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.

The authors appreciate the reviewer's constructive comments to the paper. Following are our detailed responses to your comments.

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.

We agree with the reviewer that some speculative statements are present in regards to backscatter and emission mechanisms. Some of these have already been addressed in our responses to the first review. We updated Figure 4 to highlight the percolation pathway of liquid water (see figure below). Note too that the relationship between porosity and density is reciprocal and we chose to show the density in the paper (and thus also indirectly the porosity).

We have also made the following edits:

"*During the first ROS event, radar backscatter declines at both frequencies and all polarizations as a result of increasing signal attenuation by liquid water, due to greater dielectric loss [Ulaby and Stiles, 1980]. The decline is larger at Ka-band due to stronger sensitivity of higher frequencies to snow surface changes: Ka-band backscatter reduces ~12 dB (at both VV and HH) and ~18 dB (HV) at all incidence angles. At Ku-band, VV and HH remain relatively stable at nadir but decrease by ~7 dB (at both VV and HH) and by 10 dB (HV) at 45 degrees. Soon after the onset of melt and the second ROS event, the snowpack transitions to a funicular regime (i.e. liquid water occupies continuous pathways through the snow pore spaces - see pink areas corresponding to the water percolation paths in the Micro-CT images in Figure 4. This results in downward percolation of liquid water via gravity drainage [Colbeck, 1982a; Denoth, 1999], resulting in a completely saturated snowpack. This likely leads to the large backscatter decline during the melt onset and the second rain event. Ka-band backscatter declines by ~12 dB, while Ku-band declines by more than 15 dB, at all polarizations - the decline is larger at nadir. The steeper decline at nadir from the first ROS event suggests stronger signal attenuation likely from ponded/slushy snow surface directly in front of the radar, possibly due to rain water dripping from the KuKa antenna horns, though this cannot be confirmed. Overall, Ka- and Ku-band backscatter during the ROS event fell 4 and 6 standard deviations (16.5 and 19.3 dB), respectively, outside the observed backscatter variability before the ROS event.*"

"*After the snowpack refreezes, HH and VV nadir backscatter increases approximately 20 and 25 dB at Ka- and Ku-bands respectively, and remains slightly higher than before the ROS (see pre- and post kernel density plots). This indicates an electromagnetically smoother air/snow interface due to surface refreezing, resulting in stronger surface scattering, leading to a relatively greater nadir backscatter than observed prior to rainfall. At 45 degrees, the difference in Ku-band VV and HH backscatter increases slightly by up to 1.5 dB, compared to nadir, and the angular dependence for Ku-band HV almost vanishes. This indicates a combination of dominant Ku-band surface scattering from the air/snow interface and some additional scattering from dense layers slightly below the refrozen snow surface. In contrast, the angular dependence gradually reduces for Ka-band VV and HH, suggesting dominant surface scattering from the increasingly colder and refrozen snow surface.*"

For the microwave emission section we make the following revisions:

