

The Cryosphere Discuss., referee comment RC3  
<https://doi.org/10.5194/tc-2021-78-RC3>, 2021  
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## Comment on tc-2021-78

Anonymous Referee #3

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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-RC3>, 2021

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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).

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.

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. 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?

Specific comments:

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

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).

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

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.

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

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

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.

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.

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

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?

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?

L273-275: As mentioned above, classification by thresholding the change in backscatter over time (derivative) instead of the absolute intensity could potentially help with this.

L285: Would the authors also have a physically-based hypotheses on why the use of HV may be better than HH?

L287: The overall accuracy can be strongly impacted by the number of 0's and 1's. Have you tested for instance the Cohen's Kappa metric (also based on the classification matrix) which is less impacted by the numbers of positives and negatives?

L290-291: The authors mention that a high FP is due to the undetected snowfall on DOY177, but wouldn't this classify as a false negative (i.e. no snow detected by S-1, but snow on ground in reality)? Further, have you looked at how this late snowfall is impacting the backscatter? I'd expect it could increase the scattering if snowfall is dry, or decrease scattering if snowfall is wet?

L305: The paper by Lievens et al (2019) mostly focuses on the increase in backscatter during dry snow accumulation (during autumn to early winter). This is a different time period, which also shows generally an increase in snow scattering, but likely due to different phenomena (for instance, there the increase is likely not caused by an increase in snow surface roughness). The reference to Marin is more appropriate here.

L309: I'm not sure how much volume scattering there would still be. At this stage, the snowpack is very wet, and penetration (necessary for volume scattering) should be rather limited. I'd therefore expect that the increase in HV is caused by surface scattering with increased depolarization by the rougher snow surface.

L316: I think it might also be worth stating that your approach focuses on a different phase of the wet snow season than Nagler's approach (which is more focused on the initial detection of liquid water in the snowpack by the decrease in backscatter). So both approaches could be complementary.

L334: The fact that no independent validation is performed should definitely be mentioned earlier in the paper and be identified as a shortcoming of the analysis.

L338: I'm not sure if different orbits (with different incidence angles and different timings when combining ascending and descending tracks) is likely to increase the performance. The impact of the incidence angle should be further investigated (or at least recommended for future research). Denser time series would be available when switching to the EW mode over Greenland.

L345: Please mention that your method focuses on the transition of wet snow to snow-free, whereas the other referred approaches mostly identify the transition from dry to wet snow.

L358: Again, I find the wording of SOS ambiguous. There will already be much liquid water within the snowpack by the time the backscatter reaches its minimum value. Thus, one could argue that snowmelt starts much earlier in reality.

L361: I'm not sure you can say that dry versus wet snow is detected by the algorithm (only wet).

L364: The literature on SWE reconstruction could be improved (e.g. mention the work of Margulis, Bair, etc.)