RESPONSE TO ANONYMOUS REVIEWER #1

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.

Antarctic near-surface winds are important for blowing snow, precipitation and ice shelf stability. This article presents projections of Antarctic winds for the end of the century under a high-emissions scenario, using the MAR model with a suite of driving global models from CMIP6. A comprehensive overview of changes in winds is presented, with meticulous evaluation of the added value of dynamical downscaling. The authors also decompose the momentum budget in the regional model projections. Although a previous paper on future winds (Bintanja et al., 2014) did estimate two of these terms, to my knowledge this is the first to provide a full budget decomposition of projected winds from regional model output. The results are novel and interesting and I would support this paper's publication subject to revisions.

Major comments

The main points I raise in this review are regarding the application of the budget:

1. It's not quite clear from this analysis how the momentum budget decomposition was applied for each of the model configurations. Was your choice of parameters for diagnosing theta0 the same as in Davrinche et al. (2024)? Is there a way to test how robust your results are to the choice of Hmin?

For each model downscaled by MAR, we have performed the momentum budget decomposition following Davrinche et al. (2024). In this previous paper, in the supplement, we have validated our method and tested how robust our decomposition was to the choice of Hmin. We have also evaluated our pressure gradient force (PGF = KAT + LSC + THW) from the momentum budget decomposition (PGF) against the native pressure gradient force, which is a native output from MAR. It is also equivalent to comparing the native turbulence with the residual term. Because a lot of effort has been dedicated to the evaluation of the method in Davrinche et al., 2024, we are not planning on adding too much information about the evaluation in this paper. However, as mentioned in Point 15, we have added a quantification of the error arising from closing the momentum budget. We will also update the following sentence in the revised version:

L190: "*The method is described extensively in Davrinche et al. (2024).* ***For each model downscaled by MAR****, we compute the momentum budget in the cross- and downslope directions and we decompose it into 6 different accelerations,defined as follows:*"

2. The regional analysis is quite long and has a large figure count – you may be able to significantly improve readability by reducing the number of figure panels shown in Section 3.5, which focuses on a very specific question (the drivers of decreases). The vast majority of the changes shown in those regional panels are already visible in Figure 5.

We thank the reviewer for this suggestion. We will take it into account by moving figure 8 and 10 to the supplement.

3. A time-correlation analysis between the wind speed and budget terms would really help understand the role that surface forcing plays, instead of just looking at the change. How much variance in the July monthly mean between 1980 and 2100 do we explain with just VLSC from Equation 5, then how much when we add other terms?

We refer the reviewer to Section 4.4 of Davrinche et al., 2024 for a detailed study of the correlation between wind speed and the momentum budget terms. Given how long this paper already is, we did not want to repeat that analysis here. In this previous paper, we looked at the drivers of near-surface variability at the seasonal and 3-hourly timescale. Figure 1 and 2 (Figure 8 and 10 in Davrinche et al., 2024) are of specific interest to illustrate the role that surface plays in explaining the variance of July wind speed. We showed that the correlation between large-scale acceleration and total wind speed is high in locations where katabatic acceleration is weak. However, closer to the coast, none of the katabatic nor large-scale accelerations alone controls the variability at the 3-hourly timescale.

