Response to RC1

Reviewer: The manuscript entitled "Contribution of blowing snow sublimation to the surface mass balance of Antarctica" by Gadde and van de Berg presents an update of the blowing snow model implemented in the regional climate model RACMO. The authors modified several equations and parametrisations. New model runs are compared to observational data from site D47 in Adélie Land, East Antarctica, to validate the results. The study highlights the importance of blowing snow sublimation to the surface mass balance (SMB) of the Antarctic Ice Sheet and provides a valuable contribution to better account for blowing snow sublimation in models. The addressed topic is within the scope of TC and discusses a relevant and current glaciological question. Below, I provide some specific comments and suggestions for further improving the manuscript that should be addressed prior to publication in The Cryosphere.
We thank the reviewer for spending time on reviewing our manuscript. Below, we indicate the changes made to the manuscript.

Reviewer: Specific comments (major)
Overall, the manuscript is well structured and presents the changes made to the model as well as the results. The introduction ends with an overview of the content of the individual chapters and offers the reader a clear structure. However, the individual descriptions of the changes in the model and the results are very detailed and sometimes lengthy. In general, the text could be shortened and formulated more precisely in many places. Repetitions occur in various sections but should be avoided. The discussion is too brief, and the results of this specific model are only briefly compared with another model (section 4.6). This would be an interesting comparison and further validation of the results presented here. Unfortunately, the manuscript falls short on this comparison and the main conclusions, while other descriptions are very detailed. I suggest shortening the manuscript (especially chapter 4) and discussing the relevance and implications of the study in more detail. Furthermore, the language could benefit from proofreading. The authors mention that the model runs are available, but it is not clear where the data can be found. I encourage the authors to make the data as well as the updated model code available via an open repository.

Response: We thank the reviewer for the suggestions.
Major changes in the updated manuscript are,

- Section 2.2, including the description of the blowing snow model, is rewritten.

- Both sections 4.1.1 and 4.1.2 have been shortened. The discussion of section 4.1.1 has been shortened and refocused on the evaluation of blowing snow flux and relative humidity. The evaluation of the related near-surface quantities, such as wind speed, temperature, and specific humidity, are given in the appendix. Section 4.1.2 is also shortened.

- Tables 2 and 3 in section 4.1.3 are merged.

- The manuscript is now rewritten to discuss the effect of blowing snow compared to a NO-DRIFT case scenario. Results in section 4.3 focus now on RpNew and NO-DRIFT instead of RpNew and Rp3. And the comparison between RpNew and Rp3 (section 4.3), in the appendix.

- We have performed additional analysis and comparison of monthly variation of the sublimation between CRYOWRF and RACMO in section 4.5.

- We have added a table in section 4.6 comparing the RpNew and NO-DRIFT cases to document the overall effect of blowing snow sublimation compared to the NO-DRIFT case.

- L. 35: continent-wise – should it be continent-wide
Fixed in the revised manuscript.

- L. 40: RCM is already defined in l. 36. Please use abbreviations once they are introduced.
Fixed in the revised manuscript.

- L. 50: Are you referring to specific observations here or just generally saying that RACMO was evaluated against observations?
L50 is mentioning the evaluation in 'general'. Specifics are given in the next line. Rephrased in the revised manuscript.

- L. 52: What is the difference between RACMO2.3p1 and p2 and why do the different versions suggest different blowing snow fluxes?
The updates included in RACMO2.3p2 are described in detail by van Wessem et al (The Cryosphere, 2018, https://doi.org/10.5194/tc-12-1479-2018). In brief, 2.3p2 was a retuning of RACMO in order to improve the spatial representation of the climate and surface mass balance for Greenland and Antarctica. Concerning snowdrift, in 2.3p2, the snowdrift flux was halved in order to bring the modelled snow drift fluxes more in line with the snow drift observations made in Greenland.

- L. 54: To which RACMO version are you referring here, i.e. which blowing snow module in RACMO?
We are referring to RACMO2.3p2, in which the saltation coefficient was halved, $c_{salt} = 0.192$.

- L. 64: which RACMO version? 2.3p2?
RACMO2.3p2.

