Reply to reviewer #3's comments:

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

1. For readers who are not familiar with the region, it would be helpful to have an inlet in Fig. 1 showing where the map section is located in Antarctica.

REPLY: We thank the reviewer for his/her suggestion. We have added an inlet in Fig. 1 giving readers an idea of where the study domain is located in Antarctica.

2. A reference for equation 4 is missing.

REPLY: Equation (4) is taken from Dery and Yau (2002), we now state it in the text (lines 245-246).

3. There is no information on how the constants in equations 4 and 5 were determined or where these values come from. Are these values reasonable for this region or were they determined for a different setting?

REPLY: We thank the reviewer for raising this issue. Constants a_0-a_9 in Table 1 used to estimate the blowing snow sublimation rate are obtained when Eq. (4) is calibrated for a site in the Canadian Arctic close to the Arctic Ocean, while constants B and C employed in the divergence term are obtained using observations collected in the Canadian Prairies, as noted by Dery and Yau (2002). While we agree the values may not be optimal for West Antarctica in particular for PIG, the lack of in-situ measurements precludes us from assessing whether they are reasonable for our site. This is a caveat of our methodology which we now highlight in the text (lines 261-264 and 270-272). All other constants listed in Table 1 are either universal constants (κ), standard values (ρ_{water} and ρ_{ice}) or conversion factors (ρ').

4. Fig 2: Temperatures and SHF should not be displayed as histograms. Please use a box plots.

REPLY: We have updated Figs. 2b-c accordingly and thank the reviewer for his/her suggestion.

5. In Fig 3b) right side there is a misspelling in the title 'FFOR'.

REPLY: We thank the reviewer for pointing this out and have corrected the typo in the revised version of the manuscript.

6. p12L353: What is meant by the sentence "This is expected, as the downward sensible heat flux is largely offset by the upward latent heat flux ..."? Please clarify.

REPLY: We apologize for the poorly written sentence. What we mean to state is that during Foehn events the near-surface atmosphere is warmer than the surface, leading to downward or surface-pointing sensible heat fluxes that act to increase the surface temperature. However, Foehn events are also associated with drier conditions, which results in an upward-pointing latent heat flux that generally offsets the increased sensible heat flux. As a result, the surface energy budget is controlled by the radiation (mostly longwave in the colder months) and ground heat fluxes, which may result in surface melt being less likely compared to non-Foehn days. This is explained e.g. in

Elvidge et al. (2020). We have rephrased the referred sentence accordingly for clarity (lines 360-364).

7. p12L367: The sentence "The fact that surface melting is more frequent in the adjacent Southern Ocean in foehn episodes may be ..." is misleading. It is the melting in the coastal areas that is being referred to here, isn't it?
REPLY: Yes, the reviewer is correct, we are referring to coastal areas. We have rephrased the sentence in the revised version of the manuscript (lines 366-370).

8. p13L387: Please indicate that these are area-averaged values. The indication of three decimal places is not meaningful here and is in any case within the range of uncertainty. I would give the numbers to 2 decimal places. Do measurements exist that support the numbers?
REPLY: As we state in the text, these are actually the values at the closest grid-point to PIG. We have made it clearer in the revised version and have also given the numbers to 2 decimal places (lines 394-398). As noted in the reply to the reviewer's comment #3, unfortunately there are no ground-based observations at PIG that would allow us to assess how realistic these estimates are. Snowdrift measurements can be carried out e.g. with a driftometer sensor as conducted at different sites in East Antarctica (e.g. Scarchilli et al., 2010; Gossart et al., 2017; Wever et al., 2022), but to the best of our knowledge such measurements have not been collected at PIG. We now state this in the text (lines 406-417).

9. p14L403ff: Can the statement that the blowing snow plays an important role in the surface mass balance be proven by measurements?
REPLY: Yes. For example, Scarchilli et al. (2010) noted that, at the Terra Nova Bay in the Ross Sea and from observations collected from January 2006 to January 2008, blowing snow sublimation and snow transport remove (mainly in the atmosphere) up to 50% of precipitation in the coastal and slope convergence areas with the cumulative snow transportation roughly four orders of magnitude larger than snow precipitation at that site. As stated in the reply to the reviewer's previous comment, the lack of in-situ measurements at PIG prevents us from checking whether that is the case in our target region. However, we now note in the text that such a statement has been shown to be true elsewhere in Antarctica and is feasible in this region (lines 406-417).

10. p21L471: In my pdf, a strange symbol is displayed after the number 1.5. But maybe it's just a display error on my part.
REPLY: This strange symbol is indeed in the manuscript and was supposed to be σ. We have corrected it in the revised version (line 496) and would like to thank the reviewer for pointing it out.

11. Figure 6 c) and d) are difficult to read. The map section does not correspond to the representation in Figure 1, which makes it difficult to read the map.
REPLY: We have replotted Figs. 6c-d using the same projection used in Fig. 1 and improved the overall readability of the plots in the revised version of the manuscript.

12. Figure 6e) are the values shown in the Hovmoeller diagram averaged over all latitudes (red box in Figure 1). It is unclear how the values were calculated.
REPLY: As we state in the caption, the values are averaged over the latitude band 72.5º-77.5ºS, a 5º region centered on PIG's latitude (75ºS). However, and following the reviewer's comment, we have now made this clear in the title of the plot.

13. I would call the last chapter Conclusion, as there is no discussion here.
REPLY: We have renamed the last chapter accordingly (line 576).

