Revision of
'Intermediate-complexity Parameterisation of Blowing Snow in the ICOLMDZ AGCM: development and first applications in Antarctica'

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This document contains the response to a review of 'Intermediate-complexity Parameterisation of Blowing Snow in the ICOLMDZ AGCM: development and first applications in Antarctica' submitted to EGUSPHERE for possible publication in Geoscientific Model Development. Comments from the Reviewer are in black and answers are in blue. Paragraphs that have been added or modified during the revision process are copied in purple.

Reviewer #2

The following is a review of "Intermediate-complexity Parameterisation of Blowing Snow in the ICOLMDZ AGCM: development and first applications in Antarctica" By Étienne Vignon and others.

This manuscript describes the integration and evaluation of a blowing snow parameterization for Antarctica. Blowing and drifting snow on the surface of ice sheets, particularly Antarctica, has been shown to be a nontrivial contribution to surface mass balance. However, representation of this process is included in few regional-scale models used to estimate ice sheet surface mass balance. This study is novel in that it investigates the utility and computational burden of including an intermediately complex parameterization of blowing and drifting snow into an atmospheric general circulation model (GCM) that has been recently modified to better capture near-surface winds. The authors present the model design and implementation, model evaluation, and impact on surface mass balance including discussion on thermodynamic and cloud effects due to the new model capabilities. Estimates of blowing snow show skill against observations in the test region of Adélie Land and are comparable to results from a regional climate model. Finally, the authors present results of global-scale simulations with and without blowing snow and show general climatological agreement with observations with respect to surface mass balance.

Overall, I find that the manuscript is organized and nicely written. Model assumptions are clearly articulated within the text. For the most part, I find the modeling procedure easy to follow and that the figures are of good quality. The paper focuses on describing the model and evaluation against observations, which is appropriate content for GMD. As a result, I am recommending it for publication after suggested edits.

We are grateful to the referee for the careful and thorough review of our manuscript. We sincerely appreciate all the comments, which have significantly helped us improve the study. Please find below our responses to each comment.

Specifically, I would like to see the authors expand the discussion to explicitly provide closing thoughts about some of the key motivating questions that are brought up in the paper introduction, specifically those related to whether blowing snow should be included in GCM's. These are important questions that are touched upon early in the manuscript that make this study particularly engaging to the audience, and I think it would improve the paper to touch upon them again after the results are presented. Please see more detailed comments below.

Questions and suggestions

Line 27 and Line 447: I agree, the past research and this study raises this important question. It would really help round the paper out if the authors explicitly gave an opinion of the answer to this with respect to their results and what is presented in the discussion. In my view, the statement does not have to be strongly conclusive of final in any way (considering the uncertainties that are discussed) but since the important questions is raised, it would strengthen the paper to have it addressed directly within the text.
Thank you for this comment which invites us be more explicit regarding our opinion. The overall non-negligible impact of blowing snow on the SMB and on the coastal surface energy budget, jointly with the very weak impact on the other climate variables at lower latitudes are strong arguments in favour of including blowing snow in global climate model simulations, in particular in models used for polar-oriented studies and those which are intended to be coupled with ice sheet model (e.g., [5]) . This statement should however be mitigated. First, our study has not assessed – and thus demonstrated – a possible improvement in terms of simulated radiative fluxes near the Antarctic coast and of the SMB with respect to observations, which calls for further evaluation work. Second, the inclusion of blowing snow has a non negligible additional computational cost (≈4%) which might be a limitation when running the model over very long period of time or when carrying out ensemble experiments. Our opinion is thus the following. As it does not affect the global climate properties but add some sophistication in terms of process representation on the ice sheets, we recommend the inclusion and use of blowing snow parameterizations in global models for specific runs of particular interest for polar studies. However, further evaluation

