

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-57

Achut Parajuli (Referee)

Referee comment on "Comparing rain-on-snow representation across different observational methods and a regional climate model" by Hannah Ming Siu Vickers et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-57-RC1>, 2022

In this research article entitled "Comparing rain-on-snow (ROS) representation across different observational methods and a regional climate model" the authors have used three methods i.e., WRF, seNorge, and SAR based ROS estimation for the Norwegian region. The authors have highlighted five sites of interest to validate their claim. Overall, the methodology to obtain ROS is properly explained and the manuscript is well-structured. The authors have devoted a significant amount of time and showcased detailed analysis to support their scientific claim. Having said that, there are several shortcomings in the present manuscript which deems considerable modifications before the acceptance of this research article:

- If I have correctly understood, the authors have used a 0.5° C temperature threshold to partition the rain or snow. However, I have skepticism about this rain and snow partitioning (Look at DOI: 10.1038/s41467-018-03629-7). In the already published research article (DOI: 10.1038/s41467-018-03629-7), a 50% rain-snow temperature threshold is more useful than a single threshold. And the referred manuscript shows variability even in the 50% rain-snow temperature threshold for Norway.
- The authors have used several methods to derive ROS which include the SAR, WRF, and seNorge. The authors have claimed that they have used SAR images to derive ROS as a novel scientific contribution. Also, the authors have concluded that SAR-based ROS detection works well in the snow accumulation period but has certain uncertainty in the spring melting period. I feel that the authors need to work more and provide some more novel findings for this work to be accepted in *The Cryosphere*. In my personal view, the authors need to present more information about precipitation (snow rain time-series for five selected sites) itself first. How rain or snow is partitioned for different models/methods used and the biases generated in these calculations? The authors are mentioning about the uncertainty in their results but failed to evaluate ROS uncertainty. Given the in-depth analysis presented in this research, If the authors can link these missing pieces, this could unpack deeper information regarding the ROS and their importance, which would be a valuable addition.

Below here is the line-by-line comments for the manuscript.

Section-wise comment

Abstract: The abstract is written very well, it is brief and informative.

Introduction: Several modifications are required in this section.

Could it be possible to mention what happens to snowpack due to rain-on-snow events somewhere in the introduction? Also, could it be possible to provide some background information related to the importance of ROS? Information about precipitation undercatch and biases resulting from wind-induced precipitation bias should be included in the introduction.

Line 27: Rather than high altitude and latitude are it possible to mention that frequency of ROS with climate change is more pronounced where snow is dominant.

Line 31 - 34: How does ROS have an impact on food availability mortality of reindeer (why specifically reindeer, not caribou or moose or other animals). Please elaborate on how ROS has such implications. Maybe extra rain energy from ROS will contribute to depleting snowpack cold content and then initiate untimely melt? Or for less dense snowpack when ROS occurs that might induce avalanche?

Line 46: Do you mean to say that SAR help detects the presence of liquid water? If yes please rephrase the sentence as " ... microwave more radar sensor that aids to detect the presence of liquid..."

Line 48 - 51: Could it be possible to split this sentence into multiple sentences?

Line 55 - 64: I think this paragraph is confusing and it deems reworking. I believe that the authors intended to highlight the uncertainty in ROS quantification when using the RCMs or satellite products. But supporting literature is missing. I believe, the SAR dataset has been used to understand the freeze-thaw mechanism which is mentioned in the paragraph below. If any information regarding ROS and SAR is not available then other supporting literature, for instance, a snowmelt study using SAR can be used as literature, and then the authors can support the novelty claim for this research. Apart from that, literature supported precipitation undercatch is also equally important in this section.

Line 65 - 67: Please merge this sentence to the above paragraph.

Line 67 - 70: In the sentence starting with "We use the Sentinel-1...." either merge this information within the sentence where the objectives of this study are mentioned or to the relevant section such as material and methods.

Line 70 - 82: I think the paragraph belongs to the material and methods section.

I prefer the study area section in between the Introduction and the dataset and methods section. You can, for instance, mention on average how long snow persists in Norway by dividing Norway into three or four classes (as per your convenience). Then from the available dataset from five highlighted sites, show the ROS every year and how different it is. Monthly ROS or total yearly ROS difference for five sites is also ok.

Datasets and methods: In general this section is good but some justification and/or addition is required.

Figure 1: Please add the legend depicting the sites. Also, I believe, the unit is m ASL.

Line 140: I am unable to figure out what are the authors referring to here "... when updates are made to the analysis methods".

Line 141: Could it be possible to inform the time step if it is daily, hourly or monthly?

