Reviewer Comment on essd-2021-286
Anonymous Referee #1

Referee comment on "Snow depth product over Antarctic sea ice from 2002 to 2020 using multisource passive microwave radiometers" by Xiaoyi Shen et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-286-RC1, 2021

General comments

The manuscript describes a new daily snow depth product on Antarctic sea ice derived from satellite passive microwave radiometry. The proposed methodology builds on previous algorithms, which were mainly applied to Arctic sea ice, and the authors first evaluate the most useful frequency combination for Antarctic sea ice and then derive the corresponding parameters. The method itself (using a gradient ratio of passive microwave measurements and fitting the linear coefficients) is rather trivial and has been employed previously (Markus and Cavalieri, Rostosky et al.), but previous products were not very accurate over Antarctic sea ice, which makes this manuscript and the corresponding dataset a novel and unique contribution. The authors conduct fairly extensive and comprehensive comparisons to a range of in-situ data as well as the Comiso method and ICESat-2 laser altimetry data to validate their method. The manuscript also analyses and visualises seasonally averaged mean snow cover and trends in the data.

This new dataset is a very welcome contribution to science on Antarctic sea ice and beyond, as currently no operational snow depth product is available for the Southern hemisphere. Therefore, I expect this dataset to be useful alone and in combination with e.g. satellite altimetry to derive sea ice thickness. The dataset covers the full lifetime of the AMSR-E and AMSR-2 sensors of 2002-2020 and employs SSMIS data to bridge the gap in between the two, making it a complete dataset. Updating the dataset regularly in the future would be very useful though.

The manuscript is well structured, clear and it has a good length. References and citations are mostly complete. A recent related study by Kacimi and Kwok (2020, https://doi.org/10.5194/tc-14-4453-2020) could be added, though. Figures and tables provide detailed insights into the dataset and the comparisons made for the accuracy assessment. Overall, they are presented in a comprehensive manner. A few details are too small and hard to see though. A time series of (e.g. average and per section) snow depth over the full 18 years would round the paper up and add to its credibility.

The method description is kept rather short, but clear enough, as references to previous studies are given to point the reader to more details. However, it might be worth writing down how the gradient ratio is calculated either in the appendix, on the website or in the readme file. What strikes me is that the derived uncertainties (which are provided with the
dataset) are much smaller than the RMSDs calculated with respect to all other datasets. To me, the calculated RMSDs seem more realistic and I would therefore suggest rethinking and adjusting the uncertainty estimation, as the uncertainties are the values that are provided with the data.

In terms of the discussion, my view is that the derived trend and the comparison to the ASPeCt data might not be significant. On the other hand, I am missing a comment and discussion on the bias between ICESat-2 and the proposed method. Please see more details in the specific comments.

The data is accessible via the given identifier and can be downloaded with an ftp client. Uncertainty estimates are included for each file. The data can be visualised and used for further analysis easily and the authors point to a range of open-source and standard software. The data description says ‘all values are in meter’, while snow depth is actually provided in ‘cm’ and so are the uncertainties. This might cause confusion, even if it should be clear from the data range.

**Specific comments**

Introduction: The study by Kacimi and Kwok (2020, https://doi.org/10.5194/tc-14-4453-2020) should be added, as it is quite related (same as general comment).

L. 42: You state that laser altimeters and a combination of laser and radar altimetry can be used to derive snow depth. However, also radar altimetry can be used stand-alone (without laser altimetry) to derive snow depth: See e.g. Guerreiro et al. (2016) 10.1016/j.rse.2016.07.013 and Lawrence et al. (2018) https://doi.org/10.5194/tc-12-3551-2018

L. 57-58: Please rephrase the last part of the sentence. One could misunderstand it as all of the snow cover in Antarctica is thicker than 50 cm.

L. 90: Delete ‘than AMSR-E and AMSR2’, otherwise the sentence makes no sense to me.

L. 93-94: How did you calibrate SSMIS? Please add more details.

L. 95-96: Were (a) only SSMIS data used to calculate sea ice concentration in this time period, or (b) did you calculate sea ice concentration for the full time (2002-2020) yourself? Please clarify in the text.

If (a) is the case: Why did you choose the ARTIST algorithm rather than sticking to NASA Team, which is used for AMSR-E and AMSR2?

If (b) is the case: You don’t need the downloaded sea ice concentration products mentioned before?! So, please modify the other text.

L. 109: ‘subtracted’ instead of ‘misused’

L. 126: Please rephrase ‘located in eastern and western Antarctica’.

L. 140: Why were these months excluded? Please clarify.

Fig. 1: In panel b the AADC 2007 data and 2005 ASPeCt data have almost the same colour (at least in my print out). Consider changing one of them.
Caption of Fig.1: I would mention AADC before ASPeCt to be consistent with the text and legend.

Eq. 1: Could be worth writing down how the gradient ratio is calculated either in the appendix or on the website / readme where the data can be found (same as general comment).

Also: Please specify (and cite) what you take as the brightness temperature over open water for the different frequencies.

L. 196-198 The calculated uncertainties are much smaller than the RMSDs calculated with respect to other datasets (same as general comment).

L. 282-284: It would be nice to indicate these sectors on the maps in Figure 1.

L. 283: Suggest renaming the ‘Pacific Sector’ or shifting the borders. 90 degrees is still in the middle of the Indian Ocean.

Table 5: Overall, the correlation coefficients seem to be scattered around zero and even in areas with comparably many grid cells negative coefficients occur. This makes me doubt a physical correlation between the two datasets and question to which extend this comparison actually adds to the credibility of the proposed method. I therefore suggest either clarifying and discussing what could cause negative correlations or consider leaving this comparison out.

Fig. 6+7: The numbers in panel c are extremely hard to read. Please increase the font size.

In the caption of Fig. 6 and 7 panels a and b should be mentioned explicitly.

Figures 6+7c: The PMW approach seems to estimate higher snow depths for all months. Do you have an explanation for this bias? As this is quite striking, it should be mentioned in the text and ideally discussed what might cause it.

L. 337: What is the uncertainty of this trend? Upscaling 0.13cm/year is still only 2.3 cm over the full 18 years that you looked at, and smaller than the uncertainties that you found, so I wonder if it is actually a significant trend.

Table 7: Instead of this table, I would find it really useful and interesting to see a time series of how snow depth develops over the full 18 years. You could for example plot the Antarctic wide mean and means from the sectors. Then you could also plot the calculated trend line on top. I think this would greatly increase the credibility of your dataset and the calculated trend, but could also verify the consistency of the three different satellites involved.

Fig. 10: The black dots are almost impossible to see.

Also Fig. 10: I would suggest flipping the colour map to make it consistent with previous figures and common practices where blue corresponds to low numbers (decrease) and red to high numbers (increase), but this might be a personal preference.

L. 367: ‘performed ONLY slightly better’

L. 467: Please check and update the link. “https://nsidc.org/data/G10011/versions/2” leads me to “On-Ice Arctic Sea Ice Thickness Measurements by Auger, Core, and Electromagnetic Induction, from the Late 1800s Onward, Version 2”, which only contains
data from the Arctic.

Data: The data description says ‘all values are in meter’, while snow depth is actually provided in ‘cm’ and so are the uncertainties. This might cause confusion, even if it should be clear from the data range (same as general comment).

**Technical corrections**

L. 195: ‘-‘ missing between ‘July-September’

L. 311: ‘time series’ instead of ‘times’?!

L. 430: ... showed THAT no obvious ...