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Interactive comment on "The regional scale surface mass balance of Pine Island Glacier, West Antarctica over the period 2005–2014, derived from airborne radar soundings and neutron probe measurements" *by* Stefan Kowalewski et al.

Anonymous Referee #2

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This paper presents recent surface mass balance (SMB) estimates derived from airborne radar observations, and ground based neutron probe measurements of snow density along the iSTAR traverse (2013,2014) at Pine Island Glacier (PIG). This paper interestingly focuses on data uncertainties resulting from methodology and assumptions made on the interpolation error, demonstration and compares estimates with those given in previous publication from Medley et al., 2014, and with RACMO2.3p2 simulations. This paper is well written, presents an interesting new dataset for model validation and deserves to be published.

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Discussion paper

However, before publication, I have a few questions and suggestions. I hope these could improve the final paper.

Major comments:

1. My main comment concerns the dating of reflection layers. Here, the dating relies only on big assumptions made on stratigraphy characteristics. This technique is based on differences in winter and summer snow due to changes in atmospheric conditions and radiative fluxes. I suppose that dating of reflection layers and NP data is accurate but there is no comparison with stake networks, or with a clear "absolute" dating based on anthropogenic radionuclides or volcanic horizons at several cores. Since layers sometimes mismatch at several intersections, or are excluded (around iSTAR sites 2 and 19 for instance), the final dating may be not fully robust. I understand that the authors define a layer age uncertainty of +-1.4 year and assess the associated uncertainty in the surface accumulation, but is it possible that the layer dating uncertainty exceeds 1 year? In particular, melt or rain is expected to occur mainly in summer, but is it possible that significant surface melt (or rain) occurrences occurred at the beginning and end of summer but were separated by an "extreme" solid precipitation event (Turner et al., 2019), leading to a 2 maxima in density and in other snow characteristics used for the layer counting? Is there any snow erosion, which could remove the surface layer at locations in the area? Are there any stake farms or well constrained ice cores (with an absolute age of one or various layers) in the area, on which the authors could validate their estimates in snow accumulation?

2. Neutron probe is a really interesting way to retrieve snow density and the authors clearly took profit from this technique in the past and in the present paper. However, data rely on a few calibration steps. I had a look to previous papers from Konrad et al., 2019 and Morris et al., 2017, and I did not really understand how density data were validated before being used in the present paper. My concern is because in Figure 2 we observe that snow density is 550 kg m-3 at 7m below the ground level, whereas it is 600 kg m-3 at the same depth in Morris et al., 2017. Snow density in firn cores

from Konrad et al., 2019 are hard compare here because their Figure 3 is developed until 50 m. Could the author describe whether they calibrated the NP snow density data with snow pit data in the present paper or not? If not, is there a difference in the density/depth relationships between (Konrad et al., 2019), (Morris et al., 2017) and present paper. If snow density from NP measurements is biased, how will this impact the final SMB values?

3. Concerning the kriging method, is it worth using northing, easting, and elevation as explanatory variables? Would it be more relevant to use the distance from the coastline as explanatory variable? Or even the distance from Amundsen sea coast?

4. Comparison of ASIRAS data with RACMO2.3p2 simulations are really interesting but differences are not fully justified/explained in the text. Since differences are model dependant, it would be interesting to see potential differences with another model used in Antarctica (e.g., COSMO-CLM, HIRHAM5, MAR3.10, MetUM , see Mottram et al., 2020). Since Agosta et al. (2019) proposed potential justifications of the differences existing between RACMO2.3p2 and at least the MAR model, I believe that a comparison with the MAR model would make sense here. Indeed, in Agosta et al., 2019, large differences between RACMO and MAR are observed in regions where the RACMO2.3p2 model presents the largest differences with the ASIRAS data. A quick comparison could be relevant to discriminate whether the precipitation formation, advection of hydrometeors, and sublimation of precipitating hydrometeors (Agosta et al., 2019) are important or not in the PIG area. Data from Agosta et al. 2019 are available here: https://zenodo.org/record/2548848#.X0St8TXgphE If the authors are interested in higher resolution simulations, data from a more recent paper from Donat-Magnin et al., 2020, focusing in the Amundsen region are also available at: https://doi.org/10.5281/zenodo.2815907.