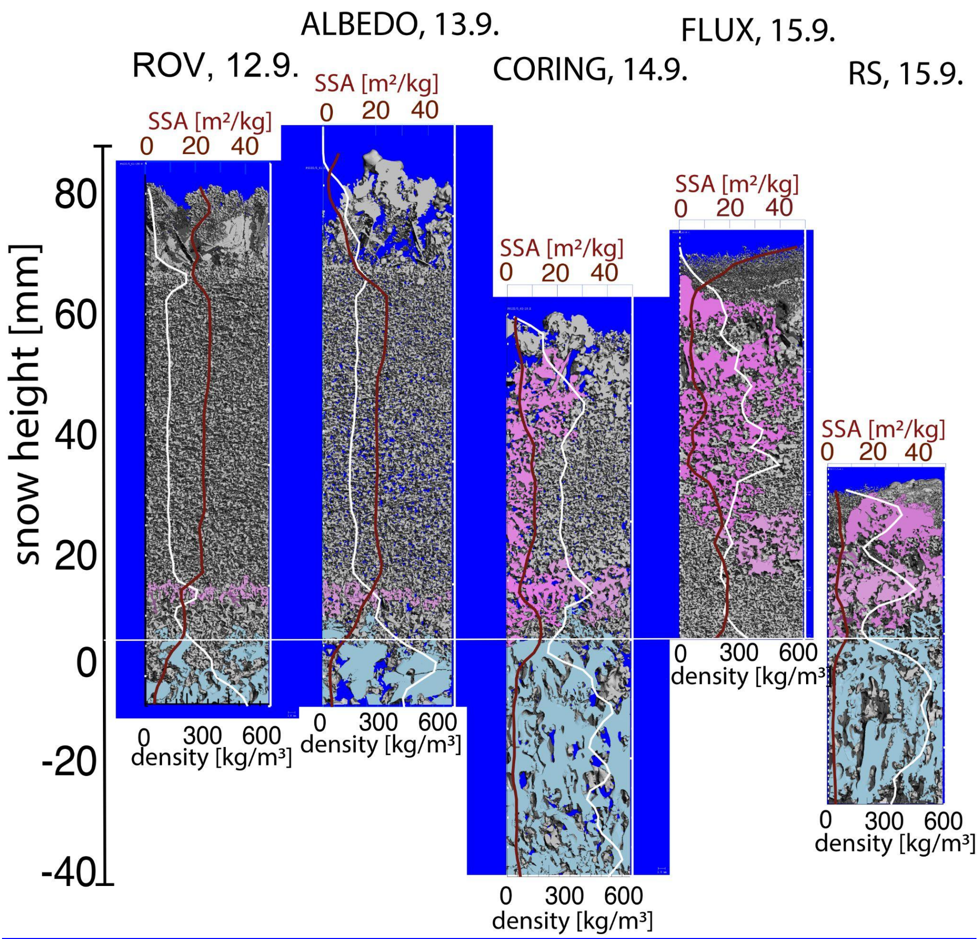
"This also suggests that at the time the data were collected, the entire snowpack is wet since the 19 and 89 GHz channels are equally impacted, and thus the emission is originating from the top of the wet snowpack at both frequencies."

"After the event, the snowpack remains cold, yet it is altered. In particular, the 19 GHz polarization difference (PD) (i.e. PD = Tb19V - Tb19H) is larger than before the ROS event, increasing from 14 to 17 GHz after the entire snowpack is refrozen (e.g. between September 16 and 18). This increase is likely the result of ice layers in the snowpack (e.g. high density

layers shown in Figure 4(e)); the 89 GHz PD on the other hand decreases from 9.4 to 6.1 GHz. Further, grain size increased throughout the snowpack, and thus Tbs are lower than before it rained (more volume scattering), affecting both 19 and 89 GHz, which decreased to 206 ± 4.2/224 ± 2.3 (19H/V) and 181 ± 9.4/187 ± 8.02 (89H/V) GHz. The larger standard deviation at 89 GHz is likely because this frequency is particularly affected by snow grain/structure scattering. Further, the 89 GHz channel is also impacted by atmospheric downwelling radiation, and small temperature fluctuations at the snow surface will influence penetration, leading to temporal fluctuations in Tbs."

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?

After analysing the micro-CT images, we found water percolation paths (see darker pink marks in revised Figure 4) after the melt onset and during the second rain event and the snowpack transitions to a funicular regime (i.e. liquid water occupies continuous pathways through the snow pore spaces. This results in downward percolation of liquid water via gravity drainage [Colbeck, 1982a; Denoth, 1999], resulting in a completely saturated snowpack. This likely leads to the large backscatter decline during the second rain event. See above modifications to the text.



What is the expected penetration depth?

The penetration depth, δp at which the radiation power falls to 1/e of its value just below the surface, can be calculated in theory if the physical properties such as density and moisture content, which influence the dielectric properties of the snow, are known. In this study we do not have that information available from the area scanned by KuKa, and hence do not calculate δp. We feel that the key piece of information for using KuKa to understand the effect of ROS on retrieved elevation from an altimeter is rather the change in waveform shape and/or a shift in the dominant scattering surface. Hence, we discuss how these change during the time series both for KuKa and CS2, indicating in particular that the peakiness (as an indicator of waveform shape) changes, with the impacts we discuss.