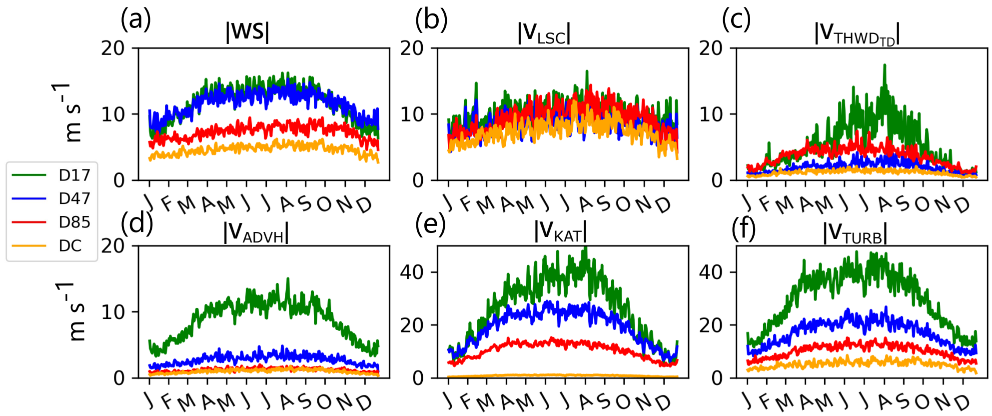

FIGURE 1. Seasonal cycle of 3-hourly nea-surface winds averaged over 10 years for (a) total wind speed, (b) wind speed equivalent to large-scale acceleration, (c) wind speed equivalent to thermal wind, (d) wind speed equivalent to advection, (e) wind speed equivalent to horizontal katabatic and (f) wind speed equivalent to turbulent accelerations. Note that the y-axis is different between panels a-d ($|WS|$, $|V_{LSC}|$, $|V_{THWD_{TD}}|$, $|V_{ADVH}|$) and panels e-f ($|V_{KAT}|$, $|V_{TURB}|$).

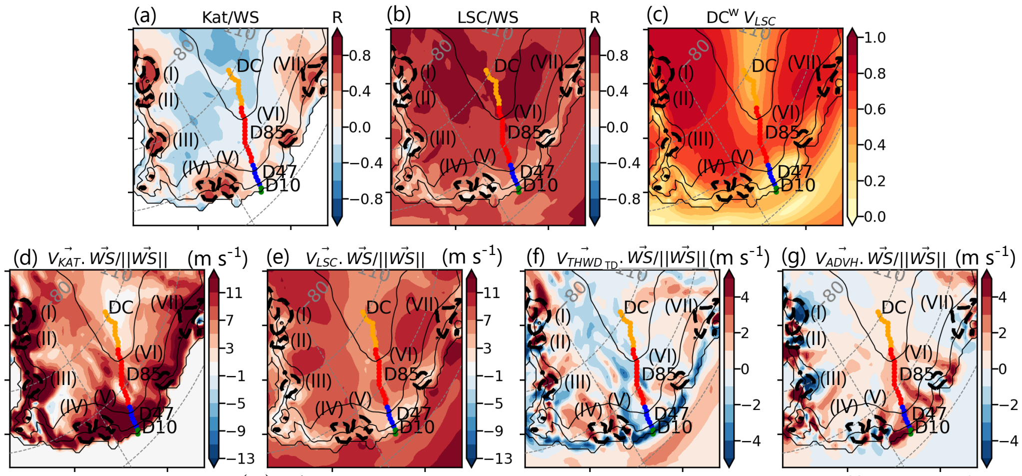

FIGURE 2. (a) Average July 2010-2020 correlation coefficient of 3-hourly katabatic acceleration and wind speed (b) Average July 2010-2020 correlation coefficient of 3-hourly large-scale acceleration and wind speed (c) directional constancy of 3-hourly large-scale wind speed. (d, e, f): Mean of 3-hourly July 2010-2020 scalar product normalised by the norm of wind speed of (d) 3-hourly katabatic wind speed and total wind speed, (e) 3-hourly large-scale and total wind speed, (f) 3-hourly thermal-wind and total wind speed, (g) 3-hourly advection and total wind speed. For the 7 panels, the dotted black line corresponds to the line for which the correlation coefficient of katabatic acceleration and total wind speed reaches 0.5. Seven zones of higher correlations are indicated: (I), (II), (III), (IV), (V), (VI) and (VII)

On longer timescales, on the continent, the large scale forcing dominates inter-annual variability (Fig. 3), while changes in near-surface forcing result in lower frequency variability. Figure 3 also shows that the dominant drivers identified on the 1980-2000 period are still the same over 2080-2100. Here, we wanted to add to the previous paper by looking specifically at the change in the different terms between the two time periods, rather than to directly look at the 20 years time correlations.

