The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-109-AC3, 2019

The Cryosphere
Discussions
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EGU

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.

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Received and published: 27 September 2019

We thank Reviewer #2 for these constructive and motivating feedbacks. As for Reviewer#1 we agree with most of the objections and have considered them in the revised manuscript. As detailed in our response to Reviewer #1, we have added an explanation on why we focused on summer SMB (now in section 1).

C1 : 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.

We first would like to point out that all the composites showed a clear significant area over the coastal region of the Amundsen Sea and over the studied drainage basins, to the exception of the 700 hPa geopotential height and the humidity divergence at 850 hPa. We thank the Reviewer for his suggestion that the mean geopotential height matters less than the gradient. To circumvent this issue, we have added the composite by analyzing the geopotential pattern divided by the domain-averaged value for each DJF season. This produces much more significance than in the initial version, as shown in the modified Fig. 9 and Fig. 12. See next comment for the case of humidity divergence.

C2: 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).

We agree that the divergence of humidity transport was too noisy and, in the end, little supportive of our mechanism. We have replaced this diagnostic by the integrated vapor transport (IVT) that is calculated as (see equation in the responses.pdf in supplementary) where v is the velocity along the y-axis and g the gravity parameter. We have replaced humidity divergence composites by IVT composites in Fig.10 and Fig.11. It clearly shows that high SMB and high melt rates are linked to a strong southward vapor transport towards the drainage basins of the Amundsen Sea. The arrival of this vapor from the mid-latitude into the colder Antarctic region can arguably induce condensation and cloud formation. We have also looked at the composite of sensible heat fluxes (this has been added to the supplementary material) versus longwave downward heat fluxes. The sensible heat flux is negative for the high-melt composite, which means that energy is going from the snow surface to the air (the temperature of the snow surface is higher than the temperature of the air above), so advection of warm air above the surface is not responsible of higher surface melt. Therefore, as suggested in our initial manuscript, changing downward longwave heat flux is the main mechanism for low/high surface melt events. Increase in cloudiness and humidity transport is therefore the main driver. We have added a comment on this in section 3.2, L. 418-419. Fig.10 and Fig.11 has been changed. (Figure below has been added to supplementary material)

C3 : (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.

See our response to Reviewer #1: we have substantially expanded our discussion of this hypothesis based on further literature review and on an additional DJF sea ice composite for JJA Niño events. Although providing perfectly robust evidence of causality would require specific AOGCM experiments, we believe that several lines of evidence indicate that such physical lag is highly probable.

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.

Explanation about boundary relaxation has been changed, as well as Fig.1 where relaxation zone is now shown in white. Fig.10 has been change related to next comments.

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

Done

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.

We followed the manuscript preparation guidelines for authors (webpage, references)

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

We have added a comment in section 3.1 L-287.288. Compared to MAR-full Antarctica we present very similar biases (Agosta et al., 2019), correlation for SMB compared to observation (Glacioclim SAMBA). R=0.95 for our simulation and R=0.93 for MAR-full Antarctica, same for bias of 0.13 and 0.14 for MAR-full Antarctica. This improvement is not really significant and can be explained only by higher resolution and higher spatial variability in our simulation. A complete comparison with RACMO2 products is beyond the scope of our studies and will be the subject of future studies.

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.

We agree we have added scatter plot (Fig.6) and related discussion L.336-337.

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

We have added a table in supplementary material (Tab.S4) and not in Fig.7 because composite dates correspond to values with low/high SMB/Melt only over Thwaites and Pine Island basins and not over the model domain as explained in section 3.2 "For a sake of clarity, we only consider the Pine Island and Thwaites basin (together) as a first approach. "

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.

Fig.8 does not represents differences (composite - climatology) like other composite as the climatology is shown in grey and composite in color, we choose to plot all in the same panel as the comparison of both the high and low composites with the climatology are necessary.

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.

We have added IVT (integrated vapor transport) in Fig.10 and Fig.11 and here is the composite for integrated water vapor (kg m-2):

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

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

We do not understand the reviewer's concern with our sentence ("NINO34 and the ASL longitudinal location are not significantly connected together (Table 1), therefore their connection to SMB can be considered as independent from each other"). In the example given by the reviewer, both ENSO and the ASL migration impact SMB, but both act through independent connections (assuming linear relationships, i.e. perfectly described by correlations). It does not mean that the coincidental phases of the two indices do not explain the strongest SMB events.

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

See our response to the major comments by Reviewer#1

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

We agree, this has been done

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.