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.

We do not have roughness during the ROS event since it was too cloudy to provide the necessary contrast in the roughness calculations. The roughness measured on the 15th at the FLUX and RS sites differ and we do not have any way to know if the radar footprint is closer to one or the other. Below are the roughness values before on the 12th and on the 15th that were obtained. We are not including modelling in this study and given that the radar backscatter is extremely sensitive to roughness and we cannot be sure of the roughness value directly underneath the radar instrument. Nevertheless, the surface roughness between the 12th at the ROV site and the 15th at the RS site decreases which is consistent with the increase in observed backscatter. However, given how sensitive the backscatter is to roughness and we cannot be sure of the roughness directly below the instrument we felt it best not to add a discussion on roughness for this study.

Device_Operation_ID	Location	Timestamp	Lat	Lon	RMSh_mm	CorrelationLength_mm	RMSh/CL
PS122-5_61-198	SNOW5_ROV	12/09/20 5:00	88.76906	101.66249	1.82222003	34.43189967	0.0529224358
PS122-5_62-39	SNOW5_FLUX	15/09/20 4:45	89.07166	107.30898	2.37603723	31.02376538	0.0765886082
PS122-5_62-44	SNOW5_RS	15/09/20 9:10	89.06394	107.67127	0.85453922	23.29952404	0.0366762522

In particular the paper needs to be focused more on the basic mechanisms driving the observed changes in backscatter and waveforms.

We have added some information on the way the radiation interacts with the snow to explain and contextualise the waveform analysis:

"Properties such as interface roughness and dielectric contrast, as well as the frequency of the radiation, will influence how much power is detected from each location within the snow and ice. For returned power to be detected, the radiation must both penetrate through overlying layers and be scattered back in the direction of the receiver. The waveforms are therefore useful to determine where radiation penetrates to, and is scattered from, to investigate the

waveform shapes (distribution of power vs range and how this relates to geophysical changes) and for interpretation of backscatter values."

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).

We have previously worked to model the relative contributions of surface and volume scattering with SMRT but the problem is that there are uncertainties in the inputs to the model that are just impossible to overcome especially in regards to surface roughness directly under the radar footprint and there are many inputs that can give the same outputs. Data on surface roughness or dielectric properties were not collected within the KuKa footprint so the uncertainties are just too large here. We also are not able to give details of, for example, how the air/snow interface roughness varied using a laser scanner/similar because these data were not collected. In addition, the modelling aspect is truly beyond the scope of this paper, which focuses on a unique set of observations.

