RESPONSE TO ANONYMOUS REVIEWER #2

REVIEW OF DAVRINCHE ET AL., 2025 – FUTURE CHANGES IN ANTARCTIC NEAR-SURFACE WINDS: REGIONAL VARIABILITY AND KEY DRIVERS UNDER A HIGH-EMISSION SCENARIO

We thank the reviewer for their valuable and helpful comments on the manuscript. We propose to implement the following changes in a revised version.

Black = reviewer comment / Blue = author's response / *Italic* = revised text.

The manuscript focuses on projections of surface wind speeds over Anarctica, which is comparatively under researched. The approach is very comprehesive (e.g., four downscaled GCMs, as well as a budget analysis) and the figures are well made - and clearly a lot of effort has gone into this work. Additionally, in general much of the results are well explained, with a good level/balance of detail. So there is much merit to the paper.

However, unfortunately the manuscript is let down by other instances of poor writing and organisation - and indeed I would even go as far as suggesting that the manuscript in its present state was not ready for submission. These concerns are especially evident in the Introduction and Methods section, which come across as rather muddled, disorgansied, and disjointed. I am sure this is not reflective of the authors abilities and knowledge, so this really has to be remedied before the manuscript can be considered for publication. I would really suggest a very thorough rewrite/revision of many of the section is necessary, with all authors contributing.

Major comments:

1. Many parts of the manuscript come across as rather unpolished and the writing disjointed. This really needs to be improved.

We are taking this comment into account, and will make sure to polish the next version of the manuscript.

2. For example, many of the sentences in the Introduction claim something but do not include a citation for evidence. So sentences such as 'On the one hand, the greenhouse warming causes an increase of the incoming longwave radiation.'

We will replace this sentence as follows: "*On the one hand, the increase in GHG concentrations causes an decrease of the outgoing longwave radiation (Mitchell, 1989). As a consequence, the temperature inversion and thus the katabatic forcing should decrease (Van den Broeke and van Lipzig, 2002; Bintanja et al., 2014b).*" In general, we will rewrite the introduction with careful attention to the inclusion of citations for any idea described.

3. and 'Although there is a consensus on the reduction of surface forcing in climate projections'.

We will add the following references in the revised version: "*(Van den Broeke and van Lipzig, 2002; Bintanja et al., 2014b)*."

4. There are also incidences of repetition, such as in the Introduction with something along the lines of 'which is proportionate to the strength of the temperature inversion' mentioned twice

We have spotted a repetition line 34 and will remove it in the revised version.

5. and in the methods and Introduction which both mention something along the lines of 'Because of their resolution, GCMs are not expected to perform well in locations with complex topography.'

We did not mention specifically the resolution of GCMs in the introduction, but rather their ability to take into account complex topography, land–sea contrasts, boundary-layer and convective processe (L51).

6. Other instances are the preambles/motivation before the results, which just say in a slightly different fashion what was said before. Please remove all repetition, and remember that your audience/readers only need to be told something once.

We were following the article writing guidelines developed in Plaxco, 2010: "*The first sentence of each paragraph should tell the reader what you expect them to get out of the paragraph that follows, which makes their job of following it far easier. Put another way; use the opening sentence of your paragraph to state your argument, and the rest of the paragraph to make your argument.*". However, we will take into account your comment and will make our best to remove all opening sentences.

7. Also there are typos, such as '(e.g. north of Ross and Amery ice shelves and north of the Peninsula' in the Introduction (so no closure of parentheses).

We apologize for this mistake, it will be corrected in next version.

8. Mistakes such as AWS defined, and the phrase automatic weather station still used.

We have spotted two AWS definitions: l66 in the preamble and l 71. Because we will remove the preamble in the revised version, there will no longer be a repetition.

9. Very random / ad hoc approaches such as using m/s in one sentence and km/hr in the following sentence (methods). These give the feel of a rushed writing process, and of a manuscript submitted before it was really ready.

