

The Cryosphere Discuss., author comment AC4  
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## Reply on RC4

Sebastian Buchelt et al.

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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-AC4>, 2021

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### 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 km<sup>2</sup> 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?

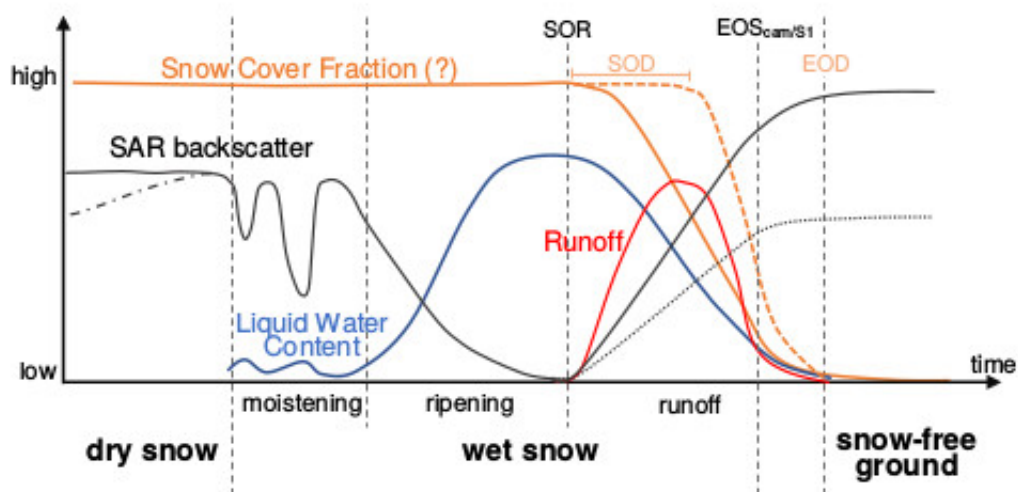
We have included a second test site and now show results also for the Kobbefjord region (Western-Greenland close to Nuuk). The Kobbefjord research area is, like Zackenberg, part of the Greenland Ecosystem Monitoring programme. Therefore, it offers a similar setup and also time-lapse camera imagery of 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)	<ul style="list-style-type: none"> <li>This term was not criticised by the reviewers and there is no interference with</li> </ul>

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

-

first observable decrease of SC fraction below 100 % in the time-lapse imagery for specific pixel

(solely for time lapse imagery)

EOD

-

point in time when SC fraction in the time-lapse imagery of a specific pixel reaches 0 %

(solely for time lapse imagery)

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:**

This study focus on two years of Sentinel-1 data covering a small part of the Zackenberg valley in northeast Greenland to develop an algorithm for mapping snow evolution during the melting season. This time series is compared with snow cover fraction observations from time lapse imagery. The physical background on which the proposed approach is based is already described in the literature and this work can be view as an interesting extension and validation of the findings by Marin et al. 2020. However there are still some aspects that should be further improved/clarified for being of interest to the scientific community.

General comments/concerns:

The title and the definitions are misleading w.r.t. the content of the paper (at least to me in the present form). In this context, the gamma nought time series can be exploited to find a time series of maps indicating snow status related to the loss of snow mass i.e., depletion curve. This is a important variable that can be extracted only with SAR information (differently from the snow cover depletion curve). However the definitions should be better described for avoiding confusion (making use also of fig. 1).

*Thank you for this very useful comment. We agree that the terminology should be revised and also made use of Figure 1 to better introduce and describe the used terms. Please note our statement on this above in the beginning of the response where we outline the new terminology. As well also note that we have changed the title of the manuscript highlighting that the focus is on the depletion.*

The use of only one track is limiting the understanding of the operational applicability of the proposed algorithm. Reconstructing the depletion curve with a sub-weekly sampling could be relevant in different contexts. This should be better analyzed and discussed.

*Please see our comment in the very beginning of the response. We have 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 as indicated by you; however, different orbits need to be analyzed separately, due to differences in local incidence angle.*

The thresholds adopted in the paper are derived from the dataset from which the reference information is available. The two considered years, which show different characteristics, already show a relative large variance in the results. Another independent test site(s) is necessary to fully understand the scope of applicability of the proposed algorithm. The use of fixed thresholds, even interesting to demonstrate the method, limit the generalization of the algorithm especially when more advanced methods are available.

*Thanks for commenting on this. We have addressed this issue twofold: 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, 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.*

Specific comments:

Fig.1: A slight increase of LWC can produce a high decrease in the backscattering. So it's unreal that in the moistening phase, cycles of increase and decrease LWC are not influencing the recorded backscattering. Moreover, the decrease in SCF does not correspond to the runoff onset especially for deep snowpack. Interestingly this curve was introduced for high alpine snowpacks. Do you have SWE measurements showing that the runoff onset is correctly identified by your time series? This would be an interesting extension of the paper by Marin et. al.

*Thank you for raising these important and interesting points. Figure 1 was adapted accordingly to your suggestion.*

*We agree that SWE measurements as a tool to assess the accuracy and meaning of SOR would be of great interest. However, our interest was to detect SC extent and its depletion as well as to assess the interaction between backscatter and small-scale SC fraction. So we decided to exclude SWE data. Further research could use available data from*

*permanently installed Snow Pack Analyzers, best case for more years to confirm the correct detection of SOR observations.*

Fig. 4 is rather difficult to be read. I suggest to divide it for section 3.1 and 3.2.

*Thank you for this good suggestion. We adapted it accordingly and split Figure 4 in two parts.*

The works of Lievens et al. on snow depth retrieval, even if showing the characteristic melting curve, state an increase in the backscattering due to snow accumulation that seems not to be noticed in your case given the comment of line 73. It would be interesting to know if you find the same behaviour in your experimental analysis and in case provide a comment on this aspect.

*Another reviewer criticised that such an increase is not mentioned here. We included it now. 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. We decided to not provide a comment on this due to the fact that our case study is focussing on a different time period of the year (melt season and not winter) and due to the limited number of winter observations (only one entire winter season (2017/18) available for each site due to the setup of our case study).*

Line 140: Small et al. in their last paper (<https://ieeexplore.ieee.org/document/9352976>) comment that the implementation of their terrain flattening in SNAP is not correct. This should be better commented in the paper.

*Thank you for raising this issue. We were not aware of this and mention this now in the manuscript.*

Line 305-310: do you have any measurements showing the increase in the superficial roughness?

*We agree with you that this would be very helpful to understand the underlying causes for the backscatter increase during runoff but, unfortunately, we do not have such data available for either of the two sites.*