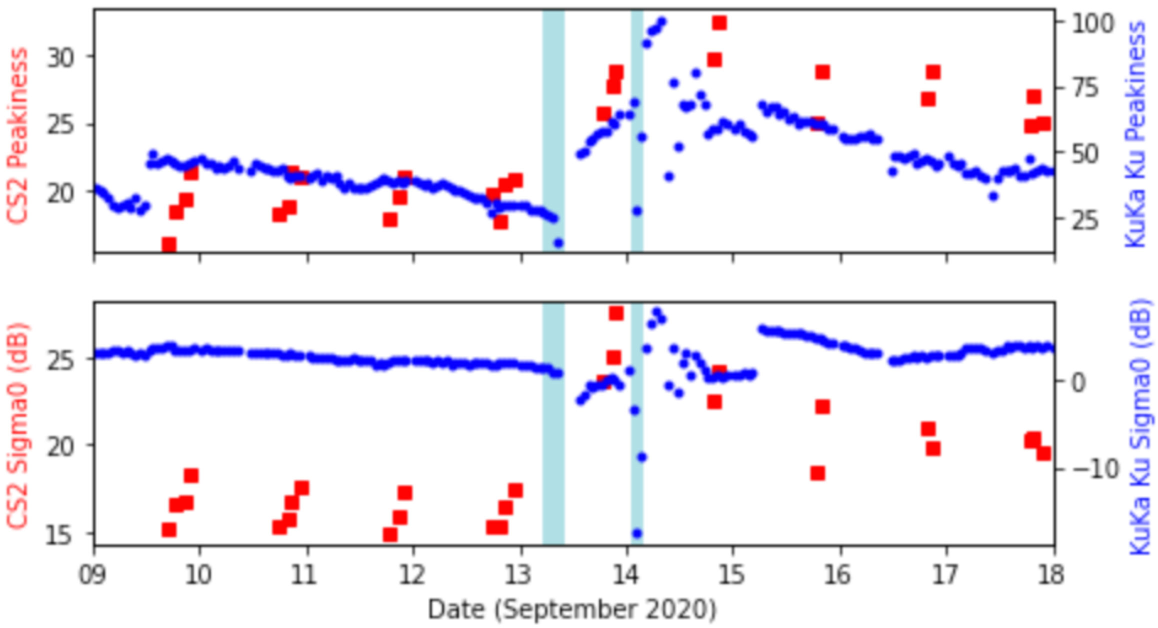
Our plots nevertheless demonstrate how the relative contributions of backscatter from interfaces and volumes varied during before, during and after the ROS. We acknowledge the reviewer's comment regarding the information to be gained from terrestrial snow studies, but still feel that the demonstration of KuKa and CS2 waveform shapes changing in the same way (using peakiness to characterise this) shows a unique comparison between ground- and satellite-borne radar altimeter instruments over sea ice, and hence how this meteorological event impacted the radar instruments operating at these two scales.

For dielectric properties, only the snowpits from the 14th contained liquid water content as the pits collected on the 12th and the 15th had temperatures below 0C. The snowpit on the 13th also shows sub-zero temperatures, but that was taken at the beginning of the first ROS event and it would have likely changed later in the day. Thus, we argue there is clearly liquid water in the grayed area in Figure 2 and no liquid water outside the grayed area.

(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.

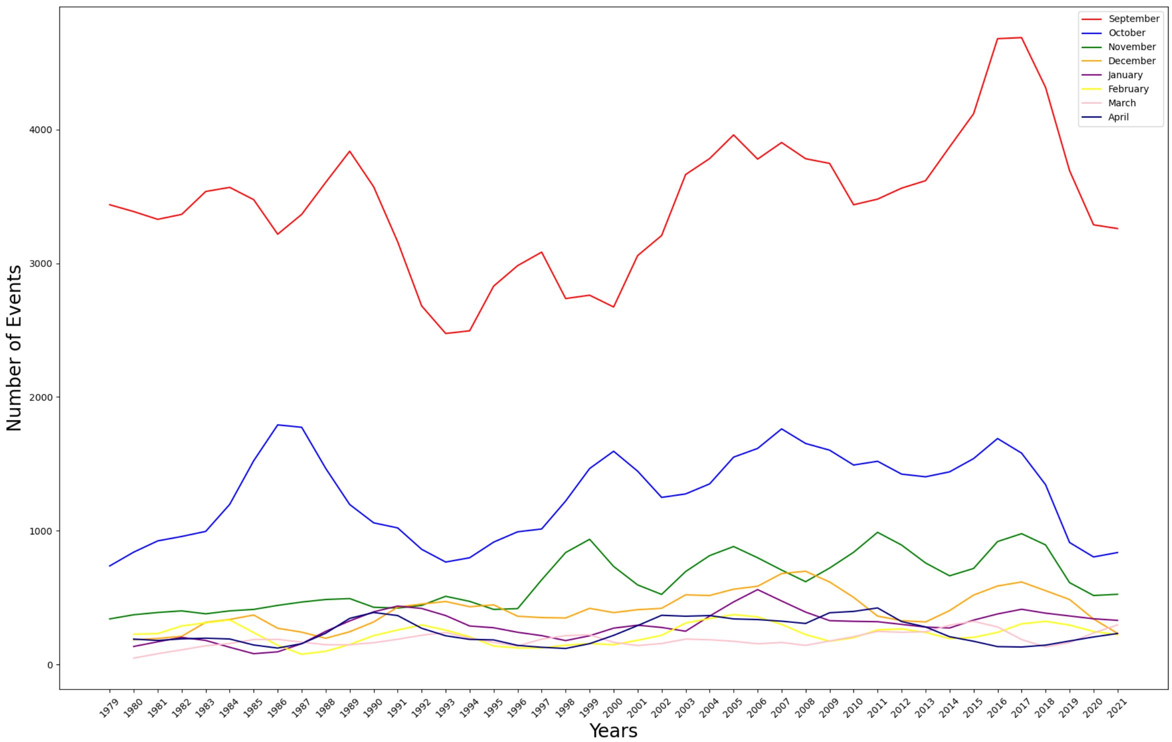
It is true that the sigma0 NRCS values and peakiness do not change in the same way. We are not sure how to add information here as we note this in the text and are not able to explain these changes beyond what is written. We do offer an alternative plot here for the reviewers/editor to look at, on which data are averaged at one hour intervals, instead of 24 hour intervals. The spread in CS2 sigma0 values is likely caused by the effect of CS2 data collected along tracks (at different times) at different latitudes/longitudes, as shown in Figure

