Reply on RC3

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-AC3, 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?
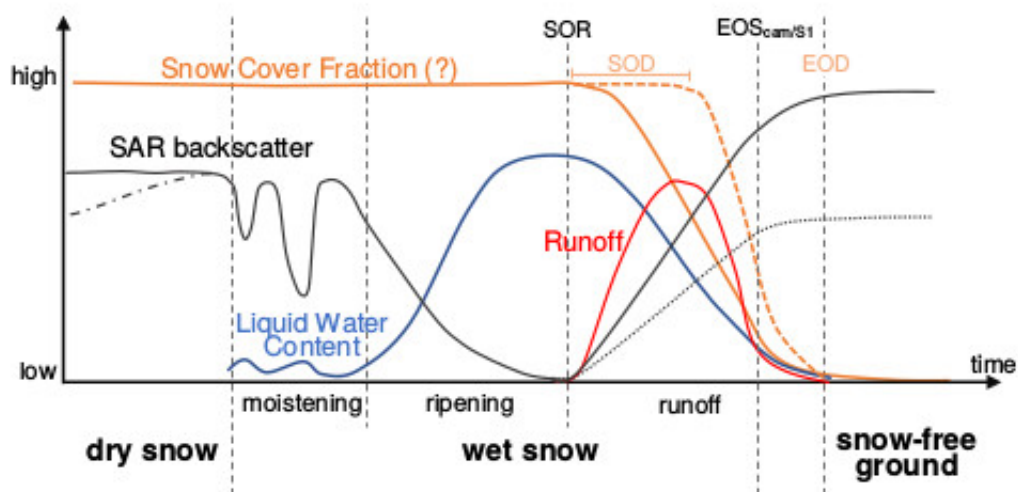
We have included a second test site and now show results also for the Kobbefjord region (Western-Greenland close to Nuuk). The Kobbefjord research area is, like Zackenberg, part of the Greenland Ecosystem Monitoring programme. Therefore, it offers a similar setup and also time-lapse camera imagery of 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:

The paper presents interesting findings on the behavior of C-band SAR backscatter during snowmelt over a region in northeast Greenland, introduces a novel algorithm to map snow cover and validates the results with snow cover derived from time-lapse images. In my opinion, this work is very relevant, timely and novel. In particular, a novelty is the focus on the mapping of the (wet) snow-covered area/fraction after the backscatter reaches its seasonal minimum until snow-free conditions. Most previous approaches have focused on detecting the (more early) onset of liquid water present in the snowpack (by thresholding the decrease in backscatter relative to a dry snow reference). There are however a number of aspects that could still be further improved.

Major/general comments:

One general concern is that the authors refer to the detected snow before the minimum in backscatter is reached as dry snow. Similarly, the minimum backscatter is defined as the start of snowmelt (SOS). However, before the minimum backscatter is reached, there is likely already a substantial amount of liquid water present in the snowpack (which causes the low backscatter), meaning that the snow can arguably not be defined as dry. Note that most other wet snow detection algorithms classify wet snow before the minimum is reached (as soon as backscatter becomes lower than a certain threshold below a dry snow reference). I'd recommend that early in the paper, the authors clearly specify what is meant here with dry/wet snow and SOS, and frame this in accordance with the literature. I'd furthermore recommend not to focus at all on the transition from dry to wet snow (also because this is not, and cannot be, validated with the available reference data as appropriately identified by the authors), and thus to limit the methods, analysis and discussion to the detection of wet snow-cover fraction (which I also believe is the main novelty of the paper).

Thank you for raising these important issues. We agree that the terminology should be revised and did so. Please note our statement on this in the very beginning of the

response where we outline the new terminology and that we have changed the title of the manuscript highlighting that the focus is on the snow cover depletion. Further, no differentiation between wet and dry snow is made anymore, as this is not the focus of our case study.

L73: "the snowpack is nearly transparent to C-Band SAR": Please modify this statement. There is considerable evidence that S-1 C-band observations, especially in cross-polarization, show an increase in scattering from dry snow accumulation by several dB. Accordingly, Fig. 1 should be modified to illustrate this increase in backscatter with dry snow accumulation. Also, the snow-free ground backscatter in cross-pol can sometimes (maybe not in this study region) be lower than the dry snow-covered backscatter in winter (depending on soil and vegetation conditions). This could also be mentioned.