- L. 103: It should read: ..serves as the boundary condition.
Fixed in the revised manuscript.

- L. 111: It seems that the verbs are missing in this sentence.
Fixed in the revised manuscript..

- L. 113: Please rewrite this sentence; it is hard to follow.
Fixed in the revised manuscript.

- L. 123: It is again a long and nested sentence. It might be easier to follow shorter sentences.
Fixed in the revised manuscript.

- L. 125: How do you justify setting d = s = 0.5?
The variables d and s have been set following Lenaerts (2012) [1]. Our primary goal with this update was to remove the numerical artefact which resulted in inexplicable variation of blowing snow flux with wind speed. We found no indication that the mobility index needed to be retuned for removing the numerical artefact, hence the values of d and s were retained, as additional modifications catering to snow particle characteristics are out of the scope of the present study for reasons explained below.

The threshold friction velocity based on [2], depend on the snow mobility index which denotes the potential for snow erosion by the wind, with $Mo = 0.75d - 0.5s + 0.5$, where d and s represent dendricity and sphericity of fresh snow. Galle et al. [2] mention that the crystal shape of fresh fallen snow does not allow a large grain cohesion in the snow pack. Therefore, this allows relatively high values of snow mobility index Mo for large d. Sintering is enhanced when the number of rounded shapes increases, so that Mo decreases when s decreases. Explicitly modelling the snow mobility index requires solving prog-

nostic equations for snow particle characteristics, evaluation of which would require observational data. However, blowing snow frequency is more widely observed and the snow mobility index was set to 0.625 ($d = 0.5$ and $s = 0.5$) to match observations [1]. A detailed discussion of the same is available in Lenaerts et al. (2012) [1].

- L. 145: What is different in the PIEKTUK-D compared to the PIEKTUK model?
PIEKTUK model [3] is a single-moment model in which only a governing equation for blowing snow mixing ratio q_b is solved. PIEKTUK-D is the 'double' moment model which has two governing equations for blowing snow quantities, one for q_b and an additional equation for particle concentration N.

- L. 147: It should read: ..follows a two-parameter gamma distribution.
Fixed in the revised manuscript.

- L. 164: I am missing a reasoning why you made exactly these six updates to the model. Can you provide a short explanation for that?
While analysing whether the updated PIEKTUK-D implementation in RACMO matches the results of the original offline PIEKTUK-D code, we found these exact issues. The six updates were needed to improve representation of snowdrift and its impact on the boundary layer in RACMO. As mentioned in the manuscript, in Rp3 many assumptions were made to simplify the implementation. These assumptions, though simplified the coupling PIEKTUK-D with RACMO, were not accurate. We made these changes to improve the coupling between RACMO and the blowing snow module and to make the calculations of PIEKTUK-D correct.

- L. 169: Please change the order of the Equations à (5) and (6)
Fixed in the revised manuscript.

- L. 170: Please mention and/or explain the entire method, not only mention the abbreviation DNS. It is 'direct numerical simulations'. Fixed in the revised manuscript.

- L. 183: There is a t missing in constitutes.
Fixed in the revised manuscript.

- L. 192: What did you test in the sensitivity analysis? Did you compare the results to observations? How did you quantify that a time step of 10 seconds produces reliable estimates?
We carried out 'time step' sensitivity test. Blowing snow model reaches equilibrium quickly after initialisation, however the equilibrium value of the blowing snow mixing ratio and particle concentration depends on the time step used for solving the evolution equations 5 and 6 (for q_b and N). We ran the model with different Δt, specifically, with default RACMO time step 600 s and reduced time step sizes. The magnitude of vertically integrated blowing snow flux was checked for each simulation. We found large difference between the values for $\Delta t = 600, 300, 100, 50$ and 20. For $\Delta t = 10$ and $\Delta t = 5$, the magnitude of blowing snow flux was nearly the same, however $\Delta t = 5$ required more number of sub-steps to reach the value. So we chose $\Delta t = 10$ s.
We have added this description in Section 2.3.

- L. 214: Please provide references when mentioning, that it's widely used in the literature.
We have added a reference in the revised manuscript.