7. What the new figure (below) shows is that CS2 does appear to see the ROS event a little before KuKa, which makes sense given the direction of the precipitation coming from lower latitudes (and CS2 tracks are at lower latitudes than KuKa). However, as stated in the paper, the 130 km limit imposed on the distance from CS2 to KuKa means that the CS2 data only cover late afternoon to evening each day. Therefore, there is not good coverage of CS2 within each day, and the KuKa data show that changes take place on very short timescales which are therefore missed by CS2. Note that KuKa was moved on 15th - hence jump in values. If this plot is felt to be more useful, we could include it instead with minor alterations to the text.



(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.

What we see in the September ROS event is a convolution of the rain and the warm air intrusion and longwave heating of the snowpack. The large change "during" the second event is not necessarily due to the rain, but to the associated warming that accompanied the event. Warming is obvious during ROS events, but we could have a similar response without rain, which we do mention in the paper, such that a ROS event could also represent liquid water in the snowpack from air temperatures going above 0C. The interesting thing with the rain event observed during MOSAiC is that you see the response in the signal immediately, which helps identify the exact timing in the signal (of the first rain event). That said, we find the comment is extremely picky, and the ERA5 analysis is fine as it is for the purpose for this paper. There are not enough studies presently on what magnitude of rainfall and/or combination of rain and warming is needed for these events to be detected in passive and active microwave signatures. A future study will focus on identifying these types of thresholds with ERA5, and evaluating if we see similar impacts in satellite data. But this is outside the scope of the present paper. Our results are presented in the context of how ROS changes over time (regardless of amount), and our results should also be considered in the context of other studies that have shown more frequent and longer duration winter warming events. We did look at monthly rainfall event changes over time for each month from September and April using a liquid water threshold to match the liquid water precipitation during the September event (see below plot). The data plotted are for single events (events that occur in the same spatial area, not each grid cell – i.e. each event was identified by rooks method requiring at least one event to be in an adjoining grid cell). If the editor feels we need to change the figure we could update based on a threshold for liquid water to be the same amount as we observed during the MOSAiC expedition, but we do not feel this really adds value to the present study, nor do we know the exact threshold that would result in a response. We of course observe that September has the largest number of rainfall events compared to the other winter months, though certainly events do happen also in October and November, though only December and March had statistically significant trends at the 95th percentile.



(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.

