The Cryosphere Discussions

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Comment on tc-2021-383

Anonymous Referee #2

Referee comment on "Rain-on-Snow (ROS) Understudied in Sea Ice Remote Sensing: A Multi-Sensor Analysis of ROS during MOSAiC" by Julienne Stroeve et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-383-RC2, 2022

Summary

The authors use a time series of atmospheric and surface geophysical observations to document the effect of rain-on-snow events on passive and active microwave remote sensing emission, backscatter, and radar waveform. The examined passive microwave frequencies (19 and 89 GHz) focus on two of the commonly used in sea ice concentration retrieval algorithms, and the active data are at the Ku- and Ka-bands found in current and planned radar altimeter missions used to infer sea ice thickness from sea ice buoyancy. The authors highlight the strong effect that a ROS event has on these remote sensing signals, and how changes in snow structure caused by ROS events are pervasive in their impact on passive emission and radar waveforms. They argue that there is an increase in ROS events on sea ice and that the topic is under-studied in that the community does not understand how these events contribute to sea ice geophysical retrieval errors. Data used are from the large, multidisciplinary, MOSAiC drift campaign that took place in the central Arctic from 2019-2020. The paper focuses specifically on data collected in late August and early September 2020. The paper is original and relevant to TC, and it should be of interest to the sea ice and snow readership. Major and minor comments are as follows.

Major

(1) Speculation about emission and scattering mechanisms: The authors use a large volume of data to document the rain event and the changes in snow properties that occurred during and after it. The MOSAiC project affords this opportunity, and the authors should be lauded for putting together such a detailed picture of the event as it happened. The impact of the event on remote sensing signals is well documented. However, despite the effort to incorporate so much detail, many statements made about the connections between snow property and microwave emission/backscatter/waveform are speculative. Examples include: on lines 242-245, where the downward percolation of water in snow is

"likely" attributed to a 12-15dB decrease in backscatter; and lines 246-250, where snow porosity is related to an increase volume scattering, yet porosity isn't examined and the authors express the need for more analysis. These speculative comments are not well enough substantiated by the data at hand, or by microwave scattering and emission theory and/or a modelling framework. While it is understandable that there isn't a lot of well-established microwave interaction theory dealing with such a complex scenario, there are still basic principles that would help drive the interpretation. For example, does it makes sense that the drainage of the absorbing water during the second rain event should lead to such a dramatic backscatter decline? Isn't the snow being wetted by absorbing rain? What is the expected penetration depth? What is the surface roughness contribution? Those look like structure from motion / photogrammetry targets in Figure 3; perhaps data on surface roughness are available and, if so, should be used.

In particular the paper needs to be focused more on the basic mechanisms driving the observed changes in backscatter and waveforms. For the active case, establishing the surface roughness and dielectric properties, and the relative contributions of surface and volume backscatter are important. MOSAiC datasets that help with this should be better utilized. Otherwise, so much of detailed analysis of various MOSAiC datasets, as interesting as it is, misses the mark in terms of guiding the interpretation and much of what we gain from the extensive analysis is consistent with what is already known to be the case from studies of terrestrial snow (i.e. what is introduced in lines 27-36).

(2) Cryosat-2 data usage: The authors compare their surface observations to Cryosat-2 backscatter and peakiness data to, as they suggest, to see how their results scale-up to the satellite scale. However, the explanations in lines 300-307 point to how the surface data do not scale-up, and the comparison is confusing overall. The observation that the satellite-based waveforms also change is correct, but given the unexplained discrepancy between what's observed at the surface and in the satellite data, it does not add much value to the paper.

(3) Winter ERA5 winter precipitation time series analysis: The authors use precipitation amount and type from ERA5 data to, as they state, expand the study beyond the time-period of the studied ROS event (i.e., winter period). Though the question of whether or not more ROS events in winter are occurring is important, the analysis doesn't effectively offer an answer to the question. The authors find an increase in the amount of rainfall during cold periods over the period of 1980-2020, but the amount of rainfall is, as stated, relatively small in magnitude. In order to make a connection to the studied ROS event, which took place in late summer and not during winter, the authors need to define an "ROS event" in terms of time period (e.g. number of consecutive days) and rainfall magnitude, then use the ERA5 data to assess whether or not these "ROS events" occur in the winter, and how much they have been changing over time. It is unclear from the precipitation amounts presented whether or not we would expect any impacts on snow properties and microwave scattering and emission behaviors that are comparable to the studied late summer event.

(4) Inferences about time series changes in snow properties during the ROS: In Section 3 there are lot of inferences made about time series changes in snow properties, using data collected from different positions on the sea ice floe. The authors acknowledge this on lines 218-220 where they state “This highlights potential spatial variability in snow conditions, yet it is difficult to separate spatial variability from temporal changes since the snow pits were not sampled at the same time.” One line 224 the authors then state that conditions are generally similar across the floe. Overall it reads like the authors are choosing to use spatial variability to explain some of the observed changes and homogeneity to explain others. As such, it is not very convincing what role the ROS events played in altering the snow physics relative to how much sampling spatial variability plays a role. An unbiased approach to the analysis is needed.

Minor Comments (by line number)

45: Delete “surface”

81: There are a lot of undefined locations in the Figure 1 map. Define them or, if they are not important, remove them.

91: The calibration was done several months before. Does this have any impact on the analyzed data, e.g. due to instrument drift?

106: HV, VV, and HH data are used in the analysis.

118: Choose better wording than “seeing”.

124: “..thick microwave absorber…”

130: physical temperature not absolute temperature

132: delete “zenith”

146: Was a manual weather observation program implemented during MOSAIC? Manual weather observations are a useful complement to these more sophisticated sensor-based techniques and add confidence to the estimations from them. With the stated goal of straightforward interpretation on line 150, manual weather observations would be very useful.

153: It would be better to clarify what time period is of interest earlier here (1980-2020).

164-175: Indicate how reliable the snow data are when sampled during melting conditions.

177: Before it was referred to as a ROS event. Now it is events. Clarify.

184: 13 September

199: Clarify what you mean by "below the snow/ice interface", i.e. what the SSL is in relation to surface snow and sea ice volumes.

205: define SSL earlier.

224: Explain the headings in Figure 4 in the caption (ROV, ALEBDO, etc.).

230-232: See major comment: would we expect VV>HH when backscatter is dominated by volume scattering?

242: See major comment: if the absorbing water is now drained then how does the backscatter decline so much in its absence? Wouldn't we expect an increase from water drainage, when the dielectric constant reduces and air and snow particles are snow scattering above the wet basal layer? Or is the wetness of the air-snow interface during the second rain event causing this effect? What is the expected penetration depth?

247: It is unclear what is meant by increasing porosity in snow pore spaces. Do you simply mean the pores are filled with water (during rain) then air (after refreezing)?

254: How does a glazed surface crust increase the dielectric constant? What is the increase compared to, cold snow? What about the surface roughness contribution to the observed change in backscatter?

265: Clarify what the green samples are.

278: That is not scaling up, which implies some kind of scaling function and consideration of spatial heterogeneity. It is comparing two different scales.

279: Define *peakiness*.

308: Section 4.2. is out of place since it refers back to Figure 5. Move the SBR data to this section, in a new figure, or move this analysis to earlier in the paper.

371: It is better to say “with a reduced brine volume” because a ROS event wouldn’t necessarily completely flush the snow of brine.

423: See major comment about ERA5 analysis.