Response to reviewers#2

Interannual Variability of Summer Surface Mass Balance and Surface Melting in the Amundsen Sector, West Antarctica

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[Comments from the editor]

I have an overall impression that the manuscript needs careful proof reading. For example, SMB and DJF need to be used more consistently, rather than both SMB and surface mass balance, and both DJF and December-January-February are used in an interchangeable manner. Also, some figure captions are incomplete. All line numbers below show these in the markup manuscript.
We thank the editor for taking the time to evaluate our revised manuscript, and we apologize for these remaining mistakes.

L139: Define DJF here as it appears first time here. It is currently defined at L248.
Done (L.131 of revised manuscript).

L203: MAR has 10 km horizontal resolution (L169) so all AWS in your model domain can be located less than 15 km from the MAR grid points. So, I think that first criterion is always met. Please clarify. And show how many AWS are located in the model domain (out of 243 over Antarctica).
We have added more explanation about this: "even if the domain resolution is 10 km, stations over islands or capes that are not resolved can be located farther than 15 km from the closest continental MAR grid point" (L.181-182). We have also added the numbers of stations used (41 out of the 243 available over Antarctica).

L213-214: do you want to say that reflectors detected with airborne radar are dated using firn cores?
We want to say that SMB derived from airborne radar was validated against firn cores. This has been clarified L. 192-193.

L215: Show time span of SMB values in the GLACICLIM SAMBA dataset.
We completed the sentence L.194-201.

L251-254: revise to "(as also found by Scott al., 2019 and Holland et al. 2019). Our analysis found very similar results using NINO3.4 (not shown)." Currently it is unclear who obtained the results with NINO3.4. And "3.4" or "34"?
This has been corrected following the editor's suggestion.

L328: define RMSE here (its definition in the figure caption is inadequate).
We now define RMSE L.284. The 10th and the 90th percentile are related to all RMSE. (i.e. the 10th percentile and 90th of the RMSE series for all the 41 stations)

L334-335: Why are less satisfactory results in the inland explained by more flatter, smoother topography there?
There seems to be a misunderstanding: we are speaking about island and resolution : 10 km is still too coarse to resolve topographic features of the smallest islands.

L337: change to DJF (also in Fig. 2 caption and elsewhere; once it is defined use it consistently).
Done.

L931: make a new paragraph here?
Done.

L992: Medley and Thomas (2019), not 2009.
Sorry for this mistake, we have changed the year to 2019.

Table 2: where is it cited? Please make sure that all tables are cited in the right order.
We have added a sentence L.325-326.

Figure 1: add distance bar.
Done.

Figure 2, Table S1, Table S2: please clarify that these are daily values (it is said daily at L326 but not explicit enough in the figure and tables).
Done.

Figure 3: update the caption. Drainage basins are not shown in the revised figure.
The drainage basins are shown using large grey contours in the revised version, as described in the caption "The drainage basins under consideration are the same as in Fig.1 (large grey contours here)."

Figs 9, 10, 11: panel a shows IVT along the y axis of this map projection. Then you need to define the map projection used in this paper.
We add the projection (oblique stereographic projection EPSG:3031) L.156

Figure 9: add scale of arrow lengths or at least explain it in the caption. Also explain hatched area more clearly. What kind of significance? And which panels show these hatches (all a-f or only a-d)?
We have added in the caption that scale of arrow lengths are shown near the upper right corners of panels (e) and (f). And we now mention that hatches are shown for panels (a-d).

Figure 10: define q, v, p, and g. Similar to Figure 9, clarify this t test and significance.
Done.

Figure 11: change "same formula as for SMB" to "same formula as for Fig. 10". Similar to
Done.

Supplement:
Table S1: typo. Change S to S1.
Done.

[Comments from reviewer#1]

The paper has undergone significant revision and the authors have made several improvements. The new version is generally well-written, including a thorough description of relevant processes, and the version is clearly the result of a substantial amount of work.

Minor Comments

Line 220: Why not directly calculate the SAM index from the MAR pressure fields (and actually, the same for the ASL?)
To calculate the SAM a global simulation is needed. Our domain is still too small to calculate the ASL (sector pressure is defined as area-average sea level pressure over sector 170-298°E : 60-80°S, Hosking et al., 2016)

Line 318: The details of the comparison with the SAMBA database should be improved. Those measurements cover lots of different time windows, how was that considered? If it wasn't, it should be mentioned that that is a possibility for the decreased performance as compared to the snow radar data.
Details are now given L.194-201.