Thanks for your statement on this. You are correct, we have adjusted Figure 1 accordingly (increase during accumulation; potential lower snow-free maxima). The increase in cross-polarization is mentioned now, even though hardly observable in our study site (1-2 dB maximum and barely above the observed variability of the signal at the spatial resolution we use (20m).

Some of the main problems of the study are the over-detection of perennial snow-free (because the threshold is too high for the more limited backscatter increase), and at the same time an under-detection of snow cover in areas where the threshold is too low. I'd strongly recommend to test using the derivative of backscatter over time to identify the EOS, rather than using a fixed 4 dB threshold. The increase in backscatter towards EOS may depend on the amount of liquid water in the snow, and the substrate and vegetation conditions, among other aspects. Thresholding the low derivative (low change in backscatter over time, following a strong increase) could potentially solve these issues, and may allow to find a more accurate EOS both in cases for which now smaller and larger thresholds (than 4dB) would ideally be needed.

Thank you for your suggestions on this. We followed your advice and 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.

Furthermore, it would be interesting to further investigate the regional differences in backscatter increase from the minimum towards the EOS, to determine its drivers. Is it mainly linked to snow depth, to the substrate type, topography, or vegetation?

While this is definitely an interesting point, 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 aspects and slopes. For Kobbefjord more geometries are available, however, here the same issues with respect to the camera position applies. Furthermore, snow distribution as well as vegetation and substrate type are also dependent on slope and aspect. Hence the contribution of each individual factor would be difficult to distinguish without considering the other effects. A detailed assessment of all factors is beyond the scope of our case study as well as is our study site too small to capture all potential effects.

Specific comments:

Please mention upfront in the paper that the algorithm proposed here is empirical.

Thank you for this suggestion. We have added this information.

L110: The use of S-1 observations in extended wide swath mode would have resulted in much denser time series (which in the discussion you state as a potential pathway for improving the results). Is there a specific reason why the IW mode was used? You could consider repeating the analysis with EW data (and simultaneously also investigate the combination of different orbits).

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.

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, more importantly, different acquisition times, which need to be considered. It might then be best to analyse each geometries time series individually instead of merging all data in one time series.

L110: Is there a reason why SLC S-1 images were selected rather than GRD images (which would have simplified the processing)?

This is just due to practical reasons, we first intended to add polarimetric features as well as coherence into the analysis, but then decided to stick with this simple and fast thresholding approach. Further, we have processed the data to gamma nought intensity using the terrain flattening approach implemented in SNAP and using the high-resolution Arctic DEM. As such we expect to have better radiometry and geolocation compared to the GRD product.

L176: "The threshold of 9 dB is set in accordance with our observations that HV snow-free summer intensity does not exceed the seasonal minimum by more than this value": this may well be the case in your study area, but in other regions (for instance prone to significant snow accumulation and melting, and/or followed by vegetation growth), I can imagine the increase can easily be larger than 9 dB. Please clearly state that this criterium may potentially not apply to different regions.

Thanks for pointing this out. We have modified the statement accordingly.

Figure 4 indicates that backscatter data are analyzed from March to August, whereas Figure 3 indicates May to October?

Seasonal minimum is searched within this period (March-August). For Zackenberg, In 2017 the search is limited only starting from May due to the limited dual-pol timeseries. As no camera images are available (Zackenberg) or used due to mountain shadow (Kobbefjord) before May and after October / September for comparison, the range is not shown even though Sentinel-1 images of the entire year are used.

L227: "areas with SOD before 1 June are excluded": how many areas are excluded by this

criterium?

Thank you for the specific question regarding the exclusion. After cross-checking our analysis, we realized that this exclusion was unnecessary. Solely start-of-season snow-free areas are now excluded. However, the results and interpretation have not changed, as the wrong exclusion due to our mistake had no significant influence on them.

L230: "Areas with 100 % and 0 % SC fraction were further segmented according to the temporal distance to SOD and EOD, respectively": this statement is not clear to me.

