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 we intend to make in the 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 will modify the manuscript to specify the p-value of regressions. We will also rephrase/adapt the manuscript when 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 subsequently to 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). We decided to do a 'qualitative' comparison with Palm et al. (2018) after the runs were done. 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) [1] 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. Simply put, 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 yields high blowing snow frequency as it is the total integrated blowing snow flux from all the layers.
We will clarify this in the revised manuscript.

Minor comments:

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

P4, L85: TrasnfEr - Transfer
We will fix the typo during revision.

P5, L113: empirircal - empirical
We will fix this during revision.

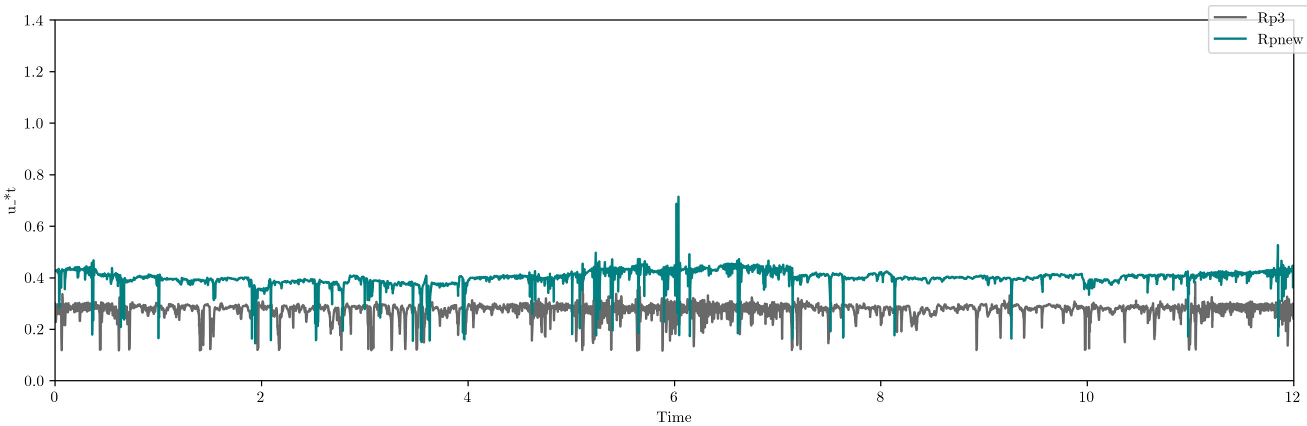

Figure 1: Threshold friction velocity for Rp3 and RpNew. Time is shown in month.

P5, L114: ", is" does not seem to fit the sentence. Remove ", is" or reformulate.
We will reformulate this during revision.

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 [2], the height of the measurement is mentioned as 2 m. Furthermore, in the supplementary materials of the data paper [3] they mention that mean values for wind speed, temperature, and relative humidity are determined from the measurement level closest to 2 m.
We will also mention this 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 signficiance using p-value. Specifically, for all the regressions p-value < 0.01. We will modify the manuscript to specify the p-value of regressions. We will also rephrase/adapt the manuscript if necessary.

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 will add additional discussion about why RpNew predicts 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 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.
Towards overall blowing snow frequency, preliminary analysis tells us that at site D47, this behaviour is mostly seasonal, in Antarctic winter when the wind speeds are high,the blowing snow events are predicted well (as you can see in Fig. 4 of the manuscript). The decreased performance is mostly in summer, however this needs to be quantified.
We will calculate the blowing snow events in Antarctic winter and summer for both Rp3 and RpNew and include this information in the revised manuscript.

P15, L367: "… 5(a)).This analysis…" - Missing space after.
We will fix this 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."
We will fix this 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 will clarify 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 (Rp3 - RpNew)/(Rp3). The word 'difference' might not be accurate here, we will rewrite this during revision.