- in terms of SMB and radiative fluxes in particular - is needed to confirm that the sophistication provided by the present scheme in ICOLMDZ goes along with an improvement of the model at the Antarctic scale. The following paragraph has been added in the discussion:
The overall significant impact of blowing snow on simulated surface mass balance (SMB) and the coastal surface energy budget, combined with its very limited effect on the climate at lower latitudes, are strong arguments in favor of including blowing snow processes in global climate model simulations—particularly in models designed for polar-focused studies, such as those coupled with ice sheet models (e.g., [5]). However, this statement should be nuanced. First, our study has not demonstrated a clear improvement in simulated radiative fluxes and SMB when compared with observations, highlighting the need for further evaluation at the Antarctic scale. Second, including blowing snow introduces a small but non-negligible additional computational cost, which may become a limiting factor for long-term simulations or ensemble experiments, especially when increasing model resolution to better capture the spatial variability of precipitation over the ice sheets. Therefore, the use of blowing snow parameterizations in global climate models can be recommended for simulations specifically targeting polar processes. Nonetheless, further evaluation is required to confirm that the added complexity of the current scheme in ICOLMDZ results in an overall improvement of model performance at the Antarctic scale.

Line 33 and Line 446: It is clearly noted that transport of mass off the continent is an important part of the quantification of SMB by the model, and that including wind-blown snow could represent this discrepancy. Including some statistics about how much snow is estimated to be transported off the continent in the global runs would be very helpful for the reader to grasp if the process is significant to the GCM simulations. One suggestion is to make direct comparison with these estimates of percent change from other studies that are referenced in the paper, to offer insight into how important the process is in the GCM. (This suggestion ties in strongly with the above L27 comment).
We completely agree that adding such an information in the paper with a comparison with previous estimates in the literature would be very informative. However, properly estimating the quantity of blowing snow advected outside of the continent requires computing the flux perpendicular to the coastline at each time step. Unfortunately, our output files do not have the necessary time resolution to perform the calculation. Therefore, we would have to reconfigure the content and resolution of our output files and re-run the global simulations which are numerically costly. This reason explains what we can unfortunately not satisfy your request here.

Line 122: Does snowfall accumulated here also include snow that is deposited (sedimentation?). It is unclear from the text if the snow being deposited is feeding back to this aging estimation. Perhaps a rephrasing of this paragraph and specifying what is meant by snowfall accumulation would help with the confusion.

This is a very good point. The aging parameterisation here aims to account for the aging since the last 'fresh snow' precipitation, thus excluding blowing snow sedimentation. This is now specified in the main text.

Line 125: Is the snow age "reset" from a different value when it snows? Or is it just "set" to 0 for new snow when it snows? This wording is a bit confusing, especially since the statement above suggests that if snow falls and it happens to be eroded then the densification equation is used. But in this specific case, would the value be 0 even though it snowed but did not actually accumulate? This might just be a question of the terms used for the different ways snow can accumulate, and I suggest using precise wording and definition for each. As noted above, maybe a rephrasing of the entire paragraph would help.
Thank you for this comment. Indeed, the snow age is reset to 0 as soon as fresh snow accumulates i.e. if there is sufficient snowfall such as some fresh snow remains after the erosion process during the time step. This point is now clarified in the paragraph and the wording of the previous paragraph has been adjusted for better consistency.
'The snow age is reset to 0 as soon as some fresh snow accumulates during the time step that is, if some fresh snow corresponding to the snow that falls at the given time step remains after the erosion process.'

Lines 126-129: This last sentence is also awkward and difficult for the reader to follow.
We have reformulated as follows:
'Within each time step Δt, we do not *a priori* know the time that corresponds to the erosion of the superficial fresh surface snow - which is the snow that has fallen during the time step - and the time that corresponds to the erosion of the underlying, and thus older, snow layers. We thus assume that the fresh snow erosion occurs during a fraction ω_f of Δt that depends on the relative difference between the fresh snow erosion flux Er and the snowfall during the time step Sf: $\omega_f = e^{-(\frac{|Er-Sf|}{Sf})}$,

Line 211: Similar to the above questions, does this end up getting treated the same as precipitation in some way? What is "precipitation" consisting of? How does deposited snow feed back into the densification equation?
The sedimentation of blowing snow particles is indeed treated the same way as the sedimentation of precipitation hydrometeors such as snowflakes. Your comment make us realize we used independently 'sedimentation' and 'precipitation' of blowing snow, which confuse the reader. The text has been corrected to use the wording 'sedimentation' only. The deposited blowing feeds back into the densification equation through the first exponential term of equation 3.