Thank you for pointing that out. We rephrased the mentioned sentence to clarify this step in our analysis.

L234: I'm not sure how useful the comparison of your threshold with that of Nagler's approach is. The latter refers to the decrease in backscatter relative to a dry snow reference, which is taken somewhere in summer to early winter (and it is not very clear when to best take the reference and what the impact of that timing is). Your threshold is used for the increase in backscatter after reaching the backscatter minimum. Furthermore, I don't think Nagler used gamma nought, and also focused on a different region (with potentially contrasting conditions in substrate, vegetation and snow properties). This should at least be mentioned if the authors still wish to include this comparison.

Thank you for this valuable comment. We mentioned the systematic differences between the two approaches now. Due to these differences, a detailed assessment of the thresholds is, in our opinion, even more important.

Is there a difference between EOS and EOD, and SOS and SOD? Please clarify.

As noted above, we addressed this issue and generally revised the terminology.

SOS: renamed as start of runoff (SOR), only detectable by S-1 time series.

EOS: end of snow cover (SC fraction falls below 50 %; S-1 time series meets threshold/derivative condition).

SOD: solely derived from the time lapse imagery, defined as: first observable decrease of SC fraction below 100% in the time-lapse imagery for specific pixel.

EOD: solely derived from the time lapse imagery, defined as: point in time when SC fraction in the time-lapse imagery of a specific pixel reaches 0 %.

Figure 6: Is this figure derived from averaging backscatter in space or just for a single location (I may have overlooked that in the description). In case of the former, how many pixels were included, and how does the averaging impact the trends. Also, was averaging performed in linear or dB scale?

Thank you for the comment. We collected all SC fraction values and backscatter intensity values of same-day acquisitions and rearranged them according to their respective SC fraction value. Furthermore, we identified the temporal distance to SOD and EOD for areas with 100 % and 0 % SC fraction, respectively, in order to capture the development of backscatter intensities before SOD and after EOD. Negative days indicate the number of days before the first observable decrease in SC fraction (SOD). Positive days indicate the number of days after the observed snow cover fraction has reached 0 % (EOD). Then the collected backscatter intensities were grouped by their respective SC fraction or temporal distance to SOD or EOD and averaged in dB scale.

Figure 6: It would have been interesting to see the full yearly S-1 backscatter timeseries (instead of only the period from Spring/May onwards). Perhaps the full time series could also give insight on the snow depth, which could be helpful for defining the thresholds in the snow-cover detection. How deep is the snowpack typically in that area? Further, the full timeseries would reveal if for instance the autumn/winter backscatter is also relatively lower or higher compared to the end of spring and summer backscatter. Now, it seems the maximum backscatter is always obtained in summer. Would that be primarily caused by the vegetation, or is the snowpack already wet (i.e., liquid water present in the snowpack) at the start of your analysis? I'd expect another minimum backscatter in autumn and an increase during winter (depending on the snow depth). Is this behavior observed in your study region?

Thank you for raising this interesting point. First, snow-free summer backscatter intensity is (in our study sites) generally above the winter backscatter values (co-pol and cross-pol) probably due to the effects of vegetation growth. We do see a decrease and a minimum of backscatter values in autumn, but usually they do not reach values as low as during spring, except for a few spots in single acquisitions. We assume that in such cases we have events of wet snowfall or snow on unfrozen ground which cause these low backscatter values similar to wet snow.

According to our observations, backscatter during winter shows hardly any changes. There might be an increase in cross-pol backscatter of 1-2 dB maximum, but it is really low and does barely exceed the observed variability of the signal at the spatial resolution we use (20m). Only for end-of-season snow-covered areas as well as areas covered by glaciers (outside the camera field of view) we observe a strong increase with refreeze. We conclude that there might be a chance that a backscatter increase along with increasing snow depth is observable, but is very limited due to lower snow depths (1.4m max) compared to observations made by Lievens et al. 2019, 2021). Further, also other effects not related to snow accumulation such as episodic snowmelt could strongly affect the backscatter (i.e. an increase in number and size of grains and ice lenses) which need to be considered.

The Figures below show the backscatter time series for 2000 randomly selected pixels:

Zackenberg   *Kobbefjord*