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 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 [4]. Figure 2 shows the yearly average of

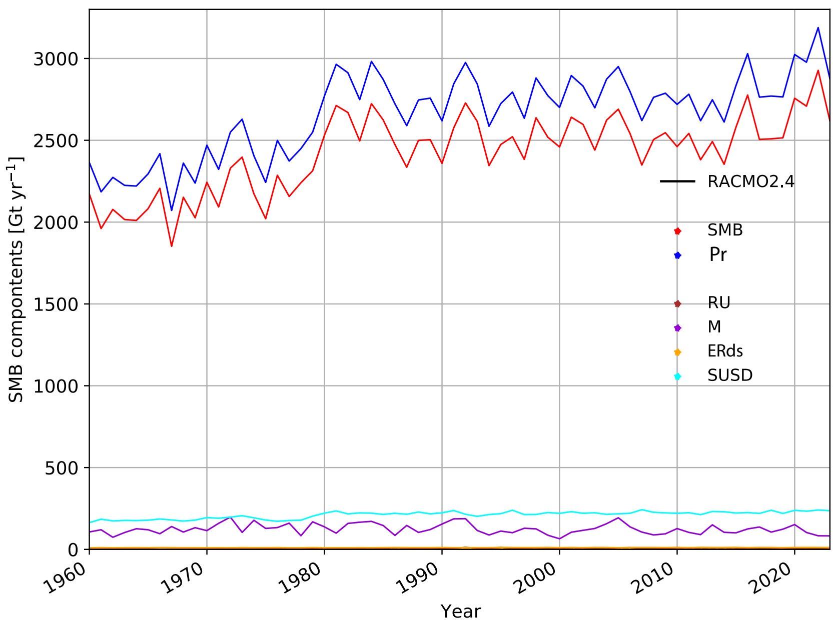

Figure 2: Variation of SMB and its components from RACMO2.4, newest version of RACMO with additional physics update. The figure is a part of the manuscript in preparation.

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 will mention the difference in the time-periods and its 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 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 will add 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.

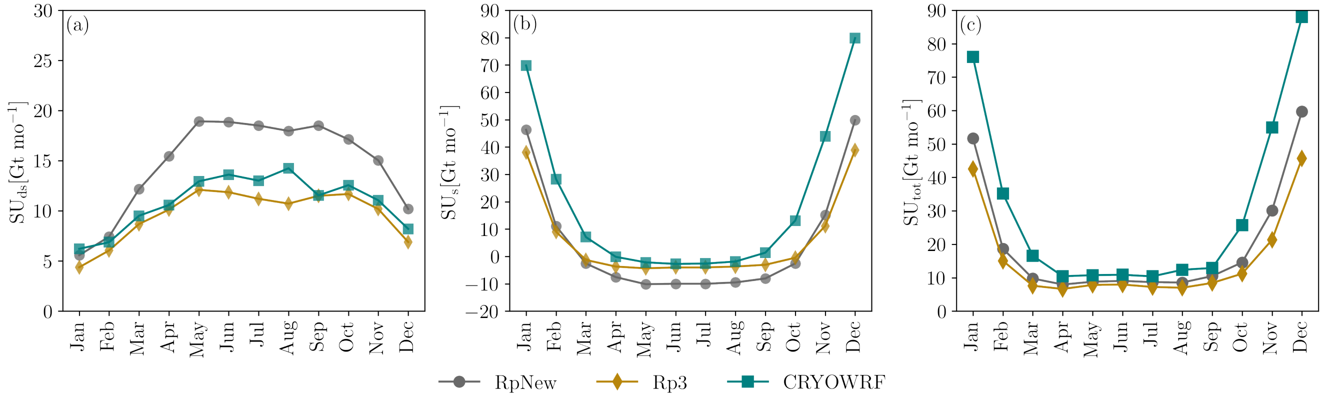

Figure 3: Monthly variation of surface-, blowing snow, and total sublimation.

We will reformulate the sentence.

P23, L527: I think it should read "radius classes" instead of "radii classes"
We will fix this during revision.

P24, L536: "insitu observation from site D47": observation -¿ observations
We will correct this during revision.

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.
We will upload the datasets on public repository before final revision and provide a DOI to the same.

References

[1] 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.

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

[3] C. Amory. Drifting-snow statistics from multiple-year autonomous measurements in adélie land, east antarctica. *The Cryosphere*, 14(5):1713–1725, 2020.

[4] 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.