-of-season snow-free

Please note as well that we have changed the title of the manuscript, which we now think is more precise. Please note as well that an additional co-author was added. Kerstin Rasmussen from ASIAQ joined, as she has maintained the time-lapse cameras in Kobbefjord and as she is an expert for environmental setting in Kobbefjord.

Yours sincerely and on behalf of all authors,

Sebastian Buchelt

Point-to-point response:

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.

Thanks for your comment on this. It is correct that the approach is focusing on the melt, as such we have highlighted this focus in the title (added "depletion"). Kindly also see our comments provided below and in the very beginning of the response.

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.

We have addressed the issue of the transferability in two ways: First, we have now included an approach that uses the derivatives of the time series and, therefore, operates more adaptively. It is presented along with the threshold-based approach. Results point out that accuracies similar to the ones achieved from the threshold-based method can be realized. As now discussed, an approach using derivatives is believed to be less sensitive to signal-differences caused by different conditions of the snowpack or the land-cover. Therefore, the derivative-approach favors transferability. Second, we have included a second test site and now show results also for the Kobbefjord region (Western-Greenland close to Nuuk). The Kobbefjord research area is, like Zackenberg, part of the Greenland Ecosystem Monitoring programme. Therefore, it offers a similar setup and also time-lapse camera imagery of the valley is available. As indicated in the revised version, we have repeated the entire processing of the camera imagery and of the Sentinel-1 time series for the Kobbefjord test site and we present results of both regions. Note that the environmental setting in Kobbefjord (low Arctic) is different to the setting in Zackenberg (high Arctic), which is also evident when studying the SC and its temporal evolution. Even though, the presented methods (threshold- and derivative-based approaches) perform well for both sites and produce reliable estimates, which compare well with the in situ measurements. For sure this is not a proof for a truly "global applicability" (which is also outside the scope of the contribution), but results confirm that the general design of the approach is not over-fitted but transferable.

Please note as well that the physical principle is the same, e.g. the characteristic seasonal pattern has been observed in the Alps (Marin 2020 et al.) as well as for other areas around the globe (Lievens et al.). Hence, they are expected to work well in other low vegetated areas. Parametrisation, however, must be adapted and adjusted to the local conditions. Further research is needed here, how the optimum parameters could be identified.

Beside the proposed approach, we think that results gathered by the use of the high-quality in situ data on the snow cover fraction provide an interesting merit, as these provide insights on the temporal evolution of S-1 data for the SC and its depletion.

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.

Agree, we adjusted the statement accordingly. This is clearly a case study now comparing two test sites (low and high Arctic). Additionally, note that analysing the backscatter evolution over time does require high-quality reference data, which of course can only have a limited spatial extent.

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.

Agree, we will revise the statement. As indicated above we have also included a method *based on the derivatives of the time series.*

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.

As recommended by the first reviewer, we included a part in the discussion to explain why we think this method is applicable also elsewhere, but that parameterization might be site specific. Please also note our statement on major issues in the beginning. Besides, we splitted Figure 4 for better readability into two parts as recommended by Reviewer 4: the first part shows the Sentinel-1 workflow and the second shows the workflow for the reference data acquired from the time-lapse imagery.

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.

We agree, an adaptive threshold could produce better results and should be studied in further research. We believe that especially in landcover types with strong SAR backscatter events driven by SC and low effects by other land surface processes, this could have great potential. As indicated above, and as also suggested by other reviewers, we have addressed this issue by introducing a method based on the derivatives of the time series.

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.

Thanks for pointing this out. We have revised the statement. In addition please note that a second test site was added and an approach using the derivatives of the time series was introduced.

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.

The only parameter that allows for a spatial assessment is the SC fraction cover data that we computed using the camera imagery. Higher SD is an observation made by another source, which we mention in the discussion. But indeed, no pixel based SD vs. threshold comparison could be made. Hence, it might be a possible explanation for the increase in the threshold, but is not assured. Therefore, we have removed the statement.