We are also limited by the fact that the momentum budget decomposition is computationally heavy, and we could not run it for 100 years, but for 2 times 20 years. We are in essence comparing two time slices, and cannot do a statistical analysis on just two numbers, showing the difference, as we did, is more appropriate.

As we understand that we cannot expect all readers to have thoroughly read Davrinche et al., 2024, we will add some key results in the revised version of the manuscript:

l32: "***At present day, large-scale forcing dominates the variability of wind speed on the interior, while closer to the coast, none of the katabatic nor large-scale accelerations alone controls the variability***

at the 3-hourly timescale (Davrinche et al., 2024). *In future projections, [...]"*

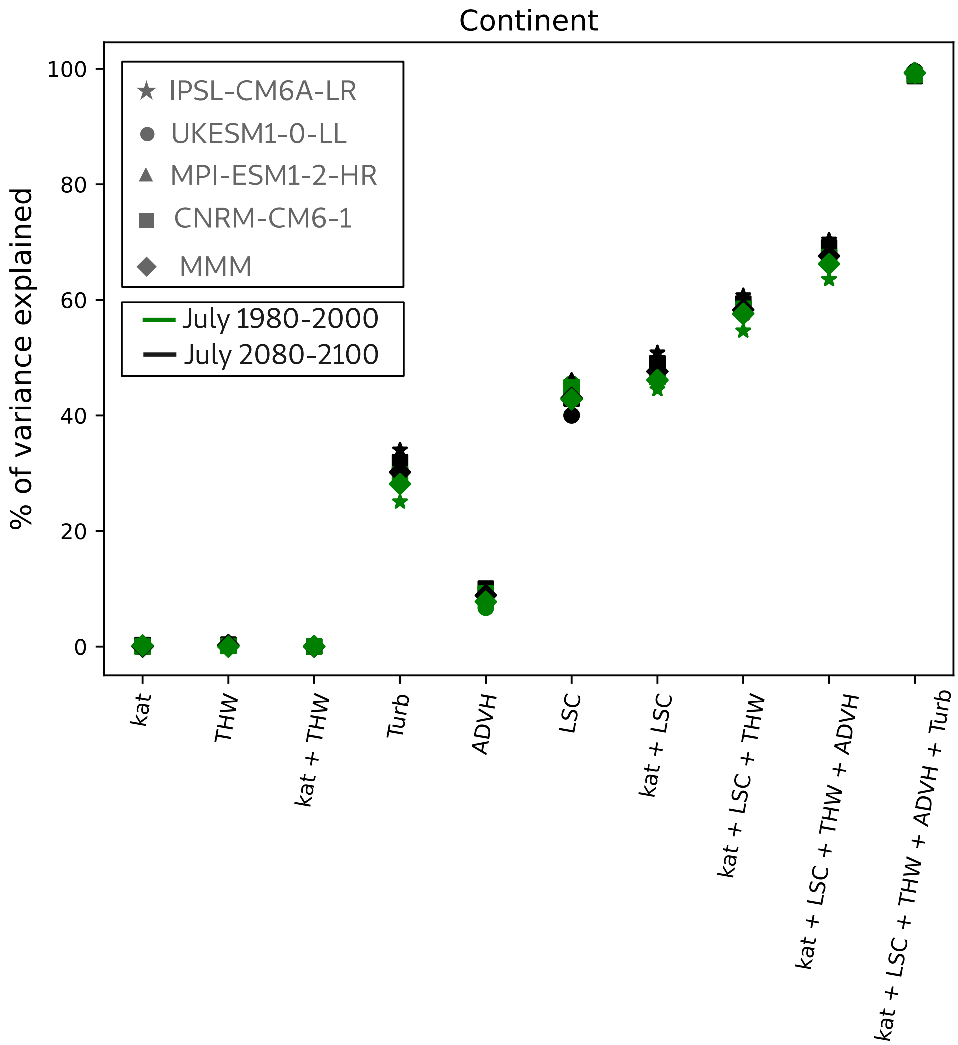

FIGURE 3. Proportion of variance of continental monthly July wind speed explained by the different accelerations for July 1980-2000 (black) and July 2080-2100 (green).