We think that display the 243 AWS can disturb the understanding of the figure, so we have not followed this suggestion.

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.

We use least-mean-square fit (linregress in python) we have added this information within the caption.

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.

We have changed Fig.2 and have added transparency on dots in order to see density differences. We have kept stations names (some might it useful) but we have changed colors (less transparent, more distinguishable). Legend has been changed and fitting method explained.

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.

We agree and we have changed Fig.3 (colormap, and range).

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.

We agree with this comment and we have changed the grey colors. As far as the color bar is concerned, we discuss high melt values but only over Thwaites and Pine Island, that's why we choose a color bar where differences in melt rate over Thwaites and Pine Island are distinguishable.

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.

Fig.9 has been changed and contours checked. We have added hatching explanation.

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

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-109/tc-2019-109-AC3-supplement.pdf

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

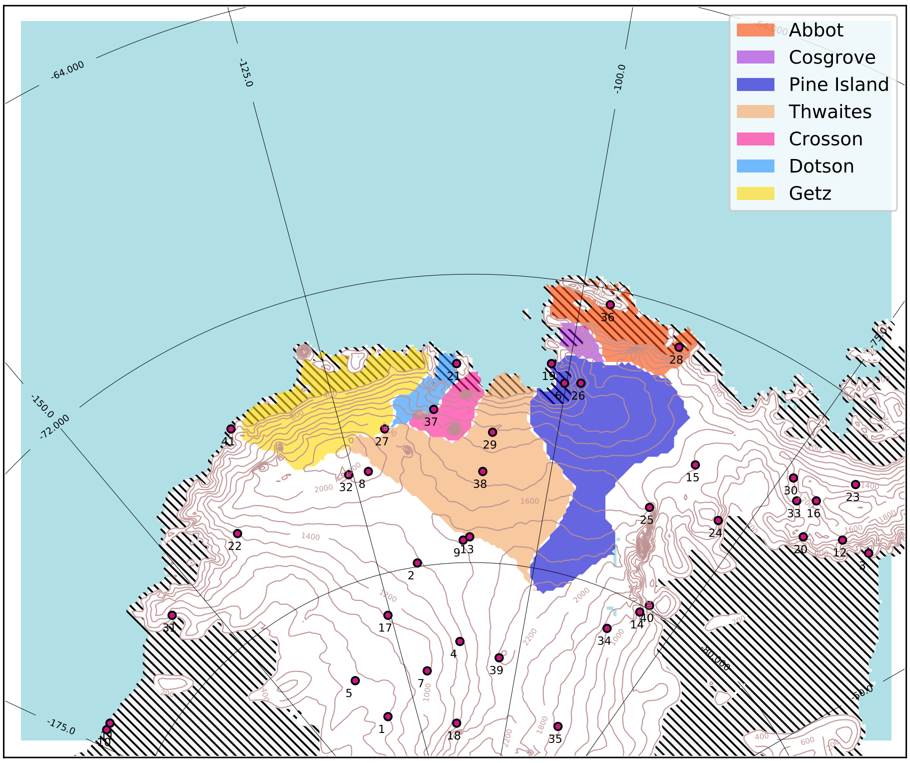

Abbot
Cosgrove
Pine Island
Thwaites
Crosson
Dotson
Getz
-64.000
-72.000
-125.0
-100.0
-150.0
-175.0

Fig. 1.

