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 we intend to make in 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. We intend to shorten the manuscript in few sections elaborated below. To further compare CRYOWRF and RACMO, we will modify sections 4.4 and 4.5. In section 4.4, we will compare the sublimation patterns from CRYOWRF and RACMO. In section 4.5, we will add a comparison between the monthly blowing snow sublimation from CRYOWRF and RACMO, to further explore how blowing snow sublimation and surface sublimation vary within the two models.
Major changes envisioned include,

- We will shorten/rewrite sections 2.2 which includes the description of the blowing snow model.
- We will shorten section 4 by mergin sections 4.1.1 and 4.1.2. We will be refocusing the discussion in section 4.1.1 mostly on the comparison of relative humidity and blowing snow flux, since relative humidity is the most important quantity for sublimation and changes are observed in this. We will also shorten the text in these two sections.
- We will merge tables 2 and 3 in section 4.1.3.
- We will refocus the discussion on comparison between RpNew and NO-DRIFT instead of RpNew and Rp3. As such we intend to include the comparison betwen RpNew and Rp3 (section 4.3), in appendix.
- We will compare the sublimation pattern over Antarctica from CRYOWRF and RACMO in section 4.4.

- We will modify section 4.5 to include a comparison between monthly sublimation changes of CRYOWRF and RACMO.
- We will also add an additional table in section 4.6 comparing RpNew and NO-DRIFT case to document the overall effect of blowing snow sublimation when compared to NO-DRIFT case.

Specific comments (minor)
- L. 35: continent-wise – should it be continent-wide
We will fix this during revision.

- L. 40: RCM is already defined in l. 36. Please use abbreviations once they are introduced.
We will modify this 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. We will rephrase this to avoid confusion.

- 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.
We will fix this in the revised manuscript.

- L. 111: It seems that the verbs are missing in this sentence.
We will rephrase this during revision.

- L. 113: Please rewrite this sentence; it is hard to follow.
We will rewrite this sentence.

- L. 123: It is again a long and nested sentence. It might be easier to follow shorter sentences.
We will rewrite this sentence.

- 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 prognostic 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.
We will fix this 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)
We will fix this in the revised manuscript.

- L. 170: Please mention and/or explain the entire method, not only mention the abbreviation DNS.
Thanks for noticing. It is 'direct numerical simulations'. We will fix this in the revised manuscript.

- L. 183: There is a t missing in constitutes.
We will fix this 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 will briefly mention this in the revised manuscript.

- L. 214: Please provide references when mentioning, that it's widely used in the literature.

We will add the references 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.
We will fix this 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. We will rephrase it to make it clear.

- 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 will rewrite this to avoid repetition.

- 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 values of linear regression between observations and model results. We will explore the seasonality and include it in the manuscript.

- 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.
We will rephrase this in the revised manuscript to avoid confusion.

- 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. We will rephrase this 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.
We will modify this 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.
We will fix this in the revised 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 $p-value < 0.01$.

- L. 293: Here, you are referring to RACMO2.3p2. In the previous paragraphs, you often only mention RACMO. Are you referring to RACMO2.3p2 when writing RACMO? Please either specify each time the version you are referring to or mention once that you are referring to a specific version.

Sorry about the confusion, we will carefully rephrase this in the revised manuscript.

- L. 298-304: Please provide correlations, R2 or another statistical measure to prove that RpNew predicts reasonably well the magnitude and occurrence of the blowing snow or that RpNew successfully predicts blowing snow mass flux reliably.

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. 314: Please stay consistent throughout the manuscript and use Eq. instead of equation.

We will fix this in the revised manuscript.

- L. 322: Again, please provide statistical evidence.

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. 348-351: Are you referring to Antarctic winter or Nov – Jan winter months? Same for summer.

We refer to Antarctic winter, we will specify this in the revised manuscript.

- L. 356: What is the reasoning that you investigate the year 2011 and not 2010 or 2012?

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 will add this information in the revised manuscript.

- L. 362: Please provide a height estimate for the upper part of the boundary layer.

We will include the planetary boundary layer height in the revised manuscript.

- L. 368Q: The given study investigates an area with high katabatic winds and it is highlighted how important the wind speed estimation is. Please elaborate why this case study provides sufficient and reliable estimates to transfer and extent the results from this study to continent-wide estimations of blowing snow sublimation, especially in areas where the wind speeds are low throughout most of the year.

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.

Figure 6: Please provide a version of this plot with colourblind-friendly colours (for instance in the appendix).
We will include a colourblind-friendly version of the figure in the revised manuscript.

L. 386: You are mentioning that there are differences between the studies, but they are still comparable. Please, if you mention differences, then elaborate on them and provide a reasoning, why they are still qualitatively comparable.
By differences we meant the different simulation time period 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 it of the period 2000-2012. Therefore, we mentioned that they are still comparable. We will rephrase this to be more precise.

- Figures 7 and 8: The colour scales are not colourblind friendly and, in several cases (e.g. Fig. 8e), it is hard to see which colour indicates 0.
We will replot the figures with colourblind friendly colormaps.

- L. 434: How do you explain that your model has the maximum blowing snow sublimation slightly shifted in its location compared to the result from Palm et al.?
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.

- L. 443: Remove to between RpNew and lead.
We will fix this in the revised manuscript.

- L. 463: Please specify what months are considered as winter and summer.
We will rewrite this in the revised manuscript.

- L. 477: Please quantify large differences.
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. We will rephrase the sentence to be more precise.

- L. 495: Why is the period here 2000-2010 while the simulations are from 2000- 2012?
Main reason is to present the results of the average over a decade, so we decided to leave 2011-2012 from the Yearly averages.

- L. 511: Here, you mention that the results are for the period from 2000 to 2010. In l. 249, you are referring to a period from 2010 to 2012 which is confusing to me. Why are you using different time periods for diefferent comparisons?

See previous response. We will also present the yearly average for 2010-2012 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.
See previous response. We will also present the yearly average for 2010-2012 to avoid confusion.

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.

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

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