5. The paper largely describes differences with Medley et al. (2014) paper, but the authors never include any figure presenting the differences. I propose to include a map presenting ME14 route, SMB results and differences with the ASIRAS data.

6. The paper is sometimes quite hard to follow for a non-expert of this area. Different datasets are used here, and the difference between ISTAR/ASIRAS and ME14 is not always clear. The authors use a many acronyms. I suggest that the authors include a table where they clearly describe the difference between ASIRAS/iSTAR data used here, and more particularly the difference with the ME14. For instance, Table 1 presents different radars (ASIRAS, pulseEKKO PRO GPR , CReSIS Accu-R) part of the information is given in the caption, but perhaps the authors could also precise in the table if field campaigns were deployed on the ground or by plane, over what distance? which were the reflection layers used for SMB estimates? which density measurements were considered (NP? Firn cores?)? how was performed the dating of the reflection layers?

7. Figures could display the route followed by ME14, and where GPR from Konrad et al., 2019 was carried out.

Minor comments: Abstract

“Thus there is no evidence of a secular trend in mass input to the PIG basin.” => please be more accurate because secular may be misinterpreted here. I suggest to replace secular (here and elsewhere) by decadal.

Line 28: "in particularly"

Lines 38-41: firn cores are only used to retrieve the depth of dated snow layer? Are they used to calibrate the neutron probe density data?

Figure 1. ASIRAS-iSTAR survey : please also include the location of ground GPR observations from Konrad et al., 2019?

Line 68, the authors write: ” Due to the reported consistency between the GPR and airborne SMB measurements in Konrad et al. (2019), we limit the comparison of our results to the basin wide estimates by ME14”. I feel that a figure (perhaps in the supplementary material) showing the different radargrams could help the reader. A quick

data comparison, before interpolation, could also be done to see how snow density and radar data uncertainty impact the final SMB value.

Line 85 “Morris et al. (2017) applied an automatic annual layer identification routine to their snow density profiles and used the annual H2O2 peak depths as an additional guidance for the annual layer dating.” => is it possible to observe the removal of on year of snow in the PIG area (due to erosion) or multiple summer maxima?

Line 88: Here I understand that the authors did not consider the density obtained from the firn cores to compute the final SMB. What is the difference in the final SMB if the authors use the density from firn cores to calibrate the snow density profiles?

Line 89: 43 profiles => I suppose this means that profiles were done twice at the 22 sites?

Line 92: is there any relationship between snow density profile and Accumulation/Temperature as suggested by Herron & Langway 1980 equation?

Line 120: how does hoar produce thin ice layers? Is it possible to have short rain our melt events in spring or late summer? Could wind erosion or sublimation create wind crusts in this region?

Line 144: “We assign an estimated date to the mean value of N points” => do you mean “the mean value of depth”?

Line 198: "may by due"

Line 275: please discuss this sentence according to Agosta et al. (2019) results. Indeed, according to this publication, sublimation of precipitating hydrometeors are missed at low elevation in RACMO2.3p2. This could justify that RACMO 2.3p2 presents a positive bias at low elevation. Conversely, they suggest that MAR snowfall rates generally exceed those simulated by RACMO2.3p2, by more than 30% on the lee side of the West AIS (Marie Byrd Land toward Ross ice shelf), and close to crests at the ice sheet margins. Here, a comparison with MAR could be interesting.

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Lines 288: The authors refer to the ASIRAS or the Hydrid SMB estimates, but line 296 they refer to the ASIRAS vs. the ME14 one, whereas they refer to the Hybrid estimate at line 298. The difference between these estimates is not clear. Why do the authors use ASIRAS / hydrid estimates in different parts of the text?

Line 325: "but also measured density and strainrate profiles suggest a mean annual SMB of 200 kgm−2yr−1 based on theoretical grounds, which both are in a better agreement with the collocated ASIRAS based results." => Is it possible to explain briefly the way this value of 200kg m-2 is computed?

Line 328- 331: please give elevation of sites 2, 18, 19. It would be interesting to discuss the differences between the RACMO2.3p2 and MAR models here (is there any potential explanation related to precipitation formation /advection /sublimation of precipitating particles to the ground described in both models).

Line 352: "secular" => decadal?