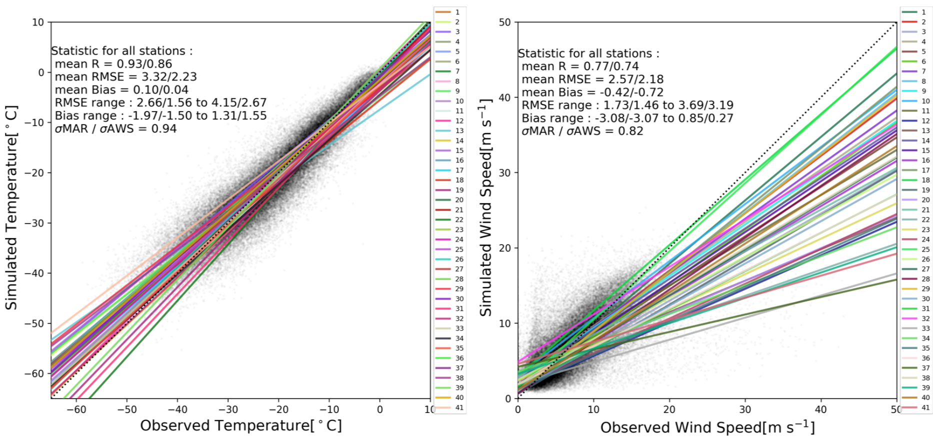

Statistic for all stations :
mean R = 0.93/0.86
mean RMSE = 3.32/2.23
mean Bias = 0.10/0.04
RMSE range : 2.66/1.56 to 4.15/2.67
Bias range : -1.97/-1.50 to 1.31/1.55
σMAR / σAWS = 0.94
Simulated Temperature[°C]
Observed Temperature[°C]
Statistic for all stations :
mean R = 0.77/0.74
mean RMSE = 2.57/2.18
mean Bias = -0.42/-0.72
RMSE range : 1.73/1.46 to 3.69/3.19
Bias range : -3.08/-3.07 to 0.85/0.27
σMAR / σAWS = 0.82
Simulated Wind Speed[m s⁻¹]
Observed Wind Speed[m s⁻¹]

Fig. 2.

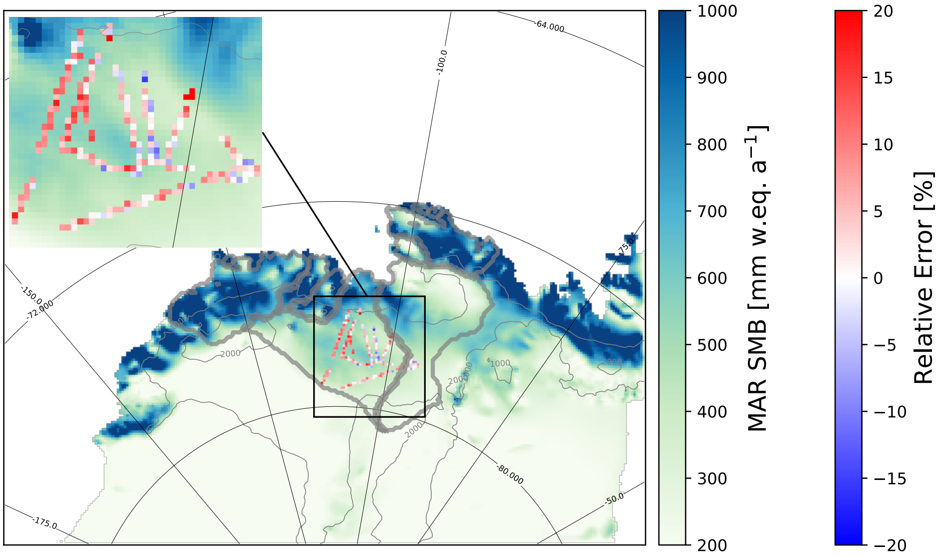

MAR SMB [mm w.eq. a^{-1}]
Relative Error [%]
1000
900
800
700
600
500
400
300
200
20
15
10
5
0
−5
−10
−15
−20
64.000
-100.0
-150.0
-72.000
-175.0
-80.000
-50.0
2000
1000

Fig. 3.

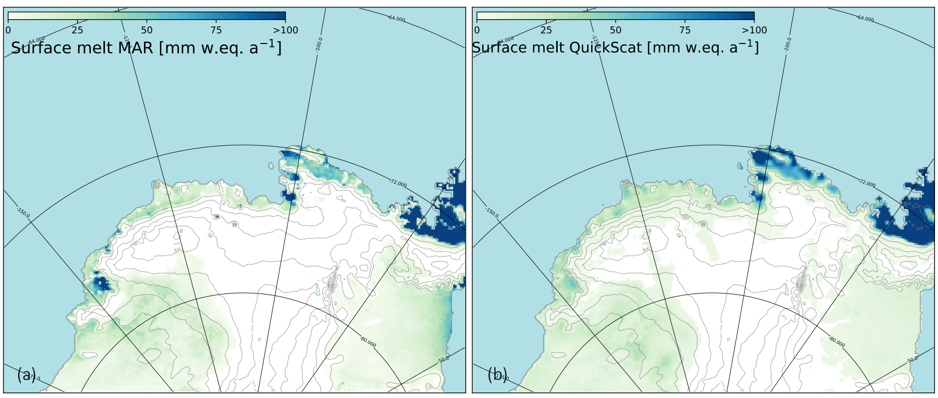

0
25
50
75
>100
Surface melt MAR [mm w.eq. a^{-1}]
Surface melt QuickScat [mm w.eq. a^{-1}]
(a)
(b)

Fig. 4.