We have spotted this line 84, and modified it in the revised version of the manuscript.

10. There are also parts of it which are disorgansied, such in section 2.1 mentioning ERA5, and then ERA5 not being explained until later (also it's not explained in a logical fashion from the methods that ERA5 is being used to select the GCMs.).

In Sec 2.1, ERA5 was mentioned but not explained in the preamble. Because we are removing all preambles in the revised version, ERA5 won't be mentioned before being explained.

11. Poor paragraph structure such as section 2.1.2.

We have updated this paragraph: "*ERA5 is the latest reanalysis produced by the European Centre for Medium-Range Weather Forecasts (Hersbach et al., 2020). Its horizontal spatial resolution is ∼ 31 km and outputs are given at a hourly frequency. The assimilation system (IFS Cycle 41r2 4D-Var) uses 10 members to produce a 4D-Var ensemble of data assimilation (Hennermann and Guillory, 2019). Among various reanalysis products (MERRA-2, JRA-55, ERAI, NCEP2, and CFSR), ERA5 has been shown to perform best in capturing monthly averaged wind speeds (Dong et al., 2020).* "

12. Finally, some odd sentences such as 'We focus on the Antarctic continent, which is the source region of the katabatic forcing' in the final paragraph of the Introduction.

This will be changed in the revised version:
L58: "*We focus on the Antarctic continent,* ***where katabatic winds are developing in the sloped regions due to the quasi permanent radiative cooling by the ice sheet (Phillpot and Zillman, 1970)****, and on the winter season, as it is the season for which both the katabatic forcing and the mean wind speed are the highest (Davrinche et al., 2024).*"

13. Methods: Out of the blue it is mentioned that the subset of AWSs are selected based on their ability to represent ERA5. This is not justified. Additionally, this seems a rather strange choice, as ERA5 would also struggle to represent steep coastal gradients, so also do poorly representing katabatic winds. So justification is clearly required.

The subset of AWS is not selected based on their ability to represent ERA5. On the contrary, we show that ERA5 is not able to reproduce correctly surface wind

speed in some locations with complex topography. As we do not expect GCMs to perform better than the reanalysis over the period of available AWS observations (as stated L119), we decided to exclude AWS that were already misrepresented in ERA5, which assimilates observations in Antarctica. In the end, we exclude stations located in the Transantarctic mountains and at the interface between the continent and the ocean, which follows expectations. However, we wanted to use a rigorous method to exclude those stations.
We understand that some sentences in the manuscript might suggest that we exclude stations based on their ability to represent ERA5 instead of based on the ability of ERA5 to represent the climatology of the AWS. The following changes will thus be implemented in the revised version:

- L68: *Therefore, we* ***exclude*** *a subset of AWS based on i) the ERA5 reanalysis'* ***in****ability to represent the mean wind speed and variability in areas of complex topography, and ii) the length of available winter time series to evaluate GCMs on a representative climatic time scale.*
- L113: The title "*2.1.4 Exclusion of sites near complex topography based on performance of ERA5*" will be changed to "*2.1.4 Exclusion of sites near complex topography*"
- L113: "*These four stations exhibit the largest biases in terms of temporal variability ($R < 0.3$ and $\sigma N > 2$, which indicates that the variability in ERA5 is underestimated) and mean amplitude ($B > 30\%$, which indicates that ERA5 overestimates the mean value of the wind speed).* ***Additionally, these stations are all located at the foot of the Transantarctic mountains (Fig. 1), which justifies their exclusion in the quantitative analysis.***"

13. The correction to the AWS dataset is also poorly explained (Equations 1 and 2) – its not even clear what is being corrected, and what 1-3 and 1-6 refers to.