Line 352: Could you add a comment on if we should expect that the parameterization significantly affects wind or temperature? Is this surprising at all?
Blowing snow is expected to have an impact on near-surface temperature through

latent effects (sublimation) and radiative effects. This is in fact what is shown and discussed in Figure 8 in the main paper. The effect on the wind speed is very weak, more subtle and indirect. The modulation of near surface temperature and stability can affect the katabatic forcing [2], and the effect of blowing snow particles on atmospheric stability can modulate the near-surface turbulence and drag, with very little impact on wind speed [1], but this effect is not taken into account in our parameterization. Overall, blowing snow has a very little effect on wind speed. Coming back to your comment on line 352 where we discuss the summertime time series, we overall expect little effect of blowing snow on temperature as the wind speed and blowing snow fluxes have a moderate magnitude. In the main text, we have modified the corresponding paragraph as follows:
The activation of the blowing snow parameterisation has overall a little effect upon simulated wind and temperature time series. In fact, the moderate blowing snow fluxes and concentrations in January are not sufficiently strong to significantly affect the air temperature and atmospheric stability - and subsequent katabatic forcing - through particles sublimation.

Lines 391-392: Figure 6e does not appear to show that there is an overestimation of flux during these months. Perhaps I am misunderstanding the comment and if so, please rephrase.
The sentence has been rephrased as follows:
The magnitude of the simulated blowing snow flux at the first model level at D17 is either close to or even exceeds the FlowCapt^{TM} measurements between 0 and 1 m (Figure 6e) and is therefore likely overestimated, concurring with the too strong simulated wind speeds at this station particularly during the extended winter.

Lines 431-432: Do you think this is associated with the spatial resolution that you needed to run with? Or do you think there is physics that is missing to capture these winds? Please add a brief comment to this effect in the text.
The simulation of the Foehn effect over the Antarctic Peninsula is very sensitive to the model resolution [4], so are the topography-induced circulations. At this stage, we cannot speculate on possible issues regarding the physics content for the representation of Antarctic Peninsula Foehn winds but we can add that the resolution we are using in the global runs is insufficient to finely capture the relief contrast and associated winds. The text has been modified as follows:
Comparison with observations (circles) reveals a reasonable agreement, except to the east of the Peninsula. This might be attributed to an excess of precipitation associated with a possible underestimated Foehn effect due to the quite coarse horizontal resolution employed in the global runs.

Line 434: Here you state that the differences should be considered negligible locally. In the same vein as my earlier questions about the significance of blowing snow and its continental-scale magnitude, I suggest the authors bring the implications of this result back up in the discussion. It seems to be an impor-

tant conclusion to this work. Is this value negligible locally but more significant continentally?
Our statement was not very clear because we wanted to emphasize that the differences are not negligible locally as their magnitude can exceed 10 kg m^{-2} yr^{-1}. Following your recommendation, we have added the following sentence in the Discussion section:
The difference is locally not negligible as it can exceed several tens of kg m^{-2} yr^{-1} in magnitude..

Figure 10: Caption – please specify in the caption the difference between "observed SMB values" in a) and SMB observations in b). My understanding from the text is that they are different because the grey dots in a) are the points of the observations themselves and the circles in a) are those positions interpolated onto the GCM grid. This would make sense why there are many more circles in a) closer to the pole. But it seems like there still should be way more circles in a) in Thwaites and Ross area. Are the grey also showing the locations of observations that do not meet the criteria of use? If so, those observation locations should probably be removed from b). It is also unclear why there are values off the coast of Ronne when there are no grey dots in b) near those locations. Please clarify this in the text and in the caption.
Thank you for pointing this incomplete explanation. Circles in panel a show the the observed accumulation data averaged onto LMDZ grid cells and weighted by the time-length of the corresponding observation during the considered period (here 2000-2005). Grey points in panel b show the locations of all SMB observations. This aspect is now clarified in the figure's caption:
'Grey dots in panel (b) show the location of all SMB observations available in the observation dataset. Circles in panel a are the averaged values from observations within each model grid cells, the average being calculated by weighting with the observed accumulation duration. Let's recall that we discard observations covering less than 3 years and only keep observations during the 5-year simulation period.'