Specific comments

4. L30: A "thermal wind that acts to replenish the pressure low created by the downslope displacement of air". Could you describe what you mean by this in more detail – a thermal wind operates at large scales in a baroclinic atmosphere.

Do you mean the local thermal wind acceleration term or the thermal wind relationship used to calculate lsc?

We meant the thermal wind induced by horizontal gradients in the depth of the temperature deficit layer. It is explained in more details in Sec. 2.3.2. It quantifies the effect of baroclinicity at low levels. As we understand that it might confuse the reader in the introduction, we will rephrase:
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.*"

5. Intro: please briefly review Bintanja et al. (2014) and consider the advancements made here relative to that research. I see you reference them later – it's worth signposting early on.

The reference will be mentioned earlier in the introduction: L60 "*We mitigate GCM limitations used in previous studies (Bintanja et al., 2014) by downscaling them with using the regional atmospheric climate model MAR. We use the momentum budget decomposition to analyse how each family of drivers evolves in the different downscaled GCMs. In addition, we perform this analysis for four recent CMIP6 GCMs carefully selected on their ability to represent the large-scale circulation in polar regions. It enables us to mitigate single-model analysis issues and to test how robust potential changes are.* "

6. L54: I'm not sure if dynamical downscaling alone ensures a physically realistic simulation of boundary-layer dynamics. Rephrase perhaps?

Yes, it is not just the downscaling, but also the better model physics over snow in MAR that leads to improvements. We will rephrase it in the revised version:
"*This ensures a better resolution of the ice sheet topography as well as a more realistic simulation of boundary-layer dynamics achieved through adapted parametrizations of the interactions between the snow/ice surface and the atmosphere.*"

7. L100: Why select July and not the more usual climatological season of JJA?

The cost of computation and storage of the momentum budget terms for the four downscaled models is high and we could not afford saving all variables. Hence the limited number of studied months.

8. The supplement is very large and there's a lot of flipping back and forth between the main text and the supplement. If there is a way you can reduce the size of the supplement it would improve the flow. S1 and S2 are one equation I believe?

We feel that removing parts of the supplement would weaken our analysis. However, in order to improve the flow, we will remove as many back and forth flipping as possible. For instance:

- L399: As Ross ice shelf is now presented in the supplement, this has removed a supplement-main body flip
- L426: As Shackleton ice shelf is now presented in the supplement, this has removed a supplement-main body flip

9. S1.1: is the relative uncertainty the standard error?

Not exactly, the relative uncertainty is the standard error divided by the mean value:

$$\text{Relative uncertainty} = \frac{\frac{\sigma_{July}}{\sqrt{N_{July}}}}{|\overrightarrow{V_{July}}|} \tag{0.1}$$

We acknowledge that it might be complicated for the reader, and will update this sentence in the revised version of the manuscript:
Supplement, l3: "*To test whether datasets are long enough to be representative of a climatological period, we compute using ERA5 the minimum value of N_{July} for which the standard error on the mean value of the July wind speed between 1980 and 2020 is inferior to 5 % of the mean value.*"

10. L124: why calculate the metrics for December and the annual mean if we are only focused on July?

We wanted to give a more general result regarding the added value of downscaling by MAR regarding the representation of near-surface winds. We understand that it is not the primary focus of this paper, and will therefore present only July in the main body:

- We will compute the TPS for July only, and will move the last 6 colums of Table 4 to the supplement.
- We will update Fig. S3 as follows:

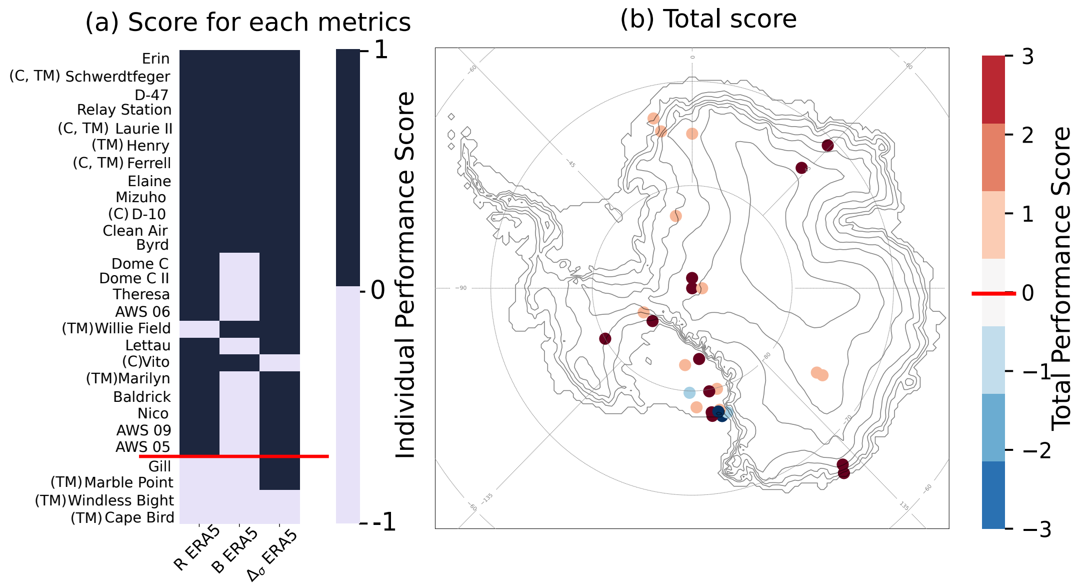

FIGURE 4. Score of the 28 pre-selected AWS stations compared to ERA5 for all July available AWS data Three metrics are considered: the correlation coefficient (R), the normalized mean bias (B) and the normalized standard deviation (σ_N). Each metrics for each station gives a score equal to -1 and 1 depending on its performance (see Sec. 2.1.4). Positive values indicate a good performance. (a) Scores for each metrics and for each stations. (b) Sum of all individual scores. Red solid line on the colorbar indicates the threshold under which stations are excluded based on their comparison with ERA5. Those stations are shaded in blue.

- Update underlined stations on Fig. 1
- Update Table 2
- Update Fig. 3 as follows:

- We will update Sec. 3.1
- We will remove Fig. S4

11. Supplement L16: check the reference to 'Figure 5 in the manuscript'.

It should indeed refer to Figure 3, thit will be updated in the revised version of the manuscript.

12. Supplement: in my PDF Figure S3 shows after Figure S4.

It will be corrected in the revised version of the manuscript.

13. Figure S3: I am a bit confused by the (b) panel colour bars. What is the left and right coloured bar showing? Maybe it would be simpler to show each

