General Comments:
I have no general remarks. You improve the manuscript substantially, discussed most of my points sufficiently well, and I do not see an added value to insist on a more elaborated discussion whether the ROS event discussed in your manuscript is representative for winter conditions or not. You noted that you don't see the point here because the "official" start of winter in October was just two weeks away. But this was not my point. I was referring to winter conditions in general, and more the mid- to end-of-winter conditions - aka January through April - where snow thickness is larger, where ice is colder and where air-temperatures are colder as well. But fine. Readers can judge by themselves whether this ROS can be seen as a representative case of "typical" winter-time ROS events or not.

We thank the reviewer for their thorough comments. We have responded and made the suggested edits (see below).

Specific Comments:
L139/140: "Note, the 37 GHz ..." and your reply to my respective comment in the 1st review --> Yes, I do and already did believe this. I was just wondering whether you would specify what you mean by unstable, e.g. "erratic TB fluctuations of the order of 50 K or even more during observations periods when the other two frequencies revleard TB variation less than 5 K". But you decided to not let the reader know this detail. Fair enough.
We didn't feel it was necessary to show the figure of how the 37 GHz channel behaved. However, we now state the following to satisfy the reviewer.
Note, the 37 GHz channel was unfortunately unstable, showing at times unphysical fluctuations of more than 50K and thus we only focus on results at 19 and 89 GHz.

Figure 4, caption, Line 5: At least I have difficulties to clearly separate darker pink from lighter pink and have (still) difficulties to see the 10 mm (only) thick refrozen icy layer by means of the level of pink. But I note the peaks in density at around 10 mm and 30 mm for the RS site on Sep. 15 and at 35 mm at the FLUX site.
We changed the light pink to purple and updated the plot and descriptions. We hope this allows the reviewer to more clearly see the dense layer compared to the percolation paths and the SSL.

L252: "Given that surface conditions were generally similar across the floe" --> As for the vertical ordering of the snow layers possibly yes but not when it comes to the snow depth. But again, fair enough.
Yes there are slight fluctuations in snow depth as expected from snow drifts, but it is the conditions that is more important here.

L349-351: Another source of typical TBS observed over sea ice could be sea-ice concentration retrieval papers. Even though the values reported therein might contain a small residual weather influence these are another helpful source of information against which your measurements can be backed up. One such paper would be the one by Ivanova et al., in The Cryosphere, 9(5), 2015, where the Appendix A lists typical values for freezing season conditions. There is also a paper by Ivanova et a., 2014 in Transactions on Geoscience and Remote Sensing on sea ice concentration algorithms.
Thanks for pointing out the values in the Appendix from Ivanova et al. (2015). It is encouraging that our values do match theirs quite well. We have added that reference to satisfy the reviewer.

L444: Not sure why Kaleschke et al. (2016) is listed here as this is about SMOS based thin ice thickness retrieval, hence L-Band, a frequency you did not include. Since the "main playground" for the SMOS thin ice thickness retrieval is FYI with thickness values below about 70 cm with potentially little snow easily penetrated by the long L-Band waves the relevance of your study on SMOS/SMAP based thin ice thickness retrieval can be questioned.
We listed the L-band in the implications section as a discussion point. It is true we do not evaluate the L-band instrument that was also deployed during MOSAiC, as wet snow may also impact this frequency. With an L-band wavelength of 21 cm, a refrozen ice layer should have a depth of similar order to show a scattering effect. However, the snow densification changes the dielectric contrast at the snow/ice interface and this could still have an impact. Again, this is about potential implications and something that the PIs for the L-band instruments could explore further.

L465/466: "(GR37/19 shown in Figure 10(c))" --> I see that you did not consider my suggestion to also show the PD19 or (as it is very simple to compute) the PR19 based on the values shown in Figure 5. As you may know, the tie point triangle in PR19 - GR3719 space of the NT algorithm highlights very well that the main contribution to the sea ice concentration derived using the NT algorithm comes from the PR19 which is - as you state in your comments to my 1st review - only a normalized version of the PD19. I am sure, that plotting the PD19 alongside the NT sea ice concentration would be an eye-opener. But, fair enough, you did not see it that way.
We show in Figure 10 the PD at 89 GHz and the GR for 19/7 and 37/19. While we could show the PD at 19 GHz, it can already be inferred from Figure 9 and Figure 5 and we do mention some differences in values in the text. We agree that it would also highlight the NT algorithm dependence, but we already have so many figures we tried to keep things succinct. We do plan in a follow-on study to evaluate discrete events detected in reanalysis to get more insights into satellite retrievals. We would be happy to collaborate with the reviewer on such an effort.