- L. 221: You could add a wind rose or another type of graphic to illustrate the directional consistency of the katabatic winds.
We did not add the wind rose as the same is already available with the observational dataset [4, 5].

Specifically, Figure S1 in supplement [4]. We will add reference to this.

- L. 231: Please add a space character between up and to.
Fixed in the revised manuscript.

- L. 236: The description in this paragraph is a bit confusing for me. To clarify: The observations of wind speed are measured at a height of 2 m and the model results are obtained using the Monin-Obukhov theory to calculate from the first atmospheric level in RACMO (which height is this?) to a 2 m wind speed. Is this correct?
Yes, that is correct. Rewritten in the revised manuscript.

- L. 239: Which RACMO version was used for RpNew? RACMO2.3?
RACMO2.3p3.

- L. 250: Are you referring to high annual mean wind speeds or high wind speeds during events? You introduced the data already in section 3. Please avoid describing the observational data at several places in the manuscript.
We meant high annual mean wind speeds here. We have rephrased this in the revised manuscript.

- L. 251: Are the values in the table mean values for the period 2010-2012? Is there a seasonality in the model-data agreement/disagreement?
The values in the table represent the linear regression values between observations and model results. We did not observe any seasonality in the model data; the model performance remains the same.

- L. 252: I am not aware of the word underprediction. I would suggest using the word underestimates instead of underpredict here as well as in the rest of the manuscript. Same for overpredicted in l. 265.
Rephrased in the revised manuscript.

- L. 256: Please provide a better description when you are talking about which model/model result.
Since both Rp3, RpNew, and NO-DRIFT predict similar values for wind speed we just mentioned RACMO. Fixed in the revised manuscript.

- Figure 3: You are providing many numbers after the decimal point. I personally would suggest to only show two numbers to keep the plot simple and clear.
Only two digits are shown after the deimal point. Fixed in the revised manuscript.

- L. 275: I agree that the modified model RpNew shows better agreement between observed and simulated values. However, if you write about significant improvements, I would like to see p-values and/or a measure of the significance of the results.
For all the linear regressions presented in the manuscript, p-value was used to determine if the correlations are significant. We will modify the manuscript to specify the p-value of regressions. We will also rephrase/adapt the manuscript when necessary. For all the regressions we obtained a $p-value < 0.01$.

- L. 286: R2 would be 0.57 if rounding from 0.5683 as given in Fig. 3e.
Fixed in the revise manuscript.

- L. 286: Please provide statistical evidence when mentioning significant improvements. Just mentioning an R2 of 0.57 is not sufficient.
For all the linear regressions presented in the manuscript, p-value was used to determine if the correlations are significant. We will modify the manuscript to specify the p-value of regressions. We will

also rephrase/adapt the manuscript when necessary. For all the regressions we obtained a $p-value < 0.01$.

In the manuscript, we have mentioned the particular version of RACMO wherever it was unclear.

For all the linear regressions presented in the manuscript, p-value was used to determine if the correlations are significant. We will modify the manuscript to specify the p-value of regressions. We will also rephrase/adapt the manuscript when necessary. For all the regressions we obtained a $p-value < 0.01$.

Fixed in the revised manuscript.

For all the linear regressions presented in the manuscript, p-value was used to determine if the correlations are significant. We will modify the manuscript to specify the p-value of regressions. We will also rephrase/adapt the manuscript when necessary. For all the regressions we obtained a $p - value < 0.01$..

We refer to Antarctic winter, we have now specified this in the manuscript.

The 'original' height of the sensor at D47 is 2.8 m. However, Amory et al. (2020) [4] mention that, due to harsh weather conditions at D47, they could not do reset the height of the sensors owing to the elevation changes due to snow. As a result, by late December 2012, the measurement heights decreased from their initial values to 1.5 m for wind speed and direction and 0.9 m for temperature and relative humidity. We decided to do the evaluation for 2011, since the 'sensor' height in this year was around 2 m, and was easier to compare with the model results.
We have added this information in the revised manuscript.