Line 395: "Inspection of the artificial cluster highlighted in Fig. 5 revealed" => replace by fig. 8?

Table 2: please include site coordinates and elevation.

Table 3: Are the basins from Mouginot et al. (2017) similar to those given by Rignot et al. (2019)? If not, perhaps it would make sense to use the most recent basins.

Table 5: do values refer to results written as OLK(NN) in Figure 7?

Figure 5: please include ISTAR site numbers on a) and b) to help the reader in retrieving the locations cited in the text.

Figure 7: Please define NN

Figure 8: I suggest to include the elevation contours. Is it correct to write Eastings when the x-axis is related to the north-south direction, and Northings for the y-axis when it is on the east/west direction? I don't understand why the kriging procedure

induce strange vertical (or horizontal) lines of similar values in Figure 8a (this point is particularly visible near the glacier outlet)? Is it because northing, easting, and elevation are used as explanatory variables? If yes, is it not more relevant to use the distance from the coastline as explanatory variable? Or even the distance from Amundsen sea coast?

References:

Agosta, C., Amory, C., Kittel, C., Orsi, A., Favier, V., Gallée, H., van den Broeke, M. R., Lenaerts, J. T. M., van Wessem, J. M., van de Berg, W. J., and Fettweis, X.: Estimation of the Antarctic surface mass balance using the regional climate model MAR (1979–2015) and identification of dominant processes, The Cryosphere, 13, 281–296, https://doi.org/10.5194/tc-13-281-2019, 2019.

Donat-Magnin, M., Jourdain, N. C., Gallée, H., Amory, C., Kittel, C., Fettweis, X., Wille, J. D., Favier, V., Drira, A., and Agosta, C.: Interannual variability of summer surface mass balance and surface melting in the Amundsen sector, West Antarctica, The Cryosphere, 14, 229–249, https://doi.org/10.5194/tc-14-229-2020, 2020.

Herron, M. M., and C. C. Langway, 1980: Firn Densification: An Empirical Model. J. Glaciol., 25, 373–385, doi:10.3198/1980JoG25-93-373-385.

Konrad, H., Hogg, A. E., Mulvaney, R., Arthern, R., Tuckwell, R. J., Medley, B., and Shepherd, A.: Observations of surface mass balance on Pine Island Glacier, West Antarctica, and the effect of strain history in fast-flowing sections, Journal of Glaciology, pp. 1–10, https://doi.org/10.1017/jog.2019.36, 2019.

Medley, B., Joughin, I., Smith, B. E., Das, S. B., Steig, E. J., Conway, H., Gogineni, S., Lewis, C., Criscitiello, A. S., McConnell, J. R., van den Broeke, M. R., Lenaerts, J. T. M., Bromwich, D. H., Nicolas, J. P., and Leuschen, C.: Constraining the recent mass balance of Pine Island and Thwaites glaciers, West Antarctica, with airborne observations of snow accumulation, The Cryosphere, 8, 1375–1392, https://doi.org/10.5194/tc-8-

1375-2014, 2014.

Morris, E. M., Mulvaney, R., Arthern, R. J., Davies, D., Gurney, R. J., Lambert, P., De Rydt, J., Smith, A. M., Tuckwell, R. J., and Winstrup, M.: Snow Densification and Recent Accumulation Along the iSTAR Traverse, Pine Island Glacier, Antarctica, Journal of Geophysical Research: Earth Surface, 122, 2284–2301, https://doi.org/10.1002/2017JF004357, 2017.

Mottram, R., Hansen, N., Kittel, C., van Wessem, M., Agosta, C., Amory, C., Boberg, F., van de Berg, W. J., Fettweis, X., Gossart, A., van Lipzig, N. P. M., van Meijgaard, E., Orr, A., Phillips, T., Webster, S., Simonsen, S. B., and Souverijns, N.: What is the Surface Mass Balance of Antarctica? An Intercomparison of Regional Climate Model Estimates, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-333, in review, 2020.

Turner, J., Phillips, T., Thamban, M.,Rahaman, W., Marshall, G. J.,Wille, J. D., et al. (2019). The dominantrole of extreme precipitation events inAntarctic snowfall variability.Geophysical Research Letters,46,3502–3511. https://doi.org/10.1029/2018GL081517

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