The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-109-RC2, 2019

Interactive comment on "Interannual Variability of Summer Surface Mass Balance and Surface Melting in the Amundsen Sector, West Antarctica" *by* Marion Donat-Magnin et al.

Anonymous Referee #2

Received and published: 17 July 2019

This paper presents results from the regional climate model MAR run for the Amundsen Sector of West Antarctica. The paper is well written and thorough, although some parts the paper needs improvement.

After reading the paper and collecting my points of concerns, I've read the other review and the comment of David Bromwich. I agree with their major concerns and these concerns have to be addressed. Additionally, I have the following major comments:

It needs to be addressed why SMB summer is discussed and not the annual SMB. I can imagine a reason, but this - or any other - reason is not given.

Although the patterns in Figures 8-12 are logic and reasonable, its worrisome that most

of the signals showed are insignificant. Try to get a better understanding of the significance. For example, for geopotential fields the gradient matters more than the value, so you might take a "relative elevation" approach similar to the ASL central pressure. You might also try a different method to determine significance, for example, bootstrapping. If the patterns remain mostly insignificant it implies that the shown patterns do occur during high/low melt/SMB but not necessarily lead to high/low melt/SMB.

I'm not convinced that humidity convergence @ 850 hPa is the best parameter to show. For SMB anomalies: As the moisture holding capacity of air is not that big, the convergence is directly linked to precipitation generation. Added compared to SMB is a whole bunch of noise due to variations in the elevation of the 850 hPa level and noise is added by apparently near stationary numerical waves. I would be more interested to see anomalies in the temperature @ 700 hPa / 850 hPa and vertical integrated moisture content fields. For melt anomalies: it likely boils down to that high melt years have also higher summer SMB although this relation might not be significant. Furthermore, the authors do show that cloudiness increases, but fail to prove that his is the only cause. To which extend is the higher melt due to cloudiness and which extend due to advection of warmer air? What is the anomaly of temperatures at 700 hPa? This anomaly can be easily included in Figures 12 a,b. I know temperature and cloudiness anomalies are likely covarying, so disentangling might be complicated. Helpful might be the MSSA technique (Plaut and Vautard, 1994; Allen and Robertson, 1996).

(line 527): CDW intrusions cannot be proven directly with the data from this manuscript (although SSTs and wind stress are available), but sea ice anomalies are available. It takes only a few steps to verify if the hypotheses are confirmed by data, so take those steps. And if the data does not confirm this hypothesis, that must be stated as well.

Minor comments

158: The sentence on the boundary relaxation is ambiguous: It could also mean that every 6 hours the state in at the boundaries "is forced back" to ERA-Interim values.

However, I presume that every time step fields are relaxed to ERA-Interim fields with 6-hourly temporal resolution. Rephrase to remove this ambiguity. Furthermore, add the boundary relaxation zone to the graph by using shading or something else and explain in the text how wide this zone was. From eg Fig 10 I conclude it was rather narrow, explain why or add a reference.

131: polar-oriented. Did you mean “polar adapted”? Oriented is not wrong but uncommon in this meaning.

224: I would prefer if these webpage-links could be included as references so that the text becomes less disturbed. But that’s up to Copernicus to solve/decide on.

298: How this performance compares to other studies, thus MAR-full Antarctica and various RACMO2 products? Add a comment on this in the text.

321: It might be interesting to make a scatter plot of the modelled and interpolated QuickScat melt for their overlapping time period. You could color code the dots per ice shelve or drainage basin and even add regression lines per drainage basin. You don’t discuss the few spots in West Antarctica where MAR gives high melt rates – do this. And have a look at https://www.the-cryosphere.net/13/1473/2019/tc-13-1473-2019.pdf if this might be a possible explanation for your model deviations too.

359: It would be nice if these high/low SMB/melt years as listed, maybe by adding symbols in figure 7.

363: In Figure 8 your plotting two differences per frame – that makes it harder to include signs of significance. Are these differences significant? Make a comment in the text and, if possible, find a way to display if deemed relevant.

378: Cloud cover could be a poorly performing parameter – I know models in which this is the case. Verify if you find similar/equivalent patterns in the vertical integrated cloud content (please add these figures in the rebuttal letter) and state in the manuscript if similar / equal patterns are found in the vertical integrated cloud content.

379: As snowfall exceeds the SMB due to sublimation, the “95%” in the quote is a bit odd. Rephrase.

425-427: This is not necessarily true. If positive SMB anomalies occur only if NINO34 is positive and ASL-longitude is negative, then their impact on SMB is not unrelated even though NINO34 and ASL-longitude are unrelated themselves. Thus, statistically verify your claim.

438: Would it not be more straightforward to see if there is a correlation between SMB and melt rates? And if not, state this.

Table S1: Add the numbers used in Fig 1 to the table – Yes, I know they are ordered from 1 to 41, but adding the number makes it just a slightly bit easier.

Fig 1: Consider to include excluded AWS stations in the figure using a different color, as long as they are on the map. Names are not needed.

Fig 2: The lines are not explained in the figure caption. Are the lines derived using normal fitting or perpendicular fitting techniques? Colors are not different enough to identify stations in the graph, so either use more distinguishable colors or simply don’t try: give all lines the same color.

A drawback of a dot-plot is that you can’t see differences in density once the dots form a continuous cloud. It might be worth the work to calculate the dot-density per (e.g.) 0.01 C-squared (Fig 2a) and plot this point density as contour graph on top of the dots. This added information on the point-density would make a statement like line 273-274 visible from the graph, the overestimation for low wind speeds is not well visible in the point cloud.

Fig 3: I’m not fond of the graphical solution to plot SMB in greyscale – details are hardly visible nor quantifiable. For example, I have no clue what the magnitude of the SMB from MAR is near the Medley data. Replace the grey by colors and add the basin delineation in a different manner. In all solutions, more detail must become visible for

SMB ranging from 200 to 500 mm w.e. per year.

Fig 5: Replace the grey by clear colors and extend the scale to higher values than 100 mm w.e. per year – this should be obvious as you do discuss these high melt values in the main text.

Fig 9: Contours in b are labelled with 0-2-5-8 intervals, but their regular spacing looks like 0-2.5-5-7.5. Check this. Hatching is not explained – should be done here too. Hatching line thickness varies with viewer.

Plaut, G., and R. Vautard, 1994: Spells of low-frequency oscillations and weather regimes in the Northern Hemisphere. J. Atmos. Sci., 51, 210–236, doi:https://doi.org/10.1175 Allen, M. R., and A. W. Robertson, 1996: Distinguishing modulated oscillations from coloured noise in multivariate datasets. Climate Dyn., 12, 775–784, doi:https://doi.org/10.1007

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-109, 2019.