We thank the reviewer for the helpful feedback, these suggestions have significantly improved the manuscript and figures, we are appreciative of his or her help and time.

We have addressed all the comments here, point by point responses to the comments are listed in BLUE.

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.

Thank you very much for your feedback and advice, these suggestions have significantly improved the text and figures. We have revised the manuscript according to all your comments, details are listed in below.

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).

This reference has been added in the revised manuscript.

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

Thank you for this important comment. These references (radar altimetry can be used to derive snow depth stand-alone) have been added in the text.

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.

Agree. This sentence has been revised to avoid the existing misleading information:
'*... this method is limited to dry snow less than 50 cm thick and thus may underestimate the snow depth in some regions of the Antarctic.*'

The original sentence is:
'~~*... , this method is limited to dry snow less than 50 cm thick, which is clearly less than the snow cover over Antarctic sea ice (Kwok et al., 2014).*~~'.

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

Accept and Done.

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

The related reference has been added in the revised manuscript:
'*The SSMIS brightness temperature observations were calibrated to AMSR-E data based on the method from Wentz (2013), and …*'.

Reference:

Wentz, F. J.: SSM/I Version-7 Calibration Report, Remote Sensing Systems, Santa Rosa, California, USA, RSS Technical Report 011012, 46 pp., 2013.

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.

These brightness temperatures from AMSR-E, AMSR-2 and SSMIS were used to obtain the full time (2002-2020) sea ice concentrations by using the ARTIST Sea Ice (ASI) algorithm (i.e., (b)).

The related text has been revised as the sea ice concentration products do not need to be downloaded:
'*… Here, the AMSR-E/Aqua Daily L3 25 km Brightness Temperature ~~and Sea Ice Concentration~~ Polar Grids (Version 3) product … were all applied. … the NSIDC AMSR-E/AMSR-2 Unified L3 Daily 25 km Brightness Temperature ~~and Sea Ice Concentration~~ Polar Grids (Version 1) product was used. …*'.

L. 109: 'subtracted' instead of 'misused'

Done.

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

The sentence has been revised:
'*… which were mainly **located in eastern and western of the Antarctic sea ice region** (Fig. 1b).*'

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

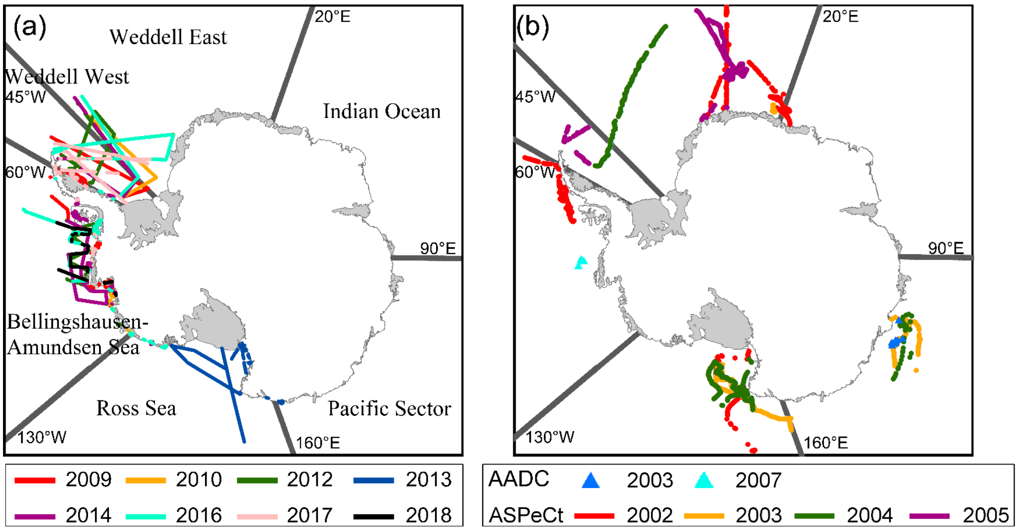
These months are excluded as no data are provided. This reason and the operation periods of used ASPeCt data have also been added in the revised text (Section 4.3 and Table 6):
'*… The operation periods of used ASPeCt data are listed in Table 6, no data can be obtained in the missing periods. …*'.