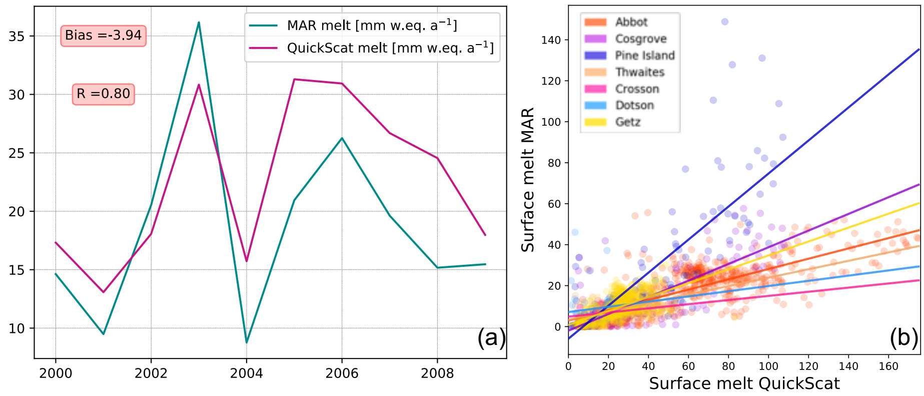

Bias =-3.94
R =0.80
MAR melt [mm w.eq. a^{-1}]
QuickScat melt [mm w.eq. a^{-1}]
(a)
Abbot
Cosgrove
Pine Island
Thwaites
Crosson
Dotson
Getz
Surface melt MAR
Surface melt QuickScat
(b)

Fig. 5.

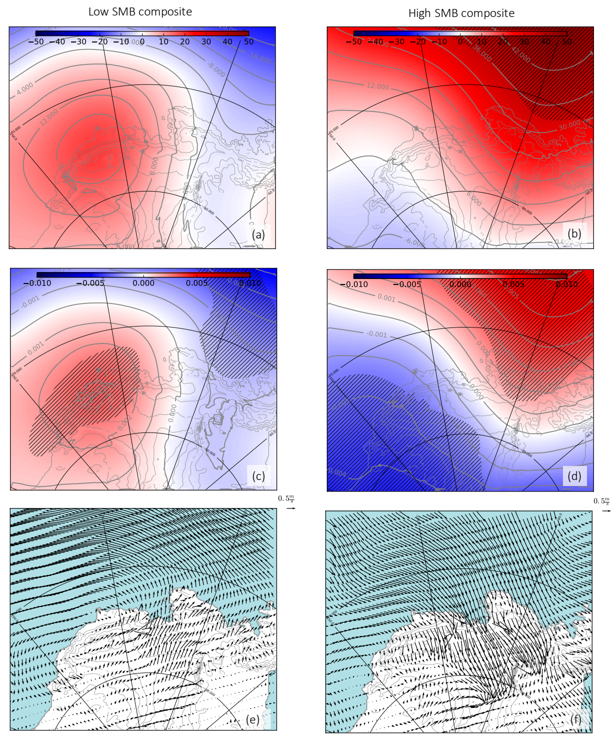
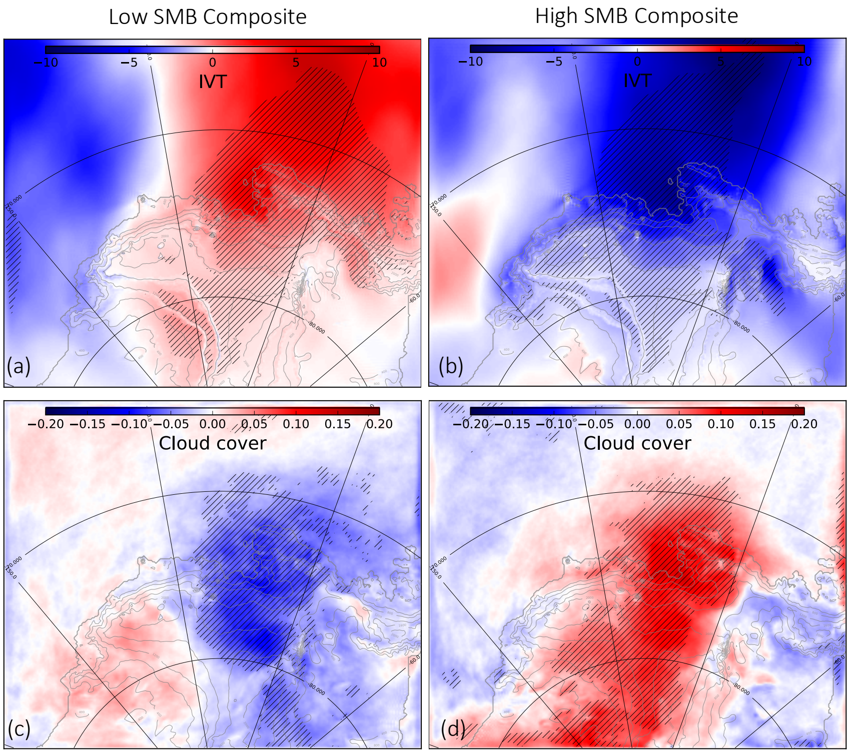
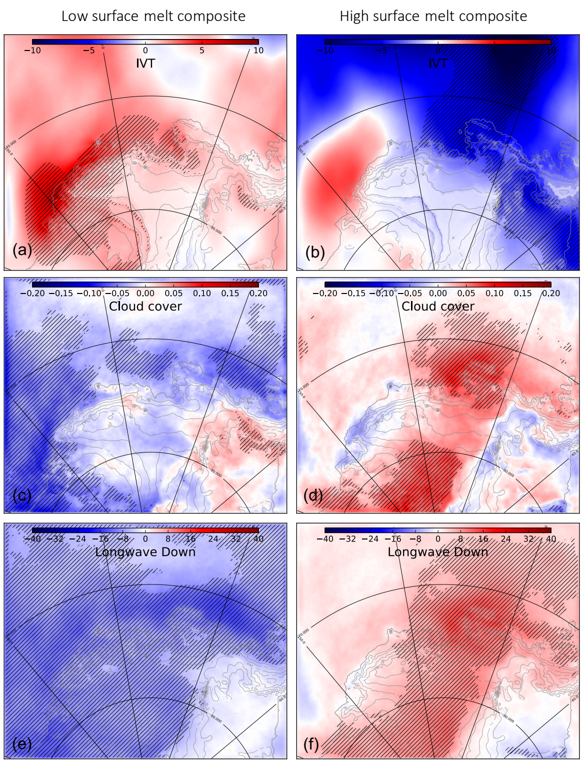