We largely disagree with the reviewer that we are doing a biased analysis. The impact of the ROS on the backscatter and emission are large and since it is also seen in the satellite observations, it was observed across the entire floe and at even larger spatial scales. We have updated the microCT figure in response to Reviewer 1 which highlights both the similarities and slight differences between cores taken on the same day (i.e. at the RS and Flux sites). We of course cannot sample directly under the in situ instruments as that would disturb the snow surface and thus we have to work with the data that was collected and the snowpits provide a broad sense of conditions encountered. Yes, there is some spatial variability but there is also general consistency seen in the snowpits, and the time difference between the FLUX and RS snowpits on the 15th could in part explain differences in those profiles. Even so, the large changes we see in the radar backscatter and the microwave emission are driven by the rain/melt event rather than slight density differences between snowpit locations. Further, the distances between the sites were 100-150n at most, and it is very unlikely that it would rain at one site and remain dry and cold at the other.

Minor Comments (by line number)

45: Delete "surface"

Done

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

We added the relevant locations on Figure 1.

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

According to the manufacturer, the calibration should be stable for long periods. No calibration was made during leg 5 by the crew manning the RS site. However, we saw from leg 2 that the calibration was stable over the entire leg 2 time-period. Comparison of calibration coefficients by the manufacturer using calibration data conducted during Leg 2 and after MOSAiC show that the instrument was stable throughout the expedition. Further, calibration impacts the absolute values of sigma0, but we are focused on the relative changes in backscatter and radar waveforms, and these are large changes (many orders of dB) from the ROS, relative to any small calibration changes or small backscatter offsets.

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

118: Choose better wording than “seeing”.

We have replaced ‘seeing’ with ‘microwave emission from the sled’

124: “..thick microwave absorber…”

corrected

130: physical temperature not absolute temperature

Changed to physical

132: delete “zenith”

deleted

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.

The rain and the warming up were also “manually observed” from a surveillance camera deployed at the RS site, however it is more scientific to use actual data collected through instrumentation.

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

We clearly state that we use data from 1980 to 2020 to overlap with the MOSAiC ROS event and put it into context with how ROS is changing since 1980. We don’t feel we need to move the time-period mention to earlier in the paragraph as first we say why we use ERA5 before mentioning the time-period we focus on.

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

We did not use unreliable snow data during wet conditions (such as SMP inferred density/SSA, which are currently not valid for wet conditions). Therefore, all data that we present here are valid for wet conditions. For example, the comparison between manual density measurements and microCT shows excellent agreement (Figure A1, Appendix). Likewise, the comparison between manual snow surface temperature and surface temperatures from AWS agrees (comparison not shown).

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

There were two ROS events, we now specifically say 2.

184: 13 September

Changed as suggested

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.

This was a typo. We have rewritten it to read “*except just below the snowpack*”.

205: define SSL earlier.

We added this sentence after the first mention of the SSL: “*The surface scattering layer is an anisotropic snow-like structure that forms the surface of melting sea ice and originates from sea ice. In this study, it is the layer between snow and solid ice*.”

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

We added this sentence to the caption of Figure 4: “*The samples were taken at the following locations: ROV, ALBEDO, CORING, FLUX.*”

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

Yes, at higher incidence angles, greater VV than HH is caused by volume scattering from the snow grains. To avoid confusion, we have added a reference from Tjuatja et al. (1992) to justify our observation.

Tjuatja, S., Fung, A. K., & Bredow, J. (1992). A scattering model for snow-covered sea ice. IEEE transactions on geoscience and remote sensing, 30(4), 804-810.

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?

After further analysing the micro-CT images, we found water percolation paths (see darker pink marks in revised Figure 4) during the second rain event and the snowpack transitions to a funicular regime (i.e. liquid water occupies continuous pathways through the snow pore spaces. This results in downward percolation of liquid water via gravity drainage [Colbeck, 1982a; Denoth, 1999], resulting in a completely saturated snowpack. The liquid water is not drained from the snowpack, but it percolated within the snowpack. This likely leads to the large backscatter decline during the second rain event.

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)?