Table 6. The operation periods of used ASPeCt data in this study.

Year	Month
2002	August, September, December
2003	January, March, April, September, October
2004	March, April, October, November
2005	January, March, August, September

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.

The colour for AADC 2007 data has been changed in the Fig.1b. The updated figure is shown below:



Caption of Fig.1: I would mention AADC before ASPeCt to be consistent with the text and legend.

Accept and Done.

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.

The calculation equation for gradient ratio has been added into the revised text (in Section 3.1).

A reference has been added here to provide the brightness temperature over open water for the different frequencies:
'*... the brightness temperatures over open water for the different frequencies can be referred to Ivanova et al. (2015).*'.

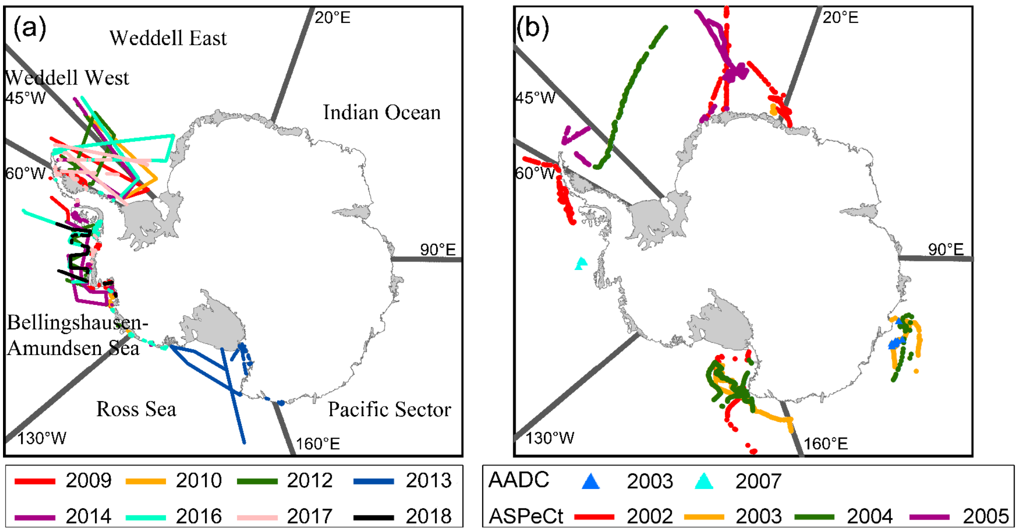
L. 196-198 The calculated uncertainties are much smaller than the RMSDs calculated with respect to other datasets (same as general comment).

To give a more reliable uncertainty estimation, we have adjusted the uncertainty estimation method by adding the contribution of the limited sample size of the OIB data, this was constructed by performing a sensitivity analysis to quantify the interannual variability (caused from the limited sample size) of the regression coefficients from Eq. 4. The snow depth uncertainty in summer (an average of 32.50 cm) was larger than that in the other seasons due to the effect of liquid water in the snow layer. In autumn, winter and spring, the average snow depth uncertainties were approximately 20.76 cm, 17.85 cm and 23.79 cm, respectively. These values are closer to RMSDs calculated with respect to other datasets.

More details can be found in Section 3.3 in the revised text.

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

Agree and Done. The updated figure is shown below:



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

'Pacific Ocean' has been revised to 'Pacific Sector' in the revised text and figure.

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.

A discussion has been added here to clarify the negative correlations:
'*Some negative correlation coefficients in Tables 7 and 8 (original Tables 5 and 6) can be found even in areas with comparably many grid cells, this is due to the observation bias of the ASPeCt*

data (± 20% bias is found for undeformed ice thicker than 0.3 m, and ± 30% bias for deformed ice, Worby et al. (2008b)). Due to the limited accuracy of ASPeCt samples, the evaluation may be biased, but ASPeCt shipborne data can still be assumed as a proxy for performance evaluation due to its large spatial-temporal coverage.'.

