Reply to reviewer 1' comments:

The authors would like to thank the reviewer for his/her valuable comments, which helped to improve the quality of the manuscript. Kindly find below in blue our response point-by-point to the reviewer's inputs.

In the paper 'Foehn winds at Pine Island Glacier and their role in ice changes', the authors use a long reanalysis period (40+ years) in combination with other observations to investigate the role of foehn winds on the surface mass balance of Pine Island Glacier (PIG). This is an important location for understanding mass loss and atmosphere-ice-ocean connections due to the potential of raising global sea levels significantly over the next few decades. Foehn research in the Antarctic has been increasing since the early 2010's and much is now understood about their role on surface ice processes. However, in this particular region, there has been little work investigating atmospheric controls on surface ice melt and sublimation, and so the paper is novel in this aspect. It is also a particularly long period of analysis, with some more in-depth case study analyses, which are useful. The paper also highlights the importance of blowing snow and sublimation which is difficult to observe and model, and therefore the paper likely has future implications for research.

1. As there has been much research focusing on foehn winds in the Antarctic, there is much to summarise and review. However, there are excessive details in the introduction and discussion (i.e a lot of comparison to other locations in the Antarctic which may not always be necessary), which creates a very long (45 page) paper, and a very high number of references. I would suggest that some of the introduction be cut short, as well as some of the discussion. Whilst it is important to put PIG into context with other regions in the Antarctic, there is a lot of focus on the Peninsula and McMurdo Dry Valleys, which doesn't add anything to the paper, and this could be significantly shortened without compromising on the paper's results or conclusions. There are also some insignificant trends presented and discussed, which should not take up so much space in the paper given their lack of significance.
REPLY: We have shortened the Introduction, which had more than 1,500 words that have been cut by roughly 25% in the revised version, removing 10 references. The discussion about trends that are not statistically significant was substantially simplified, and they are no longer mentioned in the Discussion and Conclusions section. We have also reduced some of the paragraphs in the results sections where we agree we went too far in comparing the findings at PIG with those at other regions in Antarctica. We believe the paper is more readable now thanks to the comments/suggestions made by the reviewer.

2. The figures show important information but should be replotted – the colour scheme is not colour blind friendly, the text is quite pixelated, and many of the colours used are not intuitive and require a lengthy figure caption. Some of them are also difficult to read with thin black lines for the topography underneath colour and wind vectors. Additionally, many figures are spread across pages – which would likely be changed in the final formatted version, but paneling would help a number of the figures.
REPLY: We agree with the reviewer that the quality of the figures in the submitted version does not meet the journal's standards. We have made sure all the issues listed by the reviewer, in particular with respect to the colours used and the easiness of visualization of some of the contour lines, have been addressed in the revised version. Regarding figures spread over multiple pages, we did our best to mitigate this problem but we also want to make sure all figures can be easily visualized as quality should not be compromised. As the reviewer sates, this issue will be addressed in the final formatted version.

3. There should be some greater justification of using ERA5 for such a study when higher-resolution surface mass balance models are now available. It would be interesting to compare some of the surface mass/energy balance with studies using a coupled atmosphere-mass balance model to ensure that the results are not dependent on ERA5s representation of glaciers and snow-covered surfaces. I therefore recommend major revision.

REPLY: We thank the reviewer for raising this issue. We have now better justified in the text that the performance of ERA-5 is adequate both for the detection of Foehn events (lines 191-194) and the surface mass balance analysis (lines 219-226), in which the reanalysis precipitation (P) and melting (M) are used.

The reviewer lists in his/her specific comment #5 some modelling products that can be used for this work. We have explored such an option but in the end decided it is not feasible. They are listed below:

- Antarctic Weather Research and Forecasting (WRF) Mesoscale Prediction System (AMPS) model output is freely available for the period 2002-2016 (https://polarmet.osu.edu/AMPS/), but (i) the spatial resolution around PIG varies from 30 km in 2002 to 10 km to 2013-2016, and we know that the model-predicted temperature, moisture and wind fields at the near-surface are highly sensitive to the horizontal resolution in particular in stably stratified environments such as Antarctica, and (ii) not all the fields required for the surface mass balance analysis are available (e.g. the surface roughness length is not provided);