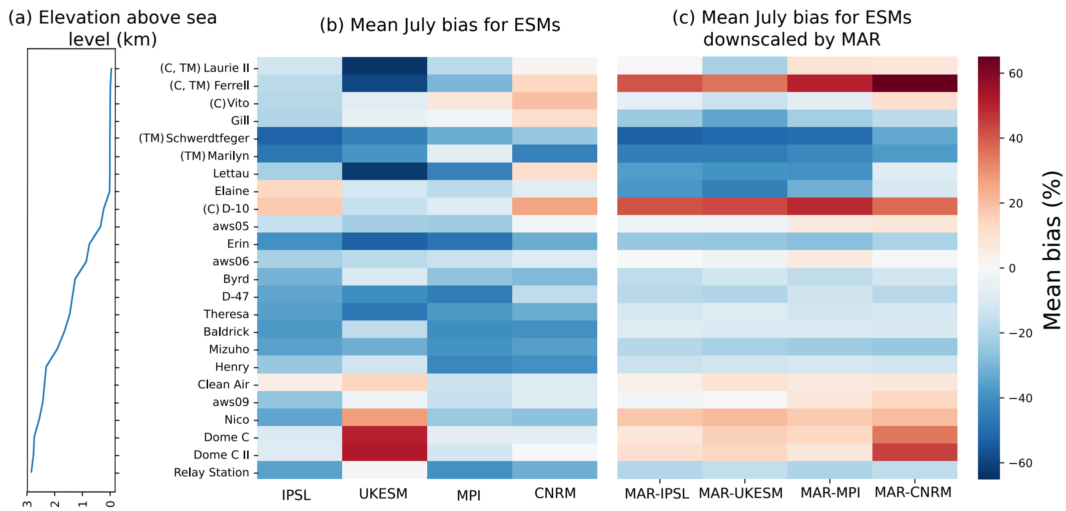

FIGURE 5. (a) Altitude of the selected stations. Mean normalized bias (B) for wind speed with regard to the AntAWS observations (B $= (|\overrightarrow{V_{GCM}}| - |\overrightarrow{V_{AntAWS}}|)/|\overrightarrow{V_{AntAWS}}|$ for (a) and B $= (|\overrightarrow{V_{MAR-GCM}}| - |\overrightarrow{V_{AntAWS}}|)/|\overrightarrow{V_{AntAWS}}|$ for (b)) for the 24 selected AntAWS stations, computed for July (b) using the GCMs, (c) using the GCMs downscaled by MAR.

individual TPS on the grid and e.g. hatch the gridcells which pass the threshold?

As mentioned above, we will update Figure S3 and replace the heatmap by a regular map with blue shaded stations indicating stations that do not pass the threshold.

14. L203 Is this strictly speaking the boundary layer? You imply here that the height at which the vertically integrated temperature deficit becomes zero is the top of the boundary layer. In East Antarctica however the temperature deficit can extend to 4km height (see e.g. Figure 3 in van den Broeke and van Lipzig, 2003). This is much deeper than the top of the stable boundary layer, which vdB and vL say is ‘poorly constrained’. Is it not more correct to say that it’s just the vertical integral of the temperature deficit? My understanding is that the temperature deficit can extend far above the boundary layer, which over the plateau may be e.g. 10-150m at Dome C, Pietroni et al., 2012: https://doi.org/10.1007/s10546-011-9675-4

Yes, it is correct. It will be mentioned in the revised version of the manuscript: ”*$\hat{\theta}$ is the vertically integrated potential temperature deficit from the top of the inversion layer. Above the inversion layer, as $\theta = \theta_0$, both $\Delta\theta$ and $\hat{\theta}$ become zero.*”

15. L226 does the residual here also encompass any errors from closing the budget (e.g. finite difference approximations) or is it directly output from the model?

Yes, we will detail that L226: "*The residual term (**TURB**) encompasses vertical advection (which is weak), turbulent drag (which opposes the other accelerations and is strong when the wind speed is high)* ***and potential errors arising from closing the momentum budget. A comparison of MAR's native turbulent acceleration and our recomputed residual turbulence as detailed in Davrinche et al., 2024 enables us to conclude that the error resulting from closing the budget in July is weak compared to the absolute value of the turbulence (ie ∼ 10 % for all models).***"

16. Figure 3a – what is the x-axis here

The former x-axis represented the stations' number. We removed these values to only keep the ticks (see Fig. 5).

17. Section 3.1 no need to restate this first para, or move to the introduction.

This paragraph will be moved to the introduction in the revised version.

18. L270 figure panel reference needed

The panel reference will be added in the revised version.

19. Figure 3: I think I missed what the collocation method is? Are you using nearest neighbour or bilinear? Is MAR regridded to the same grid as the ESMs? If not it would be useful to do this as an additional analysis to just check if the added value comes from being able to collocate a gridpoint closer to the location of the AWS in MAR.