We will explain more clearly what equations 1 and 2 refer to:
L76: "*According to the logarithmic theoretical profile of wind speed in the boundary layer, with a constant roughness length $z_0 = 1$ mm (Vignon et al., 2017), we estimate the maximum correction between wind speed measured at the real height of the sensor and wind speed at 3m to be between -10 %* ***(for the correction from 1 to 3m)*** *and 7 %* ***(for the correction from 6 to 3m)*** *of the theoretical value:*

$$correction_{6m-3m} = \frac{log(\frac{6}{z_0})}{log(\frac{3}{z_0})} = 1.07 \tag{0.1}$$

$$correction_{1m-3m} = \frac{log(\frac{1}{z_0})}{log(\frac{3}{z_0})} = 0.90 \tag{0.2}$$

"

14. Selection criteria for GCMs: This seems to state that their performance in the Arctic is also taken into account, which is completely unjustified.

For practical reasons, we did not want to use the entire CMIP6 range of models in our study. We aimed to use a small subset of 4-5 models that 1) are not too wrong, and 2) represent a range of possible climate projections. Among CMIP6 models, we have selected those that performed the best in the Antarctic. As both poles share common physical processes, we have selected among models that performed well in the Antarctic those who also performed well in the Arctic to get to a reasonable number of models. Additionally, the choice of these four models for our study is supported by another study by Williams et al. (2024) where these models were classified among the best performing ones in Antarctica, in winter when comparing their sea ice extent, surface air temperature, zonal wind at 850 and 50 hPa to ERA5.

15. There are a lot of locations mentioned, but I don't think they are always shown on a map.

All stations are shown on Fig. 1. Regions of interest are shown on Fig.4. However, the Transantarctic mountains and the Plateau are not explicitly shown on a map. We will add them on Fig. 1 in the revised version of the manuscript.

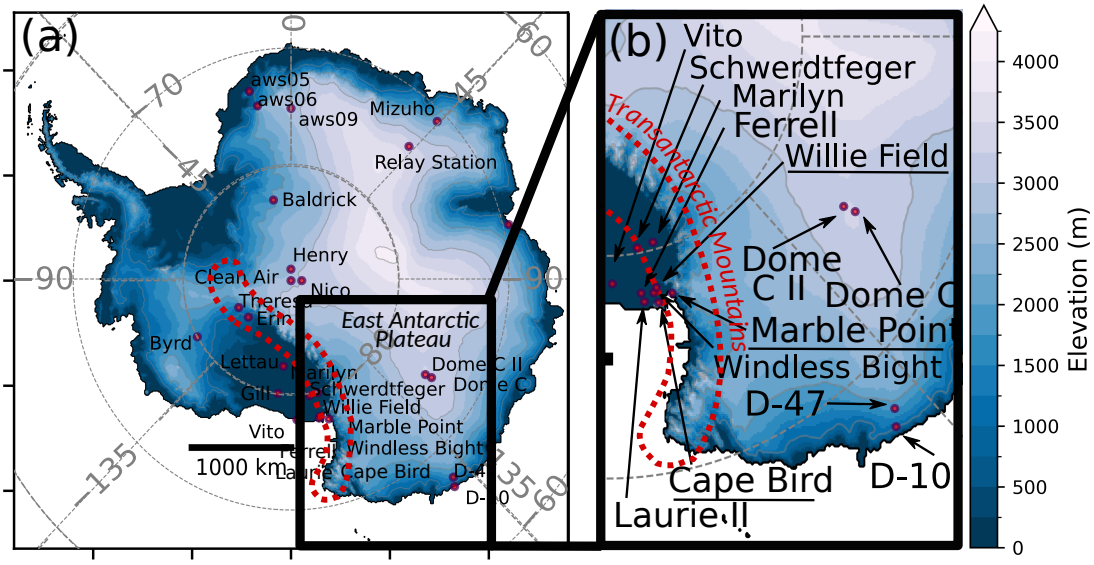

FIGURE 1. Elevation, from Bedmachine (a) over all Antarctica, (b) zoomed on the black rectangle area. Superimposed are the 28 pre-selected AWS. Stations that have been discarded because of the inability of ERA5 to properly represent winds at these locations (see Sect. 2.1.4) are underlined. Red dots indicate the Transantarctic mountains.