In L 362, we intended to mention that blowing snow sublimation does not happen at the surface as the air is saturated near the surface but happens above the surface in the boundary layer. We did not mean it is the top of the planetary boundary layer. We have rephrased this in the revised manuscript to be more precise. Typical planetary boundary heights at site D47 varies between 200 – 400 m.

Please note that our primary goal was to correct the variation of snowdrift flux as function of the near surface wind speed, the goal was not to get the best agreement with the snow drift observations on the expense of the relation between wind speed and snow drift flux. Since, the blowing snow flux now 'varies' in the expected, 'power-law' fashion we believe it provides a better overall representation of the fluxes.
It is worth mentioning here that, we are not aware of any other dataset of snow drift observations in Antarctica of such high quality, therefore this is the best comparison that we could do. Furthermore,

as snow drift fluxes are increasing exponentially with the wind speed, big errors are potentially made in high-windspeed regions. The impact of snowdrift sublimation in regions with low wind speeds is inherently small, even though these regions are much larger in size.

A colourblind-friendly version of the plot is included in the revised manuscript.

By differences, we meant the different simulation time periods 2010-2020 (CRYOWRF), while 2000-2012 for RACMO. Longer simulations with RACMO2.3p2 (van Wessem et al) and RACMO2.4p1 (van Dalum et al 2024) show that the Antarctic climate of 2013-2020 is very comparable to that of the period 2000-2012. Therefore, we mentioned that they are still comparable. We have rewritten this in the revised manuscript.

Colourblind-friendly versions of the plots are included in the revised manuscript.

Palm et al. (2018) results are mostly available for cloud-free skies and blowing snow layer height greater than 30 m. Since our main goal was to compare the results with the observations from site D47, we initially did not think about filtering blowing snow fluxes to only 'cloud-free' days and blowing snow layers of height greater than 30 m. Since we did not store the multi-level data required for filtering results, it would be difficult to ascertain the reason for the slight shift. Therefore, we decided to only look at the seasonal patterns and 'qualitatively' compare our results since it was not our goal to perform a one-to-one comparison of our results with the satellite observations. We will mention the limitation in the revised manuscript in detail.

Fixed in the revised manuscript.

Antarctic winter months (April–September), summer months (October–March). Rewritten in the revised manuscript.

It is mentioned in detail in Table 4.6, there is a 41% increase in the blowing snow sublimation with RpNew when compared to Rp3. Rewritten in the revised manuscript.

The main reason is to present the results of the average over a decade, so we decided to leave 2011-2012 from the Yearly averages. In the revised manuscript we have included the averages between 2000–2012, to avoid confusion.

The main reason is to present the results of the average over a decade, so we decided to leave 2011-2012 from the Yearly averages. In the revised manuscript we have included the averages between 2000–2012, to avoid confusion.

- L. 534: I am again confused by the time frame of the experiments. Here, the period 2000-2010 is mentioned while in l. 249 values are reported for the period 2010-2012. Please clarify.
The main reason is to present the results of the average over a decade, so we decided to leave 2011-2012 from the Yearly averages. In the revised manuscript we have included the averages between 2000–2012, to avoid confusion.

Response to RC2

Reviewer: The authors introduce a new version of the blowing snow scheme in RACMO. The new model is described and a first validation comparing the model to local as well as satellite blowing snow measurements is provided. Furthermore, the authors also discuss their results in comparison to an older version of RACMO and another continent-wide simulation over Antarctica. The paper overall fits the scope of the journal. Below a list of comments if provided, which should be taken into account before considering the paper for publication.
Response: We thank the reviewer for spending time on reviewing our manuscript. Below, we indicate the changes made in the revised manuscript.

General / major comments:
For the validation against measurements as well as compared to the other model, the term "significant" is used regularly in the text. It is unclear if significance tests have been performed. Are the changes actually significant based on significance tests? And if yes, which changes are and which are not? If this is not feasible at least the wording should be adapted accordingly (see detailed comments below).
For all the linear regressions presented in the manuscript, p-value was used to determine if the correlations are significant. We have modified the manuscript to include p-value of all regressions. We will also adapted the manuscript wherever necessary.