We removed this phrasing in the revised manuscript during reply to reviewer 1. Instead this paragraph now states:

"*After the snowpack refreezes, HH and VV nadir backscatter increases approximately 20 and 25 dB at Ka- and Ku-bands respectively, and remains slightly higher than before the ROS (see pre- and post kernel density plots). This indicates an electromagnetically smoother air/snow interface due to surface refreezing, resulting in stronger surface scattering, leading to a relatively greater backscatter than observed prior to rainfall. At 45°, the difference in Ku-band VV and HH backscatter increases slightly by up to 1.5 dB, compared to nadir, and the angular dependence for Ku-band HV almost vanishes. This indicates a combination of dominant Ku-band surface scattering from the air/snow interface and some additional scattering from dense layers slightly below the refrozen snow surface. In contrast, the angular dependence gradually reduces for Ka-band VV and HH, suggesting dominant surface scattering from the increasingly colder and refrozen snow surface*."

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?

On the 15th, after the rainfall ended and temperatures dropped below freezing, and the snow pit sampled from the RS site showed a completely refrozen snow pack. We have removed 'glazed surface crust' in the revised manuscript.

As discussed in response to major comment above, after the snowpack refroze, HH and VV nadir backscatter increase "*indicates an electromagnetically smoother air/snow interface due to surface refreezing, resulting in stronger surface scattering, leading to a relatively greater nadir backscatter than observed prior to rainfall".*

However, we don't have the actual roughness measurements at the ROS site before to verify this, and given the sensitivity to roughness in modeling the radar backscatter we do not feel we can discuss the surface roughness contribution to the backscatter in the paper.

265: Clarify what the green samples are.

Unclear what the reviewer is commenting on as in the original version we clearly stated that the green samples were selected from the first rainfall event. We have however revised the figure in response to Reviewer 1 so we now write:

"*As we saw with the backscatter changes, the waveforms are stable before the first ROS event (e.g. black sample, 09-13 00:25 UTC) and power is returned from the air/snow interface and also from below. During the rainfall event (dark green sample 9-13 08:49 UTC) the power from the air/snow interface has reduced but the echo shape is otherwise similar.*

Following the first rainfall period (magenta waveforms, 09-13 13:58 UTC), most of the power is returned from the air/snow interface and the power returned from ranges greater than ~1.6 m drops in both bands, causing a shadow-like region of lower power in the echograms. Note *also the shift in the peak return in the green samples relative to the black ones in the inset towards shorter range at Ka-band. During the second ROS event (cyan and brown waveforms on 09-14 01:10 and 02:06 UTC), the peak power associated with the air/snow interface decreases and there is a further shift to shorter ranges, now also evident at Ku-band.* "

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

We of course are not scaling up the measurements from KuKa radar but see how results are applicable at the satellite scale. While this comment is rather nitpicky, we nevertheless changed the sentence to read: "*To see how these results are applicable at satellite scales, we investigated the change in CryoSat-2 waveforms during this time period…*"

279: Define peakiness.

Peakiness was already defined in the paper as: "K*uKa Ku-band peakiness is computed by taking the subset of range bins above the noise floor (-70 dB) and dividing the maximum power by the mean power in those bins, to calculate a peakiness value for each 24-hour period using a similar methodology to CryoSat-*2".

It is unclear what else the reviewer wants here; we have slightly rephrased in the revised paper to try to clarify: *"KuKa Ku-band peakiness is computed by taking the subset of range bins above the noise floor (-70 dB) and dividing the maximum power of any of those bins by the mean power averaged across all of them, to calculate a peakiness value for each time interval using a similar methodology to CryoSat-2"*

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.

We prefer to keep it as it is since it's good to show the SBR data together with the backscatter data on the same plot as the synergy between the observations can be important for future studies that try to combine information from several sensors.

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

We didn't say it completely flushed the snow of brine, we said it can flush brine from the snowpack, thereby freshening the snowpack. The amount of freshening is subjective. But we now change the second to read: "*We hypothesize that, upon refreezing, a reduced brine volume and colder snowpack allows greater penetration of radar signal. Thus, the error introduced by snow salinity is reduced*."

423: See major comment about ERA5 analysis.

Citation: https://doi.org/10.5194/tc-2021-383-RC2
See responses to major comment.