Line 147: What sorts of adjustment is applied to correct precipitation bias? Please mention the relevant equations or literature.

Line 153: As mentioned above, since the authors are trying to understand the ROS does this threshold better reflect the real scenario?

Line 157: As the authors are using the seNorge model, I request the authors to showcase the equation of how liquid water content is calculated to derive the ROS event.

Line 161: Could it be possible to provide information on what sensors are used to measure

air temperature, total precipitation, and snow depth?

Line 166: Same comment as above about the use of threshold temperature and precipitation for ROS partitioning.

Section 2.5: I am confused here. How WRF, seNorge, and ground-based calculation is different in terms of ROS calculation. Could it be possible to elaborate? Is it due to the difference in snow persistence or the use of different thresholds?

Line 177: Same comment as above for the use of threshold temperature for rain snow partitioning.

Results: I request the authors to add more relevant information here and apply several modifications here.

Before section 3.1, I am curious to see how the precipitation time series unfolds here. I am in favour of some intermediate plots, say, for instance, the precipitation classification to rain and snow for ground-based observation, WRF, and seNorge as a time series for 5 highlighted sites. This will indicate how different are the proposed three methods for ROS classification.

Figure 2: In my opinion, rather than displaying the monthly values, it would be better to divide, say, for instance, the initial accumulation period (October - December), winter accumulation (January - March), and melting period (April to May). I suggest removing the July August and September altogether as we probably have less pronounced ROS. In this way, you can display all three outputs (SAR, WRF, seNorge) in the same plot. For the initial period, the increased frequency of ROS affects the snow accumulation in the ground. During the winter accumulation period, the ROS might trigger avalanche risk (if certain conditions are met) and during the spring the higher number of ROS may induce flood. We can also see how different ROS has been so far from 1957 to 2019 or any other period depending upon the author's convenience. This will give new perspective to present manuscript.

Figure 3 to 6: Same above comment applies to this figure as well. Also, the quality of these figures including figure 2 should be better.

Figure 7: Could it be possible to use the same scale for ground-based and WRF-derived ROS for this figure. The scale makes it difficult to compare the WRF and ground observation of ROS. Also, could it be possible to change the bar color of ground-based observation to something different than the present (maybe slightly divergent)?

Figure 8: Would it be possible to showcase the same sites as displayed in Figure 7 for WRF comparison. Also, could it be possible to use the same scale and change to a more divergent color?

Table 1: This is a nice table. It would have been more informative if you have provided a color scale for the table say for instance red for least accurate and blue for more accurate within the table itself (just a suggestion).

Assuming that the authors already have the modeling output for WRF, seNorge, SAR, and ground observation. It seems that the SAR-based analysis is a novel work. However, authors have outlined that similar research has been conducted by Pall et al., 2019. In that regard, I request the authors to add more inputs. One way to do that is to compare different ROS derivation criteria and in-depth uncertainty analysis presenting some additional plots. Could it be possible to prepare a plot relating the SAR liquid water detection and ROS specifically in the spring melting periods? I believe the snowmelt period can be derived by seNorge, by this way you can sort if the spring period snowpack influenced rain or snowmelt to detect water content by SAR. This will show us to what extent there is such an error. Is it possible to fully understand liquid water content in the snow whether it is from melt or rain or liquid water stored as gravitational forces from seNorge? If possible, seNorge could it be integrated into SAR for identifying the episodes of rain or snowmelt to improve SAR ROS output? A map or plot showcasing such integration would be nice.

Discussion: Several modifications are required in this section.

I found this section to be very brief rather than descriptive. Very little literature is used to support the author's claim. Several relevant literatures such as this one (DOI: 10.1038/s41467-018-03629-7) are completely missing. The authors are mentioning about the uncertainty in their results but failed to properly evaluate ROS uncertainty. It is common to use a threshold method to partition the ROS but I have skepticism even in the ground-based observation in this manuscript. I believe the authors need to add a solid foundation to strengthen this section.

Line 332: I am still skeptical about the threshold method used in this study to partition the rain and snow.

Line 335 - 336: Could it be possible to provide a cross-reference for this statement (Example table XXX, Figure XXX).

Line 348: Is it due to the temporal limitation of SAR or anything else? Please elaborate.

Line 349: Solar radiation among other equally contribute to snowmelt not only warmer air temperature.

Line 355: Is this the hypothesis or based on the author's experience? Please cross-reference if possible?

Conclusion: The conclusion is looking good but eventually it will be modified, I guess.

I hope these comments will improve the overall quality of this manuscript.

Best regards,

Achut Parajuli