We forgot to mention it, but we did regrid the GCMs on MAR's grid using a bilinear interpolation. We will add it in the revised version:
"*They are regridded using a bilinear interpolation on MAR's grid.*"

20. Figure 4: (v) not quite able to tell but it looks like this is not the Ronne ice shelf? It may be worth checking – in my understanding the Ronne hugs the peninsula and the Filchner ice shelf is east of that.
Yes, according to this detailed map (), region (V) is closer to Filchner ice shelf than to Ronne ice shelf. This will be modified in the figures and in the main body of the revised manuscript.
21. Table 3: in my PDF this appears below Figure 4 (but referred to beforehand).

It will be corrected in the revised version.

22. L357: my understanding is this ˆ is the vertical integral of the deficit rather than the depth of the layer

Yes, it would be more correct to state that :
"*Associated with the changes in $\Delta\theta$ at the surface, the depth of the temperature deficit layer also decreases. Therefore, $\hat{\theta}$ reduces considerably* ***on the continent****, near the coastline (Figure S7), causing a reduction in thermal wind (Figure 5d).*"

23. L357: please specify where in Figure S7 you are referring to for the coastline

As stated in the Point 22, we will add the group of words "***on the continent***", as we are focusing on onshore winds.

24. L359: in some regions (e.g. offshore of Adelie land) the thermal wind is a positive forcing term and does not oppose the katabatic wind so it doesn't necessarily increase wind speed if you reduce it.

We were focusing on onshore winds but forgot to mention it in this sentence. As stated in the points 22 and 23, we will add the group of words "**on the continent**".

25. L398: where are these regions where 'surface forcing can also contribute to significant wind speed increase'?

In L398, it is specified for "Ross ice shelf", but we should have mentioned the MAR-CNRM model. In this simulation, the large-scale acceleration decreases, and the observed significant increase in total wind speed can only be linked to changes in the surface forcing. We will change the following sentence accordingly:
"*L398: While it is clear from the analysis of Adélie and Enderby Land that significant increases in the large-scale forcing drives changes in the near surface wind speed, analysis of Ross ice shelf (Fig. 8, MAR-CNRM) indicates that surface forcing can also contribute to significant wind speed increase.*"

26. L455 you imply here that some regions have an increased wind speed due to surface forcing, and it's true that the surface forcing does increase (kat+thw) in some regions but I don't see these mapping onto obvious increases in wind speed.

What we meant here was that wind speed resulting ONLY from surface forcing was overall increasing. "*L455: Because the thermal wind opposes the dominant direction of the downslope winds (Davrinche et al., 2024), a weakening of the thermal wind forcing increases the resulting wind speed and compensates for the decrease in katabatic acceleration. The compensating effect of thermal wind is particularly pronounced in coastal East Antarctica where it often surpasses the* *decrease in katabatic forcing, leading to an overall increase of the wind speed **resulting from** the surface forcing.*"

28. PIG -> Amundsen embayment region?

This will be updated in the revised version.

29. L454 regional specifics would be helpful here as this compensating effect only applies where the katabatic winds are active

Yes, it is true. As we do not want to introduce new categories of regions depending on the elevation or slope (as we did in (Davrinche et al., 2024)), we will add some descriptions in the sentences instead: "*L454: Because the thermal wind opposes the dominant direction of the downslope winds **in the sloped regions of Antarctica ~ 250 km from the coastline** (Davrinche et al., 2024), a weakening of the thermal wind forcing increases the resulting wind speed and compensates for the decrease in katabatic acceleration **in these onshore coastal regions**. The compensating effect of thermal wind is particularly pronounced in coastal East Antarctica where it often surpasses the decrease in katabatic forcing, leading to an overall increase of the wind speed due the surface forcing **only**.*"

30. Section 4: I may have missed it but I think the added value of dynamical downscaling is an important result to mention here too?

Yes, we will add a sentence to that end in the revised version of the manuscript: "*For all GCMs, downscaling with MAR significantly improves the representation of near-surface winds, except in the Transantarctic mountains and at the interface between the coast and the ocean.*"

1. REFERENCES

Davrinche, C., Orsi, A., Agosta, C., Amory, C., & Kittel, C. (2024). Understanding the drivers of near-surface winds in Adélie Land, East Antarctica. The Cryosphere, 18(5), 2239-2256.