The Cryosphere Discussions

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Reply on RC1

Sebastian Buchelt et al.

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-AC1, 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?

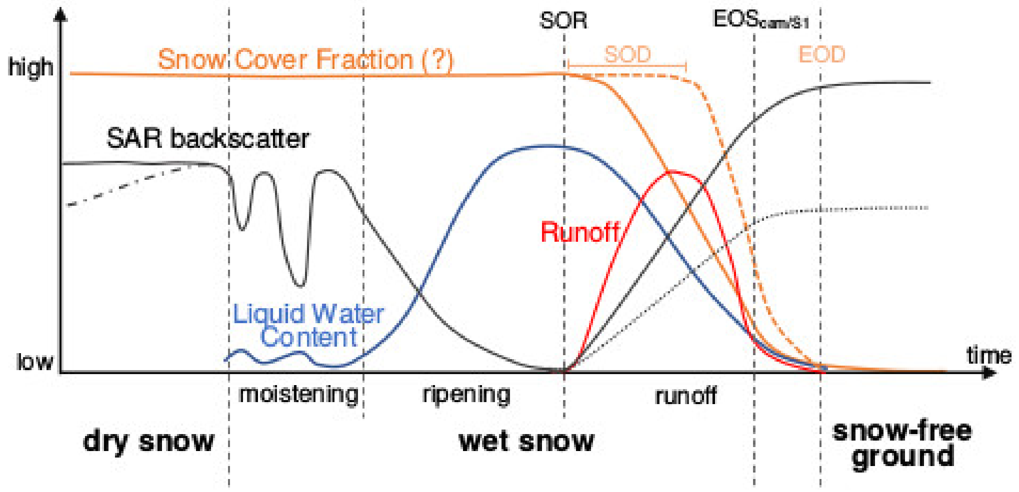
We have included a second test site and now show results also for the Kobbefjord region (Western-Greenland close to Nuuk). The Kobbefjord research area is, like Zackenberg, part of the Greenland Ecosystem Monitoring programme. Therefore, it offers a similar setup and also time-lapse camera imagery of 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

The paper studies a time series of Sentinel-1 images over Zackenberg valley, Greenland for snow melting and compares with high-res terrestrial optical data over an area. They use backscatter thresholds to identify start and end snowmelt and wet/dry/perennial snow status. They find that HV-pol data outperforms VV-data.

The paper uses S1 data at an interesting site and combine with high-resolution in situ data to validate their method development. This is valuable since S1 data is a very useful at high latitudes where optical data often fails due to cloud cover/darkness.

The authors suggest a method to retrieve snow cover (SC) at 20m spatial resolution. The method is based on finding the minimum backscatter for each pixel in the time series (SOS) where snow cover is expected to be 100% and subsequently the end of the melting season (EOS) when snow cover is 0% based on a fixed threshold 4dB above minimum. This method deviates significantly from the standard approach (Nagler&Rott,2000) relying on static reference images. A main obstacle is that the whole time-series need to be considered before SC-maps are made. Hence near-real time mapping is out of the question.

Yes, this is true. The approach is not suited for near-realtime mapping as the entire time series is needed for the estimation. Please note that the main objective of the study is the characterization of the spatiotemporal dynamics of snowmelt and related parameters.

It is somewhat unclear if the authors think that this approach is globally applicable, or only gives the best snow cover estimates for the current site. A discussion on the applicability of the method worldwide for various conditions (mountainous with variable local incidence angle, variable land types, forested areas etc.) would also be valuable. If the method has limited applicability outside Zackenberg, then why not only use the optical time series?

We agree with the reviewer that further assessment in different environments and locations is necessary. For this we have now included a second test site, Kobbefjord (low Arctic, West-Greenland). Further, we extended the discussion on the transferability, indicating that the method is developed based on the physical principle of SAR backscatter during snowmelt. Therefore it is independent from the test site as the physical principle is everywhere 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.). Parametrisation, however, must be adapted and adjusted to the local conditions. Further research is needed here, how the optimum parameters could be identified. Please note in this context that we now also present an approach based on the derivatives of the time series. This approach favors transferability as it is self-adjusting and not linked to a fixed global (scene) threshold.