The authors state, that there are differences between the satellite based estimate of blowing snow frequency and the model based results due to the fact that on one hand the satellite based results only start at 30 m above ground, while the results for RACMO include all the layers close to the surface. Furthermore, CALIPSO can only see blowing snow during cloud-free conditions or for optically thin clouds. Did the authors consider excluding cloudy days and the model levels below 30 m for the analysis? What is the reason for not applying these corrections to the displayed result? This would give the comparison more weight, as then the results could more directly be compared (relates to P16, L380ff).
Main goal of our study was to fix the numerical artefact in the variation of blowing snow fluxes with wind speed and evaluate it by comparing the blowing snow results with the observations from D47, Antarctica. Therefore, we did not store the multi-level blowing snow transport data (or filtered data above 30 m). After the runs were done, we decided to do a 'qualitative' comparison with Palm et al. (2018). Therefore, we had to only look at the seasonal patterns and 'qualitatively' compare our results, since it was not our goal to perform a one-to-one comparison of our results with the satellite observations. Since Palm et al. (2018) [6] observations are limited to mostly optically thin cloud conditions, we believe the observations do not give the complete picture of the blowing snow events, and we intended to report the blowing snow events as we see from the model results. Therefore, with the current data output, it is impossible to perform the said filtering. Nonetheless, we tried to filter the blowing snow flux with the cloud fraction, but as expected, this yielded high blowing snow frequency as it is the total integrated blowing snow flux from all the layers.

We have clarified this in the revised manuscript.

Minor comments:

P1, L7: In "a non-uniformly discretized ice particle radii to . . . " radii - radius
Fixed in the revised manuscript.

P4, L85: TrasnfEr - Transfer
Fixed in the revised manuscript.

P5, L113: empirircal - empirical
Fixed in the revised manuscript.

P5, L114: ", is" does not seem to fit the sentence. Remove ", is" or reformulate.
Reformulated in the revised manuscript.

P9, L223: Is wind speed measured at 2m above ground? Based on Amory, 2020 it seems to be at 2.8 m originally for station D-47. Is 2m an average over the measurement period given the elevation changes due to snow? (if a change is required take it into account accordingly for P10, L261).
Your observation is correct that the 'original' height of the sensor at D47 is 2.8 m. However, in the paper they mention that, due to harsh weather conditions at D47, they could not do reset the height of the sensors. As a result, by late December 2012, the measurement heights decreased from their initial values to 1.5 m for wind speed and direction and 0.9 m for temperature and relative humidity. The period we considered for evaluation i.e. 2011, the height was reduced from its initial 2.8 m and was lower. And the processed data that is made available on Zenodo [5], the height of the measurement is mentioned as 2 m. Furthermore, in the supplementary materials of the data paper [4] they mention that mean values for wind speed, temperature, and relative humidity are determined from the measurement level closest to 2 m.
We have clarified in the revised manuscript.

P11: In the comparison between the modelled and the measured data, it is shown that the results improve when simulating with blowing snow and even more when using the new version. Especially in lines 270ff the authors talk about significant biases and improvements. Are these statements based on a significance test? What kind of test was used? – If not either the wording should be changed or a significance test should be performed to confirm. In general, I think it would be interesting to see which improvements are significant.
For all the linear regressions presented in the manuscript, all the regression coefficients are tested for statistical significance using the p-value. Specifically, for all the regressions p-value < 0.01. We will modify the manuscript to specify the p-value of regressions. We have rephrased this in the revised manuscript to avoid confusion.