- The output of the Regional Atmospheric Climate Model (RACMO2) over the whole Antarctica is also freely available online for the 36-year period 1979-2014 on this website https://www.projects.science.uu.nl/iceclimate/models/racmo-archive.php. However, the horizontal resolution of this product is the same as that of ERA-5 with a much reduced vertical resolution (40 instead of 137 levels), and it also does not employ data assimilation;

- There are other modelling products such as those obtained with the Modèle Atmosphérique Régional (MAR), the United Kingdom Met Office Unified Model (MetUM) and the Consortium for Small-scale Modelling and Climate Limited-area Modelling Community (COSMO-CLM^2), as detailed in Mottram et al. (2021). However, they are mostly run at spatial resolutions of 25 km or coarser, and only a handful of variables are provided, far fewer than those given in the AMPS model outputs. As a result, their predictions cannot be used for this work.

We have also conducted high-resolution numerical simulations with the Polar version of the WRF model (PolarWRF), in particular for the case study (03-14 November 2011), in order to assess its suitability to be used in this study. The model was run in a 12 km - 4 km configuration, Fig. R1a, and the physics schemes are set up as in Zou et al. (2021), who investigated the role of Foehn effects on the surface melting at the Ross Ice Shelf. As seen in Fig. R1b, the model has considerable biases when its predictions are compared to the observations at the Evans Knoll station located north of the glacier. In fact, ERA-5 generally gives more accurate forecasts than those of PolarWRF. What is more, performing a 20 to 40 year run would take several months, and require a storage capacity of more than 100 TB, beyond our available resources.

Given this, the only option available to us is to use reanalysis data, with ERA-5 selected due to its higher spatial and temporal resolution compared to the other ones available. In addition, and as noted above and seen in Fig. R1b, for the fields used in the Foehn identification algorithm and the surface mass balance analysis, the reanalysis performance is good and hence it is suitable for this work. We hope the reviewer understands our decision to stick with ERA-5 in this study.

(a) (b)