Table 3 and 4: The multiple regression is an interesting addition. It would be more useful if the regression coefficients as well as their individual p-values for each index are listed, so the reader can understand how they interplay.
Computing p-values for the LASSO is difficult, because the optimization problem of the LASSO introduces a selection procedure on the variables, setting some to zero and some not to zero based on their correlations (see https://arxiv.org/pdf/1311.6238.pdf : "The problem with this is that the p-values can no longer be trusted, since the variables that are selected will tend to be those that are significant")

Line 690: I think care should be taken to also mention the strength of this signal (or lack thereof) as the lagged explained variance still remain very small.
We agree and have added the following sentence: "It should nonetheless be noted that even accounting for this 6-month lag, the influence of ENSO on summer SMB and melt rates remains weak, not explaining more than 15% variance" (L. 709-711).

Technical Comments

Page 1, Line 24: consider revising sentence to: Forty percent of the interannual summer SMB variance over the Getz Ice Shelf is explained by the westward ASL longitudinal migrations
We lose an important result with the reviewer suggestion: the percentage increase westward toward Getz, we therefore suggest to keep the sentence as before.

Line 66: Change "Understand" to "Understanding"
Done.

Line 155: Change to "Amundsen Sea Embayment glaciers"
Done.

Line 163: remove the extra comma and parenthesis after Dee et al.

Done.

Line 346: change “pronounce” to “pronounced”
Done.

Line 348: Please explain what variable it is the MAR overestimates. It’s not clear if its melt or snowfall
Thanks for this comment, it was about surface melt, we have completed L.355.

Line 400: change to Amundsen Sea sector
Done.

[Comments from reviewer#2]

The authors faithfully addressed my concerns and apart from the minor comments below, I think this paper is ready for publication.
L177: The text state ~400 km but the map shows a white zone of about 50 km. Which of the two is true? Or does the domain extents further than Figure 1? In any case, something needs to be adjusted.
Sorry for this mistake, the true value is 50km. We adjusted the text.

Figure 5: The authors provides an argument to stop the color scale at 100 mm w.e. per year. I’m not convinced, the other ice shelves are also discussed although in lesser extend. Moreover, I simply do not see the necessity to cut off at 100 mm w.e. per year as it is doable to expand to ~200 mm w.e. per year without loss of clarity. They seem to use now something similar to ncl’s color table cmocean_tempo (thus white, light green, blue, black), but if you use a colormap/color table with more colors, you can show the values up to 200 without losing “signal” for the 10 mm w.e. per year values.
We agree, we have changed figure 5 as suggested.

Figure 9c/d & 12c/d: These are a good additions, but it should be explained better what is exactly shown in these panels. The logical place for this explanation is in the running text.
We have added some explanations L.405-410 (about Fig. 9) and L. 424-428 (about Fig. 12).

Figure 13: adjust scale of the y-axis that dotted line in e doesn’t get cut off.
Fig. 13 has been modified as suggested.

Line 990: would “… austral summer, which represents 15% of the annual SMB, and correlations…” not be grammatically better?
Thanks for this comment, L.990 (L.629 in the revised manuscript) has been changed.

Line 1029: “We now discuss” is a bit odd, consider rephrasing into something like “Lastly, we discuss”.
Done.

Line 1030 The assumption that you need melt+rainfall to be 2.24 times snowfall before the snowpack saturates conflicts with earlier estimates. Pfeffer, 1991, JGR estimated it to be 0.7, recent observations align with that. I know, that is not saturation with water, but simply filling the pore space with refrozen water. Furthermore, Kuipers Munneke, JoG, doi: 10.3189/2014JoG13J183, made an estimate of when this process may cause ice-shelf collapse. Ice shelf collapse in this region is not as unlikely as the authors suggest now.

We thank the reviewer for pointing to Pfeffer et al. (1991), and there was indeed a mistake in our estimation: our ratio of 2.24 is valid for the case of rainfall bringing extra water into the firn, but in the case of melting, the liquid water is not extra water, it is removed from the snow mass, and the ratio becomes 0.69. Pfeffer et al. assume a maximum saturation of 830 kg/m^3 (due to some kind of close off), which reduces the ratio to 0.64, but they also take into account a melt quantity to warm the firn to 0°C (required to store liquid water), which in the end gives 0.70. To keep it simple, we have modified our text and now refer to an approximate ratio of 0.7 according to Pfeffer et al. (1991). The calculation is now done only over ice-shelve (instead of all drainage basin as before) which is more pertinent for hydrofracturing. Snowfall and surface melt over individual ice shelves has been added to Table.2.
We have added a reference to Kuipers Munneke et al. at the end of the conclusion: “In their projections, Munneke et al. (2014) found that the Western part of Abbot as well as Cosgrove could become water-saturated before the end of the 22nd century, but the other ice shelves of the Amundsen sector remained non-saturated”.

Line 1090: rephrase, see comment at 1030.
Done.