P14, Section 4.1.3: It is discussed that Rp3 overall shows a better performance in predicting the blowing snow frequency overall, while RpNew mainly improves the prediction of higher magnitude blowing snow events. It is great to see that RpNew improves for the higher magnitude events. It would, however, be interesting to see both effects discussed? Why is there an improvement for the higher magnitude events and why does it perform worse overall? What are the impacts of the decrease in performance when looking at the overall blowing snow frequency.
We have added a small discussion on why RpNew predicts comparatively lower number of events.
This is due to the comparatively 'better' performance of Rp3 during the Antarctic summer. Specifically, in Rp3, instead of using the friction velocity u_*, calculated in the land surface scheme, u_* was recalculated in every step with a simple log-law assumption (with neutral stratification). Since the assumption was
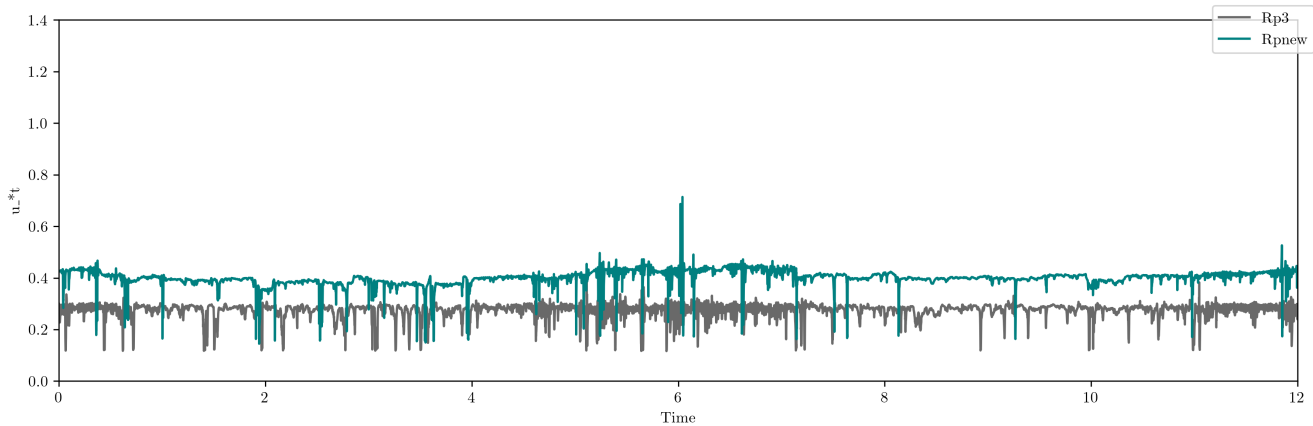
Figure 1: Threshold friction velocity for Rp3 and RpNew. Time is shown in month.

not correct, we changed it and used the friction velocity calculated from the physics module. Simply put, the assumption which was previous used (though not correct) reduced the threshold friction velocity, and thereby we observed more blowing snow events with Rp3. Figure 1 shows the threshold friction velocity used in Rp3 and RpNew, it is clear from the figure that assumptions and simplifications made in Rp3 reduced the threshold friction velocity overall which would result in higher blowing snow events compared to RpNew.
We have clarified this in the revised manuscript.

P15, L367: "... 5(a)).This analysis..." - Missing space after.
Fixed in the revised manuscript.

P21, L476: "...in under-prediction blowing snow particle concentration." - Should probably read: "...in an under-prediction of the blowing snow particle concentration."
Fixed in the revised manuscript.

P22, Table 4: From just reading the table caption and the formula, it is not clear that ERds only refers to the eroded snow blown off the continent. Please clarify.
ERds only considers the transport aspect of snowdrift. ERds is positive in case of erosion due to divergence of the snowdrift flux, and negative if convergence of the snowdrift flux brings snow to a grid box. So when ERds is negative at but less than the snow drift sublimation, snow drift still has a net erosion effect.
Furthermore, as ERds only considers the snow redistribution, the spatially integrated impact on the SMB is zero as long as drifting snow is not blown of the ice sheet. That is the reason why ERds is locally very important, but integrated over the ice sheet insignificant.
We have clarified this in the revised manuscript.

P22, Table 4: For "(a) Difference between RpNew and RP3, and (b) SMB difference between RpNew (2000-2010) with CRYOWRF (2010–2020)": Add information about the two differences shown. E.g. ... in Gt (% of xxx). To which value is the percentage related?
It is the percentage change in the quantities compared to previous version i.e. Rp3. It is calculated as the (RpNew- Rp3)/(Rp3). The word 'difference' might not be accurate here, we have rewritten this as % change in the revised manuscript.