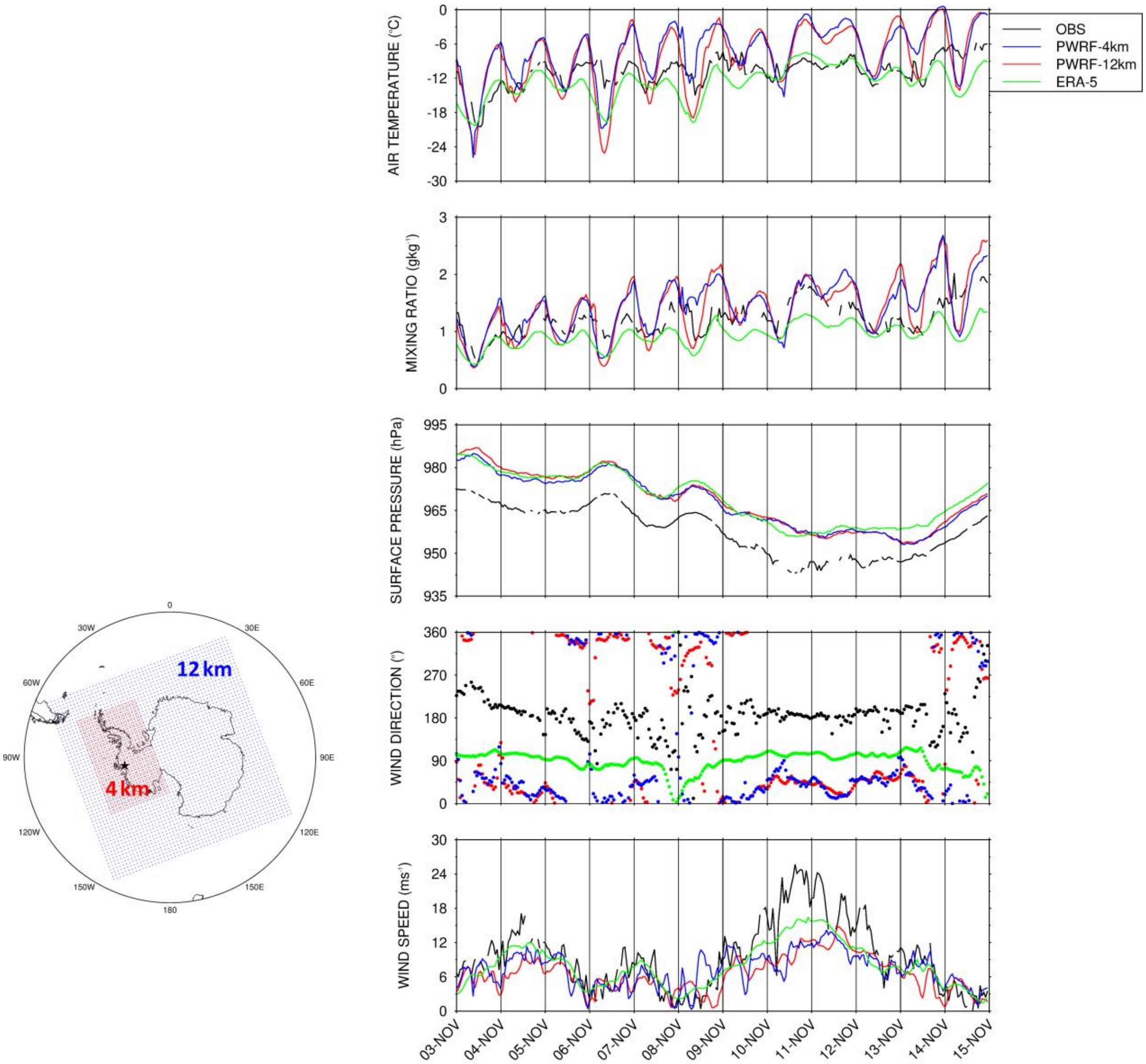

Figure R1: (a) Spatial extent of the 12 km (blue) and 4 km (red) domains used in the PolarWRF simulation. The star gives the approximate location of the Pine Island Glacier (PIG). (b) Observed (black) and predicted by ERA-5 (green) and PolarWRF's 12 km (red) and 4 km (blue) air temperature (ºC), water vapour mixing ratio (g kg^{-1}), surface pressure (hPa) and 10-m horizontal wind direction (º) and speed (m s^{-1}) at the Evans Knoll weather station (74.85ºS; 100.404ºW), located just north of PIG. The values shown for the model and re-analysis data are those at the closest grid-point to the location of the station.

Specifics:

1. Line 110: sentence structure doesn't read correctly. Suggestion: Over the Antarctic Peninsula using… , Laffin et al. 2021 found that…

REPLY: Following the reviewer's specific comment #3, we have taken the full paragraph out of the paper. In any case we do agree the wording in the referred sentence was poor and had to be rephrased.

2. Line 114-116: I wouldn't include this sentence, as the results are not significant, and it is not required for the rest of the paragraph.

REPLY: Following the reviewer's specific comment #3, we have removed the full paragraph from the manuscript. We agree that the referred sentence does not add to the discussion and should not have been included in the text had the paragraph not been taken out.

3. Line 110-121: I suggest you remove this section as it is not necessary and the introduction is already quite long. Otherwise make it smaller, or only introduce SAM when necessary later in the paper.

REPLY: We agree with the reviewer and took the referred paragraph out of the manuscript. As suggested, SAM was only introduced later in the text when it is mentioned again (lines 434-436).

4. Line 147 onwards: can you be more specific about the time range you have used for each dataset? Whilst the data availability is given, later you mention different time periods, so this needs to be clearer.

REPLY: We apologize for not having provided such information in the original submission. We have now added the periods for which each dataset was extracted to the Methods section: line 121 for ERA-5; line 132 for Sentinel 2; lines 136-137 for CERES data; lines 143-144 for the Evans Knoll weather station data (now highlighted in Figs. 1a and 6a-b); line 146 for the MODIS data used to estimate the surface melt area.

5. Line 215: Despite this statement and the large differences between ERA5 and observations, why do you still continue to use Era5? Why not consider a surface mass balance model such as MAR or RACMO or AMPS?

REPLY: We thank the reviewer for raising this issue. In the referred sentence it is stated that ERA-5 has considerable biases in the surface radiation fields. However, we do not use those fields in our analysis, except for the case study (03-14 November 2011), Fig. 7, in which they are compared to satellite-derived estimates and such discrepancies are clearly seen. The reanalysis has a superior performance in terms of 2-m temperature and relative humidity and 10-m wind speed, the fields used in the Foehn detection algorithm, with typical biases of 0.5-1.5ºC, 5-10% and 0.5-1.5 $m\,s^{-1}$, respectively, as noted by Gossart et al. (2019). We have now stated this in the text (lines 191-194).