Low SMB composite
High SMB composite
(a)
(b)
(c)
(d)
(e)
(f)

Fig. 6.



Low SMB Composite
High SMB Composite
IVT
Cloud cover
(a)
(b)
(c)
(d)

Fig. 7.



Low surface melt composite
High surface melt composite
IVT
Cloud cover
Longwave Down
(a)
(b)
(c)
(d)
(e)
(f)

Fig. 8.

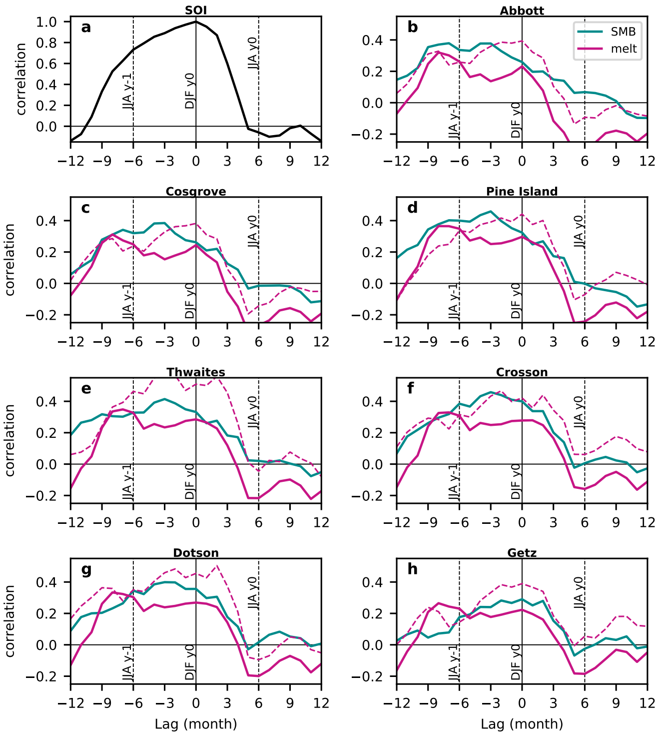

SOI
Abbott
Cosgrove
Pine Island
Thwaites
Crosson
Dotson
Getz
SMB
melt
correlation
Lag (month)
JJA y-1
DJF y0
JJA y0

Fig. 9.