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

The font size of numbers in panel c of Figs. 6 and 7 has been increased.

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

In the captions of Figs. 6 and 7, panels a and b have been mentioned explicitly in the revised text (take Fig. 6 as an example):
'*Figure 6. The spatial distributions of monthly snow depth estimates (January 2019 to June 2019) from this study (a) and ICESat-2 (b) together with the number (N) of valid grid cells in the bottom left corner of each image. (c) The probability density functions (PDFs) from (a) (red) and (b) (green). Numerical values in top right corner of (c) show the mean and standard deviation of the monthly snow depth estimates from (a) (red) and (b) (green).*'.

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.

We have added a discussion to explain this overestimation when comparing with ICESat-2 as shown in Figures 6 and 7:

'*An obvious snow depth overestimation for the proposed method can be found comparing to these from ICESat-2 in all months of 2019. Regional empirical linear regression models were used to compute snow depth from ICESat-2 total freeboard measurements. The empirical linear regression models were constructed based on the local sea ice measurements from 15 cruises in the Southern Ocean over a time period of about 22 years (1986–2007). The limited coverage of this data set and the variable nature of snow cover over sea ice reduce the representativeness of this data set, which may contribute to the underestimation of snow depth estimates from ICESat-2, more local sea ice observation data (including snow depth, sea ice freeboard and sea ice thickness) are needed to improve the snow depth estimates from ICESat-2 in recent years (e.g., 2019).*

Snow depth retrieval based on passive microwave radiometers is sensitive to grain size (Markus and Cavalieri, 1998) and ice type. For example, at microwave frequencies multiyear ice has a similar influence on the brightness temperatures as snow cover (Rostosky et al., 2018), and thus the snow depth over multiyear ice is overestimated in this case; in late winter/spring the grain size growth leads to a stronger reflected radiation and a reduction of the brightness temperature (Markus and Cavalieri, 1998). Both of these can influence the GRs and then increase the snow depth estimates. While satellite laser altimeters are independent of the snow properties and thus suffer less from the variable snow properties than passive microwave radiometers. Thus, the difference *between snow depth estimates from the passive microwave radiometers and ICESat-2 is due to their sensor and methodology difference, more observations of snow cover (including thickness, ice freeboard and snow properties) are needed to quantitatively explain the difference between these two snow depth estimates.*'

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.

The uncertainty of this trend is 0.05 cm. A decreasing of 2.3 cm over the full 18 years was found and this value is smaller than the estimated snow depth uncertainty, hence the estimated trend may not be a significant trend. According to your comment below, we delete this table (i.e., Table 7) and show the time series of snow depth develops over the 18 years in the Antarctic and six sea sectors, which should be more useful.

Table 7: Instead or in addition to 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.

Time series of snow depth estimates over full 18 years in the Antarctic and six sea sectors has been added here and the original Table 7 has been removed. The same conclusion can be derived from this figure, i.e., Antarctic snow depth showed a decreasing trend from 2002 to 2020; all six sea sectors showed decreasing trends, and these trends were decreasing across all four seasons. The above statement has also been included in the revised manuscript. This figure is also listed below:

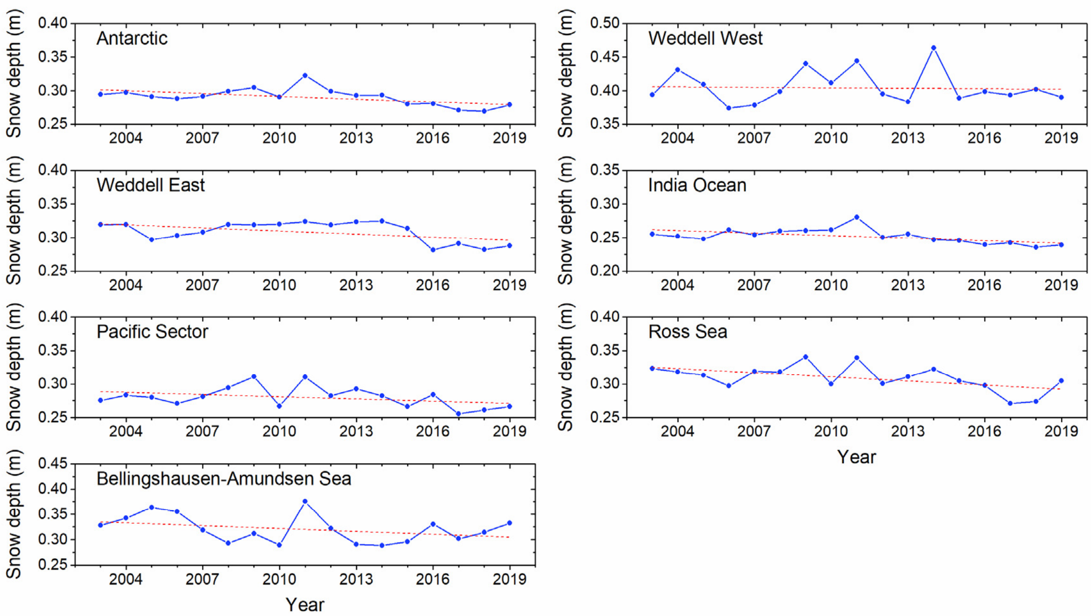


Figure 10. Time series of the annual snow depth estimates derived from the proposed method from

2003 to 2019 for the Antarctic and six sea sectors at the spatial resolution of 25 km.

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.

Accept, the colour map has been flipped in the updated figure, and the size of black dots has been increased:

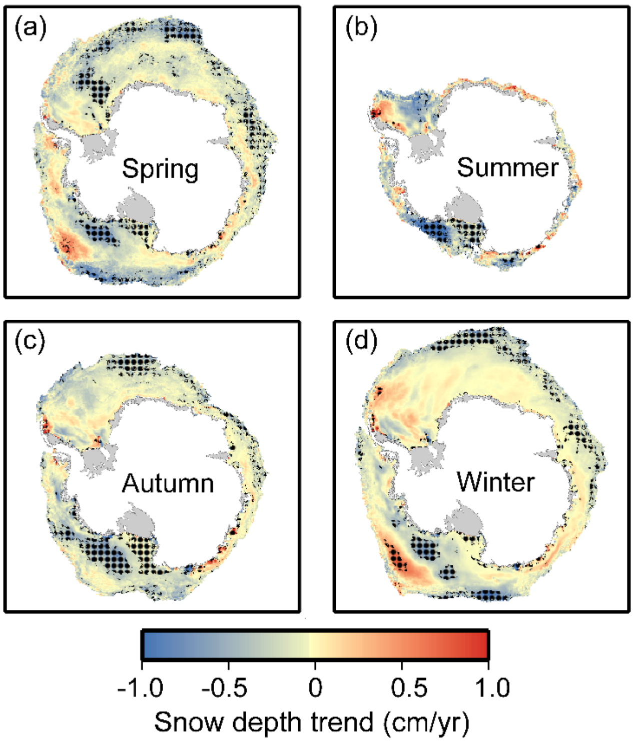


Figure 11. The spatial distributions of snow depth trends in different seasons from 2002 to 2020. For each grid cell, trend was only estimated when 12 years (or more) of snow depth estimates were obtained. Only grid cells with sea ice concentration ⩾75% are shown here, grid resolution is 25 km. The black dot means that the trend is significant at 95% significance level according to two-tailed Student's t-tests.

L. 367: 'performed ONLY slightly better'

Done.

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.

https://data.aad.gov.au/metadata/records/sea_ice_measurements_database

The link for AADC data has been corrected in the revised manuscript:
'*AADC in situ data can be derived at https://data.aad.gov.au/metadata/records/sea_ice_measurements_database*'.

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).

We will revise the data description to give the right statement.

Technical corrections

L. 195: '-' missing between 'July-September'

Done.

L. 311: 'time series' instead of 'times'?!

Done.

L. 430: … showed THAT no obvious …

Done.