REFERENCES:

Dery, S. J., Yau, M. K. (2002) Large-scale mass balance effects of blowing snow and surface sublimation. Journal of Geophysical Research, 107, 4679. https://doi.org/10.1029/2001JD001251.

Elvidge, A. D., Kuipers Munneke, P., King, J. C., Renfrew, I. A., Gilbert, E. (2020) Atmospheric drivers of melt on Larsen C Ice Shelf: Surface energy budget regimes and the impact of foehn. Journal of Geophysical Research: Atmospheres, 125, e2020JD032463. https://doi.org/10.1029/2020JD032463.

Gossart, A., Souverijns, N., Gorodetskaya, I. V., Lhermitte, S., Lenaerts, J. T. M., Schween, J. H., Mangold, A., Laffineur, Q., van Lipzig, N. P. M. (2017) Blowing snow detection from ground-based ceilometers: application to East Antarctica. The Cryosphere, 11, 2755-2772. https://doi.org/10.5194/tc-11-2755-2017.

Scarchilli, C., Frezzotti, M., Grigioni, P., De Silvestri, L., Agnoletto, L., Dolci, S. (2010) Extraordinary blowing snow transport events in East Antarctica. Climate Dynamics, 34, 1195-1206. https://doi.org/10.1007/s00382-009-0601-0.

Wever, N., Keenan, E., Amory, C., Lehning, M., Sigmund, A., Huwald, H., Lenaerts, J. (2022) Observations and simulations of new snow density in the drifting snow-dominated environment of Antarctica. Journal of Glaciology, 1-18. https://doi.org/10.1017/jog.2022.102.

<u>Reply to the editor comments:</u>

The authors would like to thank the editor for taking the time to review the manuscript and share his inputs. Kindly find below in blue our response point-by-point to the queries raised.

Some questions remain unanswered in the discussions in chapters 4 and 5, such as:

1. How can it be that blowing snow sublimation and divergence have the same magnitude as snowfall? Do other studies come to the same conclusions?

REPLY: Yes, other studies also stress the importance of blowing snow sublimation and divergence in the surface mass balance. For example, Scarchilli et al. (2010) reported that, at the Terra Nova Bay and from observations collected from January 2006 to January 2008, blowing snow sublimation and snow transport remove (mainly in the atmosphere) up to 50% of the precipitation in coastal and slope convergence areas, with the cumulative snow transportation roughly four orders of magnitude larger than the snow precipitation at that site. The lack of in-situ measurements at PIG prevents us from assessing whether for Foehn events that conclusion reached with ERA-5 data is true, but it is plausible. We now note this in the text (lines 406-417).

2. What about the uncertainties of the study and what consequences does this have for the statements?

REPLY: We thank the editor for raising this issue. Indeed, some assumptions have been made in our study due to the lack of in-situ observations. We have noted this in the text and more specifically in the methodoly section and we justified why those choices were made in the text (lines 191-194, 223-226, 261-264 and 270-272). For example, the monthly statistics on the occurrence and duration of Foehn events are reasonable given the values estimated by other authors e.g. in the more Foehn-prone Antarctica Peninsula (lines 305-310). As highlighted in Fig. 7b, for the case study in November 2011, the reanalysis of air temperature follows closely that observed at a nearby weather station, giving further confidence in using ERA-5 for the identification of Foehn events for a longer time period. We made sure to highlight potential sources of uncertainty in the text while at the same time put our findings in context with those reached by other studies at other sites around Antarctica to assess whether they are plausible.

3. Can we assume that future changes in the circulation will lead to more frequent and more intense events?

REPLY: This is a very interesting question and deserves a dedicated study. From the literature, Hosking et al. (2015), and for the 11 models of the Coupled Model Intercomparison Project Phase 5 (CMIP5) that best simulate the Amundsen Sea Low, found that it will likely shift poleward in the summer and autumn seasons and eastward in autumn and winter for the Representative Concentration Pathway RCP8.5 scenario for 2051-2100. This is consistent with a trend towards a more positive Southern Annular Mode. Largely similar findings are reached for the best performing CMIP6 models, as detailed in Gao et al. (2021).

If this is the case, and following our findings in the cluster analysis, Foehn events would likely become more frequent at PIG. We have added a sentence to the manuscript to highlight this (lines 484-487).

I would ask you to look through chapters 4 and 5 again and answer the questions.

REPLY: We would like to thank the editor for his comments above. We have updated the text accordingly and re-read sections 3, 4 and 5 to make sure all statements made are sound.

REFERENCES:

Gao, M., Kim, S.-J., Yang, J., Liu, J., Jiang, T., Su, B., Wang, Y., Huang, J. (2021) Historical fidelity and future change of Amundsen Sea Low under 1.5ºC-4ºC global warming in CMIP6. Atmospheric Research, 255, 105533. https://doi.org/10.1016/j.atmosres.2021.105533.

Hosking, J. S., Orr, A., Bracegirdle, T. J., Turner, J. (2016) Future circulation changes off West Antarctica: Sensitivity of the Amundsen Sea Low to projected anthropogenic forcing. Geophysics Research Letters, 43, 367-376. https://doi.org/10.1002/2015GL067143.

Scarchilli, C., Frezzotti, M., Grigioni, P., De Silvestri, L., Agnoletto, L., Dolci, S. (2010) Extraordinary blowing snow transport events in East Antarctica. Climate Dynamics, 34, 1195-1206. https://doi.org/10.1007/s00382-009-0601-0.