16. Minor comments (this is just a selection as there are a lot of 'minor' concerns that need to be addressed by a very thorough revision of the paper)

We will pay extra attention to re-reading and correcting all typos in the manuscript.

17. Abstract: Not clear what the distinction between katabatic and thermally driven winds is. I think some explanation of the term 'thermally driven' is necessary here, as otherwise the reader is lost.

We have added some explanations in the abstract:

L11: "*These drivers include local forcings related to the net radiative cooling by the iced surface as well as large-scale forcing.* ***We distinguish two types of local forcing: katabatic forcing (linked to the presence of a slope) and thermal wind forcing, which arises from horizontal gradients in the depth of the radiatively cooled surface layer.*** "

Additionally, we have changed the sentence in the introduction to: L30: "*On the other hand, the surface forcing includes a gravitational katabatic pressure gradient that is proportional to the strength of the temperature inversion and a local thermal wind created by horizontal gradients in the depth of the temperature deficit layer that acts to replenish the pressure low created by the downslope displacement of air.*"

18. The introduction mentions one mode of variability, the SAM. But what about the Amundsen Sea Low?

We have mentioned SAM as it is the dominant mode of variability in the southern hemisphere (Thompson and Salomon, 2002). However, we acknowledge that we should also have mentioned the influence of ENSO. We will add in the revised version of the paper:

L26: "*Large-scale forcing is intrinsically linked to the leading modes of variability in the Southern Hemisphere: the Southern Annular Mode (SAM) and the El Niño–Southern Oscillation (ENSO). The SAM is quantified by the SAM index, which represents the zonally averaged sea-level pressure gradient between 40°S and 65°S (Marshall et al., 2003). ENSO is characterized by the Southern Oscillation Index (SOI), based on sea-level pressure differences between Tahiti and Darwin (Bromwich et al., 2004). Both SAM and ENSO influence the strength and position of the Amundsen Sea Low (a persistent low-pressure center in the Amundsen Sea sector(Raphael et al., 2016)), which in turn modulates the frequency and trajectories of cyclones in the area (Fogt et al., 2012).*"

L35: "*On the other hand, the increase in GHG concentration drives the SAM towards a more positive phase by the end of the 21st century (Miller et al., 2006; Fogt and Marshall, 2020; Goyal et al., 2021) while the effect on the SOI remains highly uncertain (Beobide-Arsuaga et al., 2021; Ren and Liu, 2025).* "

19. The katabatic winds are also dependent on the size of the slope.

Yes, we did not mention it in this sentence, because we wanted to highlight the dependence of the katabatic acceleration to the temperature inversion, as the slope does not change between the two time periods, but we agree that it might be misleading for the reader, and will add it in the revised version: L39: " *On the other hand, the surface forcing includes a gravitational katabatic pressure gradient that is proportional to the strength of the temperature inversion and the slope angle.*"

20. Section 2.1.3: Not sure why the comment on the length of observations in the summer season is necessary.

Yes, indeed, it was a mistake, we were referring to **austral winters**. This will be corrected in the revised version.

21. And shouldn't the number of AntAWS stations mentioned here, actually be mentioned in section 2.1.1. Comes across as disorganised.

The minimum number of observations is justified based on the variability of near-surface winds derived from ERA5. Consequently, this discussion cannot be included in Section 2.1.1, as it precedes the introduction and description of ERA5. However, we have not mentioned the total number of AWS in Section 2.1.1 and will add it: L 73: "*For all **267** stations (except Zhongshan) [...]*"

22. Section 2.1.4: At least the third time that GCMs issues over representing complex orography has been mentioned. Repetition. Makes the manuscript look extremely disorganised and amateurish.

We have removed a repetition by discarding the preamble. The inability of GCMs to represent complex topography is now only mentioned once.

23. Line 118. Typo. Grill -¿ Grid

Yes, this will be corrected in the revised version.