L472: "over both FYI and MYI" --> I have to admit that I am not overly happy with generally stating that the Rostosky et al. algorithm is THE solution for snow depth on MYI. It has not been evaluated adequately for most of the freezing season (October through March) and it has been developed / trained with data of the same month (April) that is also then used for the evaluation. It is a first step into the correct direction but it is not the solution. Anyways, the interesting piece of information here is that Figure 10 shows that the snow thickness based on GR197 is as large after the ROS as it has been before it - which is an improvement compared to using GR3719.
We did not intend to imply the Rostosky et al. algorithm is the solution for snow depth on MYI…we are simply using it as an example, we are not advocating any particular algorithm over another. The point is simply to show a ROS event can impact these retrieval algorithms.

L477: What I again still do not support is that you do not comment at all to the fact that the snow thickness values retrieved with both approaches are considerably higher than those so representative MOSAiC floe snow thickness values. To me this clearly suggestions that both snow thickness products are biased high because of the considerable fraction of non-seasonal ice in the region the MOSAiC floe was located. I am missing a statement clarifying this in your manuscript.
Again, we were not validating any of the snow depth algorithms, but we've added some statements regarding your two concerns about the snow products, in particular we have added: "*Note that the retrieved snow depth is substantially higher than the snow depth measured on the MOSAiC flow. Both algorithms are not designed to retrieve snow depth during the refreezing season. Sea ice that survived one summer melt is interpreted as deep snow by the algorithms and thus snow depth is overestimated.*"

L481/482: "they would ... and refreezing" --> I suggest that you add the information that you were rightly providing also in your comments to my 1st review. Therein you stated that during winter with presumably thicker snow loads and colder snow surface and ice/snow interface temperatures the effect of these ROS / warm air intrusions might affect a smaller vertical portion of the snow cover. Percolation into the snow might not be as deep as during your example shown here on the one hand. But on the other hand, ice layers forming at the top of a deeper snow layer might bias altimeter measurements even more in winter than during your case. I just invite you to consider providing this information to readership and not just us reviewers (and those who eventually have the time to look through discussions of submitted manuscripts).
Ok, we added your above suggestion near lines 481. Specifically we write: "*A key difference with presumably thicker snowpacks and colder snow and snow/ice*

interfaces in winter could be that ROS or warm air intrusions may affect a smaller vertical portion of the snow cover. Specifically, percolation into the snow might not be as deep, which once refrozen, would lead to larger biases in ice thickness retrieved from radar altimeter than during the case study shown in this paper."

Typos / editoral comments:
L131/132: "Physical temperatures were made ..." --> Please check sentence; seems as if a word is missing.
We don't quite understand what the reviewer means, the sentence already reads as "*Physical temperatures were made of the absorber pads and the ambient air temperature*"

L177: "during summer" --> you could add "and (early) fall"
Done

L227: "the the" --> "the"
Done

L229: "additiona" --> "additional"
Done

L235/236: the 2nd "including the SSL" can be deleted.
Done

L237 and elsewhere: I am wondering whether you would consider to change "flux" to "FLUX" when speaking of the respective site.
Good suggestion, done.

Figure 5 caption. I suggest to add: "Note the difference of one order in magnitude in the kernel density distributions between pre and post ROS event."
Good suggestion, Done

L360: "GHz" --> "K", same in L362.
Thanks for catching that. Corrected!

L470: "increase in the MYI fraction" --> add: "and in the snow depth retrieved using the Markus and Cavalieri algorithm."
Done

Figure 10: Please use (a) to (d) to denote the panels in the figure itself and also in the caption to be consistent to your text.

Done

L634: "microcontroller.As" --> "microcontroller. As"
DOne