I dislike the fact that the authors only processed one satellite geometry. Unfortunately, there doesn't seem to be systematic acquisitions with S1 for more geometries (SciHub). This could have been used to confirm results and/or improve the temporal resolution for the SOS/EOS. Also, variability in incidence angles could shed some more insights. An alternative could have been to also look at EW mode data (HV) since Greenland is covered by numerous geometries, but at the cost of lower spatial resolution.

Yes, it is correct that only one satellite geometry provides data in IW mode for Zackenberg. We agree that, even though, LIA corrected $\gamma0$ was used, an assessment of it could provide further detail on incidence angle dependence, however, we think this is right now out of the scope of the paper. Please consider that we now show results also for a second test site, having a different morphology. Further, 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). 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.

Overall, I feel that the paper looks at a somewhat limited time series (one geometry, two years) with good results. Using the minimum backscatter per pixel/year is interesting, but I believe more work should be delivered to convince readers that this can be applicable to other sites/landscapes. The paper has limited value if it is only applicable to Zackenberg.

The physical principle is the same, hence the method should be applicable elsewhere, as indicated above. 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. Nevertheless, we agree that extending this case study to other sites is important. Therefore, 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.*

Specific comments

20 m pixels lead to significant speckle noise. Authors should evaluate whether slightly lower resolution (e.g. 50m, 100m) could lead to better performance in general.

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, 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. to observe/characterize this processes a high-spatial resolution is inherently required due to the patchiness of SC during the depletion.

Although terrain-corrected gamma is used, it could be interesting to look at local incidence angles. If the variability in incidence angles is large, there may be a variability in the contrast of gamma during wet/dry-conditions as noted by e.g. Nagler et al., 2018, which could lead to a more variable result with respect to whether VV or VH is the preferred polarization. High local incidence combined with wet snow could lead to signals close to NESZ. By eye measure from fig 2 I suspect that the incidence angle for the site is around 30 deg. This is close to the range where Nagler et al. (2018) also state that VH is superior to VV, and the results are hence supported. However, perhaps some of the poor classifications could be explained if higher local incidence angles are involved somewhere in the sloping terrain?

Thanks for pointing this out. While the analysis of the LIA is of interest, we have decided not to address this issue in the revision: for Zackenberg, only one relative orbit is available (the Zackenberg Valles is located in the center of the second swath (IW2) and center incidence angle for IW2 is 38.7°), as such the possibilities to study LIA are very limited, also taking into account that our reference data is biased as the acquisition geometry of the camera prevents analysing all aspect and slope angles. For Kobbefjord more geometries (S-1) are available, however, here the same issues with respect to the camera position applies. Furthermore, snow distribution as well as vegetation and soil type depend on slope and aspect. Hence, identifying the individual contribution of the LIA would be difficult without considering these other effects in addition. A detailed assessment of all factors is, however, beyond the scope of our case study as well as is our study site too small to capture all potential effects. Nevertheless, we have included a statement on the role of the LIA in the discussion.

Figure 5a: Colours/symbols used to separate between time laps based/in situ based data for 2017 and 2018 do not correspond with legend. Perhaps use different symbols for each year like fig 8?

Thank you for the suggestion. We adjusted that accordingly.

The term perennial snow is used throughout the paper. In my view this is snow patches that persists over several years, whereas the authors redefine it as snow that does not vanish over one summer. E.g. fig 9 shows significant amounts of perennial snow in 2018 but not in 2017. I think a better term should be found. E.g. Snow does not melt for the current year. The same could also be invented about permanently snow-free pixels.

These, I suspect, are in general only snow free for the current season.

Thank you for the valuable suggestion regarding the terminology. We renamed the perennial snow class to end-of-season snow-covered and the permanently snow-free class to start-of-season snow-free to better describe the actual observation made. Please also see the general comment at the very beginning.

Technical matters

Line 374: temporal/spatial has been switched: should be … much higher temporal (1 to 10 days) and spatial (2.5m)… Perhaps also reconsider “much higher temporal” since S1 has 6 days temporal resolution? "Much" fits better on the spatial resolution.

Thank you for pointing that out. We adjusted it accordingly.