24. Methods: Its not clear what the term 'Implausibility' is being used here for.

We understand this comment and will give more details in the revised version: "*Fraction of implausibility" is defined for each metric as the portion of the surface where the difference between historical averages in the model and ERA5 is greater than a plausible threshold set at 3 times the ERA5 interannual standard deviation (Agosta et al., 2022).*"

25. Section 2.3.2: Poor paragraph structure.

26. Section 3.1: The preamble here is inappropriate / repetition. This material should be in the Introduction or Methods, not repeated at the beginning of the

results section. This weakens the paper and makes it look disorganised.

The preamble here will be removed in the revised version. Parts of the preamble have been moved to L49 in the introduction.

27. Section 3.3: Similar comment to above, no need for the preamble.prevalence

The preamble will be removed in the revised version.

28. Line 319: SAM defined again

Yes, it is indeed the second time the SAM is defined. We will remove the bracket.

29. Section 3.3.1: Huge amount of repetition on how SAM will change.

We will thoroughly rewrite the paper to avoid unnecessary repetitions. However, it is also a matter of style and efficiency to have a few repetitions of key concept in a 25 pages manuscript. The SAM trends are described in 1 sentence in the introduction

> "*The increase in GHG concentration drives the SAM towards a more positive phase by the end of the 21st century (Miller et al., 2006; Fogt and Marshall, 2020; Goyalet al., 2021)*"

and then once in the results:

> " *This result is in agreement with previous studies that showed that the already observed increasing positive trend of the SAM will likely continue in response to increasing greenhouse gases and after the recovery of the ozone hole (which offsets the strengthening of the SAM (Bracegirdle et al., 2008)).*"

We checked for repetitions in the paper and have removed most of them. But we we would like to keep the sentences on how SAM will change as we consider that 2 mentions, 16 pages apart, does not harm the flow of the paper.

1. REFERENCES

Beobide-Arsuaga, G., Bayr, T., Reintges, A., & Latif, M. (2021). Uncertainty of ENSO-amplitude projections in CMIP5 and CMIP6 models. Climate Dynamics, 56, 3875-3888.

Bromwich, D. H., Monaghan, A. J., & Guo, Z. (2004). Modeling the ENSO modulation of Antarctic climate in the late 1990s with the Polar MM5. Journal of climate, 17(1), 109-132.

Fogt, R. L., Wovrosh, A. J., Langen, R. A., & Simmonds, I. (2012). The characteristic variability and connection to the underlying synoptic activity of the Amundsen-Bellingshausen Seas Low. Journal of Geophysical Research: Atmospheres, 117(D7).

Mitchell, J. F.: The "greenhouse" effect and climate change, Reviews of Geophysics, 27, 115–139,600 https://doi.org/10.1029/RG027i001p00115, 1989.

Phillpot, H. R., & Zillman, J. W. (1970). The surface temperature inversion over the Antarctic continent. Journal of Geophysical Research, 75(21), 4161-4169.

Ren, X., & Liu, W. (2025). Distinct anthropogenic aerosol and greenhouse gas effects on El Niño/Southern Oscillation variability. Communications Earth & Environment, 6(1), 24.

Plaxco, K. W. (2010). The art of writing science. Protein science: a publication of the Protein Society, 19(12), 2261.

Raphael, M. N., Marshall, G. J., Turner, J., Fogt, R. L., Schneider, D., Dixon, D. A., ... & Hobbs, W. R. (2016). The Amundsen Sea low: Variability, change, and impact on Antarctic climate. Bulletin of the American Meteorological Society, 97(1), 111-121.

Roussel, M. L., Lemonnier, F., Genthon, C., Krinner, G. (2020). Brief communication: Evaluating Antarctic precipitation in ERA5 and CMIP6 against CloudSat observations. The Cryosphere, 14(8), 2715-2727.

Thompson, D. W., & Solomon, S. (2002). Interpretation of recent Southern Hemisphere climate change. Science, 296(5569), 895-899.