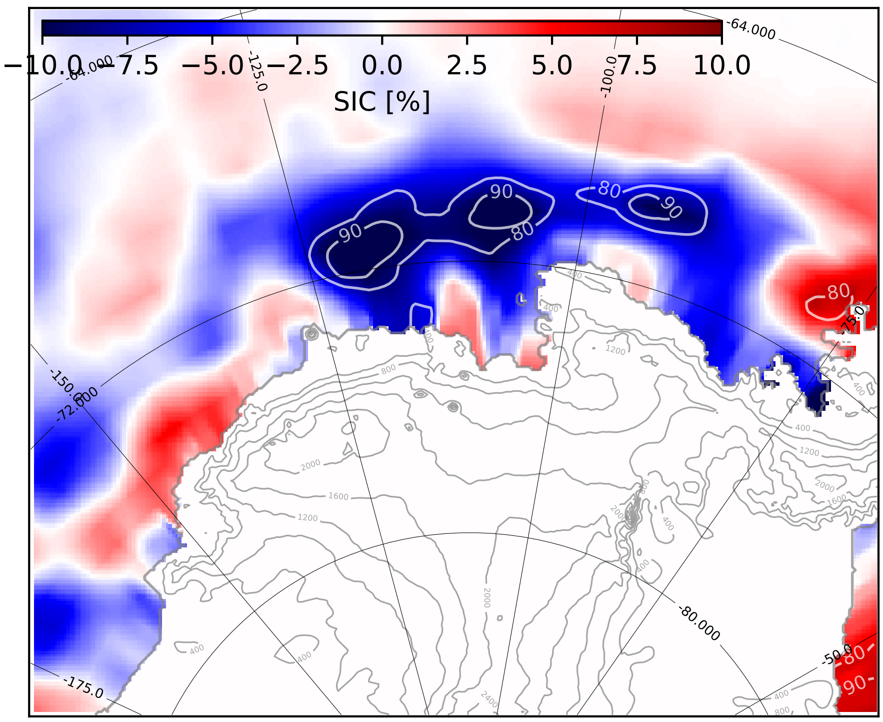

SIC [%]

Fig. 10.

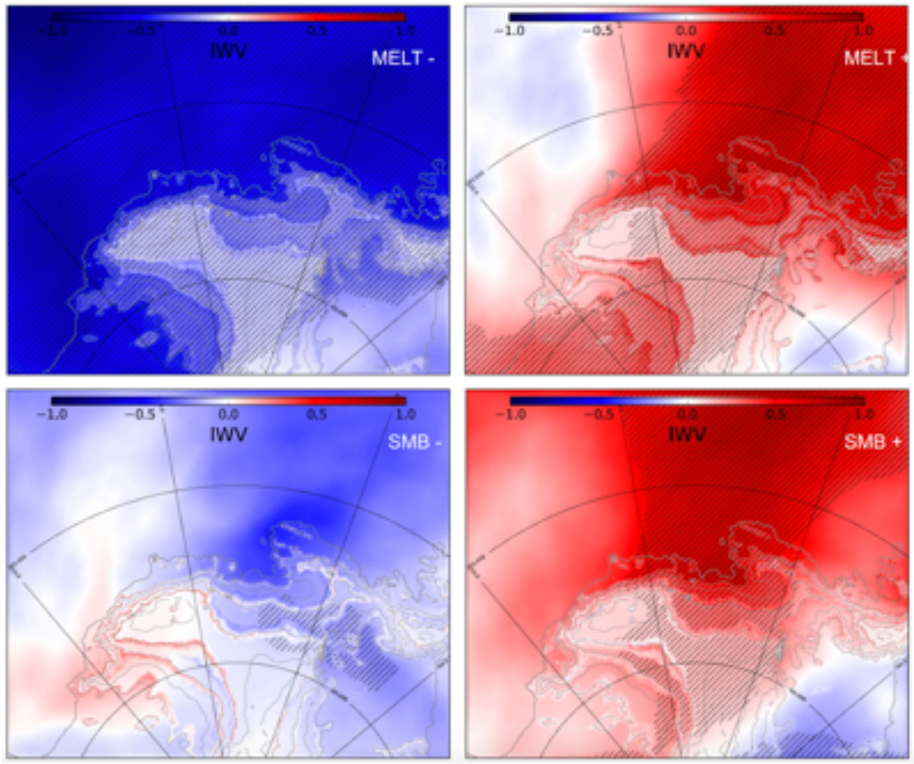


Fig. 11.