Regarding the reviewer's second question, and as we noted in the reply to his/her general comment #3, we do not have suitable modelling products that can be used for the Foehn detection and surface mass balance analysis. Conducting high-resolution numerical simulations for the 41-year period targeted in this work (1980-2021) would take a considerable amount of time. In addition, the performance of the Polar Weather Research and Forecasting Model (PolarWRF) for the case study selected (03-14 November 2011) at a spatial resolution of 4 km, Fig. R1b, is also not optimal. Given this, we have decided to use ERA-5 for the whole study, and now better justify its suitability in the text (lines 191-194 and 219-226).

6. Line 240: Has anyone evaluated P or M in era5 for the Antarctic? Both are difficult fields to represent in reanalysis products. Can we trust ERA5 here?

REPLY: In Gossart et al. (2019) the authors compared the surface mass anomaly (i.e. precipitation minus sublimation, from which the monthly mass accumulation over the period 1980-2001 is removed) from four reanalysis datasets, including ERA-5, to that estimated from the measurements collected by the Gravity Recovery and Climate Experiment satellite over the Dronning Maud Land in East Antarctica over the period 2006-2017. ERA-5 performed the best, with a mean absolute error of ~24 $Gt\,yr^{-1}$, capturing rather well the effects of the two atmospheric rivers in 2009 and 2011. In fact, the reanalysis-predicted mass anomaly closely follows the satellite-derived estimates over the 12-year period. This gives us confidence in using ERA-5 data for precipitation (P) and melting (M) in this work. We have now stated this in the text (lines 219-226). As noted in the reply to the reviewer's general comment #3, using modelling products for the surface mass balance analysis is not possible given that continuous high spatial-resolution data over the full 41 year-period are not available.

7. Line 334: Insignificant results shouldn't take up so much space in the paper – I recommend you make the section shorter and do not focus on them. Suggestion: There is no statistically significant trend in either the frequency or duration of foehn winds on the annual scale, nor on the seasonal scale, except in summer. Then proceed to discuss summer.
REPLY: We agree with the reviewer and have shortened the referred paragraph (lines 321-335).

8. Line 337: if this line remains, it should be reworded.
REPLY: The referred sentence was rephrased for clarity (lines 322-324).

9. Figure 2: I recommend changing the colours on your bar chart and/or axis. Currently, it is not clear that the yellow bars are for the left axis and the blue for the right axis, as the colours are not the same. Additionally, the error bars are different colours. The same colours are used for different variables in figure 2c, which adds to the confusion, as there is a colour legend included on this one. Could you also make these 4 separate figures into a panel somehow, as they all contain the same variables, but are quite hard to analyse together when spread across different pages. The information they contain is also not too difficult to interpret if they are smaller.
REPLY: We have made the following changes to Fig. 2: (i) the error bars are drawn in black, with the colour of a bar the exact same as that of the respective axis; (ii) a legend is added in panels (a)-(b) to make it easier to identify what is plotted; (iii) the figure is re-arranged in a 2 x 2 layout so as to make the panels close to each other and to fit in a single page. As the reviewer states in the last sentence, it is fine that the panels are a bit smaller and the information they provide is not too difficult to interpret. We would like to thank the reviewer for his/her suggestions as how to improve the quality of Fig. 2.

10. Line 368: consider rewording to: Orr et al. used higher spatial…., to obtain…
REPLY: We agree with the reviewer and have rephrased the sentence accordingly (lines 362-364).

11. Line 368: Did Orr do this over the PIG or whole Antarctica? Not clear from this sentence.
REPLY: Orr et al. (2022) focused on the whole Antarctica, in the referred sentence we are reporting the values at PIG. We have now made it clear in the text (lines 362-364).

12. Line 374: Not just regularly, but there is at least 1 hour of sublimation every day of the year for 40 years – correct?
REPLY: If an event occurs for at least 1 h daily over the 1980-2020 period we believe it is correct to use the word “regularly” to describe its frequency of occurrence. In any case, the reviewer’s statement is more accurate and we have rephrased the sentence accordingly (lines 371-373).