P22, Table4b and P23, L509ff: The time periods of the RACMO and CRYOWRF simulations are not the same (RACMO: 2000-2010; CRYOWRF: 2010-2020). The difference in the between the simulated
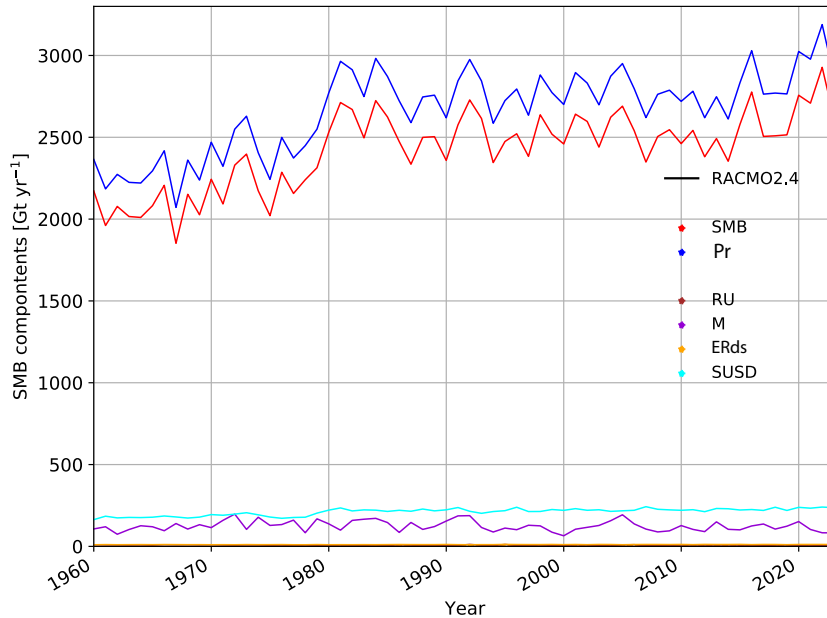
Figure 2: Variation of SMB and its components from RACMO2.4, newest version of RACMO with additional physics update. The figure is a part of the manuscript in preparation.

time periods is not discussed. How much might the different periods influence the difference in the results between the two models?

Our experience with RACMO runs suggest that the total sublimation does not vary much in the decade between 2000-2020. Therefore, we do not expect a large difference in the results, especially sublimation, during this time period.

To elaborate, after the submission of the current manuscript, the blowing snow updates along with other Physics updates were introduced in RACMO, version 2.4p1 [7]. Figure 2 shows the yearly average of SMB components for RACMO 2.4p1. The figure is a part of the manuscript in preparation. As can be seen from the figure the sublimation (SUSD) predicted by RACMO2.4p1 does not vary much between 2000-2020, and therefore we think it is justified to compare the results.

We have mentioned the differences in the time periods and their possible influence in the revised manuscript.

P23, L516ff: The authors state that they can see a trend that the surface sublimation is reduced in the presence of blowing snow sublimation. This is very interesting, can this hypothesis be discussed in more detail. And as a second step, how can it be shown that this trend is not present in CRYOWRF, compared to what? (Same for P24, L556ff).

This can be further explored by comparing the blowing snow sublimation for RpNew or Rp3 with NO-DRIFT case. We focused more on comparing RpNew and Rp3 in the initial version of the manuscript and therefore, we did not include the surface sublimation plots for the NO-DRIFT case in Figure 9. In the revised manuscript, we will add additional discussion related to this.