13. Line 355-383: Could you not do this analysis for foehn periods vs non-foehn periods. Wouldn't this tell us more about the role of foehn, as opposed to the seasonal climatology? Perhaps it could provide more insight into the role of sublimation, as currently there is very little one can gather from this paragraph in terms of foehn effect. You could then plot the difference between foehn and non-foehn periods. MPI and SPI are relatively new terms and understanding the significance of the values is currently difficult.
REPLY: We would like to thank the reviewer for his/her excellent suggestion. We have plotted the MPI and SPI intensity and frequency difference between Foehn and no Foehn periods and the Foehn effects of

discouraging surface melting and enhancing surface sublimation are clearly seen. The discussion has been updated accordingly (lines 338-380).

14. Figure 3: consider colour-blind friendly colour scheme for figures.
REPLY: We have used a colour-blind friendly colour scale (blue to red without green shading) in Figs. 3, 4 and 6 (in the remaining a colour-blind friendly colour scale was already used).

15. Missing line numbers on page 19-20.
REPLY: We apologize for this and now made sure line numbers are displayed in all pages whenever text is present.

16. Page 19, line 3: Are these the anomalies between foehn and non foehn days? Could you be more descriptive than 'values'?
REPLY: Yes, these are anomalies between Foehn and non Foehn days. We have rephrased the sentence to make it clear to readers (line 384).

17. Page 19, line 3: Qsnow is 0 here, and Qsurf is only marginally larger in magnitude than S, so how dominant is sublimation? It could be useful to write out the names of the variables here, as I am having to go back and forth 10 pages to fully understand your results.
REPLY: "S" is not sublimation, it is actually the rate of accumulation or storage of snow at the surface, the left-hand side of the surface energy budget given by equation (2). The fact that "S" is comparable to "Q_{surf}", which gives the surface sublimation, indicates that the latter plays the dominant role in the surface mass balance. We do agree with the reviewer, the way the discussion is presented is indeed hard to follow, we apologize for this. It is indeed better to write down the variables' names followed by the respective symbol/letter in brackets. We have done so in the revised version of the manuscript (lines 382-396).

18. Page 19, line 6 onwards: Quite confusing to understand this section currently, the mixture of letters and variable names, as well as other terms coming in makes it hard to fully comprehend. I would recommend changing this to use a combination of variable names and the representative letters/symbols in brackets, so that the reader doesn't have to switch back to methods so often.
REPLY: We agree with the reviewer and, as stated in the reply to the reviewer's previous comment, we have updated the text following his/her suggestion (lines 382-396).

19. Page 19, second paragraph: I do not get the importance of including all of these other studies and in very different locations and even the Arctic – this doesn't add to the paper, as you are looking at PIG, which has a different synoptic situation and climatology to other areas. It makes the paper verbose and difficult to follow. There is also some repetition about sublimation and melt characteristics.
REPLY: We agree with the reviewer and have shortened the referred paragraph (lines 398-402).

20. Line 392: why a change of time period here? Other foehn analysis is from 1980-2020, why now 2000 onwards?
REPLY: The decision to restrict the cluster analysis to the period 2000-2020 reflects the high amount of computational power needed to run the k-clustering technique. In the 21-year period there are 1181 days for analysis, with this figure rising to 2283 days for 1980-2020, and we tested a different number of clusters from one to five to find the optimal number. In any case, we would like to note that taking the full period is unlikely to change the findings, the same two modes (AAO and SAM) will most certainly be the

dominant ones. We have stated this in the text (lines 437-439) and hope the reviewer understands the reason behind our choice.

21. Line 461: out of interest, is the initial snow depth in ERA5 an expected depth? In some locations over Greenland, snow depth is over 30m deep and corrections are required before it can be used in modelling studies.

REPLY: The snow depth in ERA-5 in the period 03 to 14 November 2011 was roughly 9.21 m w.e., in the range of those observed during field campaigns as reported by Konrad et al. (2019). We have stated this in the text (lines 503-504). We would like to thank the reviewer for bringing this issue to our attention as it will be useful for future modelling studies we may perform.

22. Line 472: perhaps more evidence that a surface melt model could have been used or coupled with ERA5, to better investigate the melting.

REPLY: While we agree with the reviewer, for the reasons mentioned in the reply to his/her general comment #3, modelling products are not considered in this work. We did conduct a high-resolution run with PolarWRF for this case study but, as shown in Fig. R1b, the model performance is poor when its predictions are evaluated against the observed measurements at the Evans Knoll station located adjacent to PIG. Given this, we rely on ERA-5 reanalysis data. In any case, and in light of the reviewer's comment, we now state in the text that further insight into the melting can be gained by running a surface balance model, which will be left for future work (lines 512-514).

23. Figure 6: a and b show a different location to the other figures, which is difficult to see at first. The topography line is quite thin and hard to read, consider replotting these figures to aid the readers interpretation.

REPLY: We thank the reviewer for raising this issue. We have improved the quality of the panels in Fig. 6 in the revised version of the manuscript. In particular, the following changes were made to panels (a) and (b): (i) the IVT anomaly vectors are only plotted for areas for which the IVT standardized anomalies exceed one, in order to make the plots less busy; (ii) the colour scale of the IVT standardized anomalies was changed so as to reduce the saturation; (iii) the line width of the coastlines was thickened so as to make it easier to visualize them; (iv) the line width of the 500 hPa geopotential height standardized anomalies was thinned, which together with (iii) makes them more distinguishable from the land-sea mask; (v) the sea-ice contours were removed from these large-scale plots; (vi) the location of the Evans Knoll weather station is given by a yellow star, so as to direct the reader's attention to the location of the Pine Island Glacier; (vii) the 2-m temperature contour is now plotted with a solid instead of a dashed red line. In panels (c) and (d), the following changes were made: (i) the mean sea-level pressure contours were removed; (ii) the topography contours were made in a darker shade of gray and are now labelled; (iii) the sensible heat flux colour scale in panel (d) now goes from -100 to +100 W m^{-2}. We believe all four panels are now much easier to interpret and would like to thank the reviewer for his/her comment that led to an improvement of the quality of the plots in Fig. 6.

24. Line 526: some results repeated which are not necessary – only mention the ones here which you will go onto discuss. For instance, the ones related to ASL location. But the repeated summary of duration is not put into context so isn't needed in the discussion.

REPLY: We agree with the reviewer and have taken out the last sentence of this paragraph that is clearly not needed in the Discussion and Conclusions section.

25. Line 542: the trend analysis did not reveal a trend – it was insignificant.

REPLY: In the revised version of the manuscript no reference to trends is made in the Discussion and Conclusions section given that they are not statistically significant.

26. Line 570: in this particular region the sublimation is larger than melt – not in all locations.
REPLY: Agreed and we have rephrased the referred sentence to make it abundantly clear (line 605).

REFERENCES:

Gossart, A., Helsen, S., Lenaerts, J. T. M., Vanden Broucke, S., van Lipzig, N. P. M. and Souverijns, N. (2019) An Evaluation of Surface Climatology in State-of-the-Art Reanalyses over the Antarctic Ice Sheet. Journal of Climate, 32(20), 6899-6915. https://doi.org/10.1175/JCLI-D-19-0030.1.

Konrad, H., Hogg, A., Mulvaney, R., Arthern, R., Tuckwell, R., Medley, B., Shepherd, A. (2019) Observations of surface mass balance on Pine Island Glacier, West Antarctica, and the effect of strain history in fast-flowing sections. Journal of Glaciology, 65, 595-604. https://doi.org/10.1017/jog.2019.36.

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., Souverijns, N. (2021) What is the surface mass balance of Antarctica? An intercomparison of regional climate model estimates. The Cryosphere, 15, 3751-3784. https://doi.org/10.5194/tc-15-3751-2021.

Orr, A., Deb, P., Clem, K. R., Gilbert, E., Bromwich, D. H., Boberg, F., Colwell, S., Hansen, N., Lazzara, M. A., Mooney, P. A., Mottram, R., Niwano, M., Phillips, T., Pishniak, D., Reijmer, C. H., van de Berg, W. J., Webster, S. and Zuo, X. (2022) Characteristics of surface "melt potential" over Antarctic ice shelves based on regional atmospheric model simulations of summer air temperature extremes from 1979/80 to 2018/19. Journal of Climate, 1-61. https://doi.org/10.1175/JCLI-D-22-0386.1.

Zou, X., Bromwich, D. H., Montenegro, A., Wang, S.-H. and Bai, L. (2021a) Major surface melting over the Ross Ice Shelf part I: Foehn effect. Quarterly Journal of the Royal Meteorological Society, 147, 2874-2894. https://doi.org/10.1002/qj.4104.