On observing this trend with CRYOWRF, it would be difficult to show this without a simulation of CRYOWRF with the blowing snow model switched-off. However, the data provided by Gerber et al. (2023) does not include a simulation without the blowing snow sublimation. However, we will include monthly sublimation from CRYOWRF in our plots (see Figure 3), from the figure we see that major difference between Rp3 and RpNew comes in winter months when blowing snow sublimation is dominant. Increase in blowing snow sublimation of around 6 Gt/yr in winter is balanced by the 'condensation' of approximately 6 Gt/yr. As you can see in the figure, CRYOWRF does not show such a 'condensation' as the surface sublimation during winter from CRYOWRF is nearly zero. This indicates that such an effect
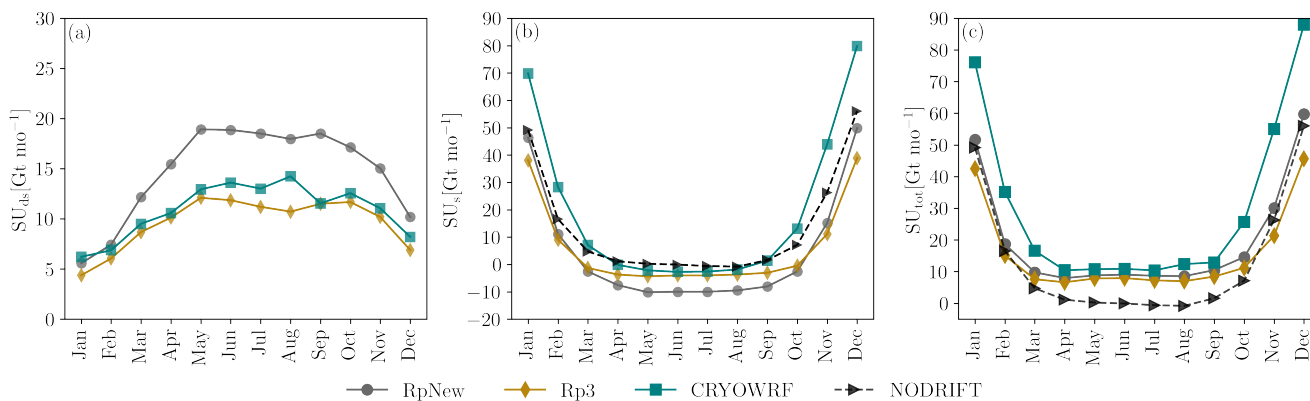
Figure 3: Monthly variation of surface-, blowing snow, and total sublimation.

is not present within CRYOWRF. But, for a definitive answer, we would need a simulation of CRYOWRF without the blowing snow model, and can be explored in the future.
We have added a discussion related to this in the revised manuscript.

P23, L518: The authors state that the difference in erosion due to snow being blown off Antarctica is "significant". Is this based on a significance tests? What test has been used? Please specify or reformulate.
We have reformulated this sentence in the revised manuscript.

P23, L527: I think it should read "radius classes" instead of "radii classes"
Fixed in the revised manuscript.

P24, L536: "insitu observation from site D47": observation -¿ observations
Fixed in the revised manuscript.

P25, L570: The authors state that the RACMO data is available, but they don't say where. Please add a link to the repository from which the data can be downloaded.
A link to the dataset has been added in the revised manuscript.

References

[1] J. T. M. Lenaerts, M. R. van den Broeke, S. J. Déry, E. van Meijgaard, W. J. van de Berg, S. P. Palm, and J. Sanz Rodrigo. Modeling drifting snow in antarctica with a regional climate model: 1. methods and model evaluation. *Journal of Geophysical Research: Atmospheres*, 117(D5), 2012.

[2] H. Gallée, G. Guyomarćh, and E. Brun. Impact of snow drift on the antarctic ice sheet surface mass balance: possible sensitivity to snow-surface properties. *Boundary-Layer Meteorology*, 99:1–19, 2001.

[3] S. J. Déry and M. K. Yau. A bulk blowing snow model. *Boundary-Layer Meteorology*, 93:237–251, 1999.

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[5] C. Amory, C. Genthon, and V. Favier. A drifting snow data set (2010-2018) from coastal adelie land, eastern antarctica. *Data*, 2020.

[6] S. P. Palm, V. Kayetha, and Y. Yang. Toward a satellite-derived climatology of blowing snow over antarctica. *Journal of Geophysical Research: Atmospheres*, 123(18):10–301, 2018.

[7] Christiaan T van Dalum, Willem Jan van de Berg, Srinidhi N Gadde, Maurice van Tiggelen, Tijmen

van der Drift, Erik van Meijgaard, Lambertus H van Ulft, and Michiel R van den Broeke. First results of the polar regional climate model racmo2. 4. *EGUsphere*, 2024:1–36, 2024.