Figure 10 and Line 440: The plots here are a little confusing, because there are many SMB values outside of the continent. I realize that is might be where the blowing snow is depositing, but for this case in b), is precipitation outside of the ice sheet being considered for both the with and without blowing snow simulations? (I suspect maybe yes since there are negative values for b) outside of the ice sheet.) Or are the values outside of the ice sheet only AIS-sourced values (i.e. wind-blown and not atmospheric precipitation). In that case, can the values outside really be considered true SMB? I guess it is also possible that the GCM ice sheet grid extends past the black coastline boundaries drawn on the figure. Please try to revise the text and caption to be clearer about what is being shown.
Thank you for pointing this issue out. In fact, a very subtle mistake was present in our interpolation script (affecting the interpolation in longitudes leading to inconsistencies in the treatment of coastal grid points). It has been fixed. The

figures have been corrected and are now shown in the revised version of the manuscript.

Figure 11: I have a similar confusion to the above, over Fig. 11 which shows (precipitation – erosion). What is "precipitation" in this context? Presumably it is the blowing snow deposition? Most likely, clarifying the text and caption for each figure would alleviate most of the confusion.
Indeed we meant 'blowing snow deposition'. The text and caption have been modified accordingly.

Minor edits

Line 45: This statement is awkward, please rephrase. Maybe use "constraints" instead of "constrains"?
Thank you, we have rephrased as follows:
Several parameterizations of snow erosion and transport have been proposed so far. However, to our knowledge, all of them were developed for mesoscale models and often involve a level of complexity — as well as an additional computational cost, particularly due to the inclusion of extra water species — that is not always compatible with the constraints of global climate simulations.

Line 104: "in" → of
Corrected.

Line 159: "authors" → authors'
Corrected.

Lines 306-307: precipitation "is" diagnosed? "prevent" -¿ "prevents us"? In general, this sentence is awkward. Please rephrase for clarity.
The sentence has been rephrased as follows:
However, the LMDZ cloud scheme diagnoses the vertical snowfall flux at each time step but does not compute the specific content – or mass mixing ratio – of snow particles [3]. This prevents us from robustly estimating a horizontal flux of all the particle categories – including snowflakes – from model outputs.

Line 313: "follows"
Corrected.

Line 317: "measures" → measurements (?)
Corrected.

Line 323: "event" → events
Corrected.

Line 344: "month" → the month
Corrected

Line 391: "first model" → the first model
Corrected.

Line 393: "at" → during (?)
Corrected.

Line 423: Should this be "tenth of K"? A few tens of K seems very large.
Yes of course, this has been corrected.

Line 424: Antarctic
Corrected.

References

[1] H. Gallée, G. Guyomarc'h, and E. Brun. "Impact of snow drift on the Antarctic Ice Sheet surface mass balance. Possible sensitivity to snow surface properties". In: *Boundary-Layer Meteorol* 99 (2001), pp. 1–19.

[2] Y. Kodama, G. Wendler, and J. Gosink. "The effect of blowing snow on katabatic winds in Antarctica". In: *Ann Glaciol* 6 (1985), pp. 59–62.

[3] J.-B. Madeleine et al. "Improved representation of clouds in the LMDZ6A Global Climate Model". In: *J Adv Model Earth Sys* 12 (2020). DOI: 10.1029/2020MS002046.

[4] Andrew Orr et al. "Comparison of kilometre and sub-kilometre scale simulations of a foehn wind event over the Larsen C Ice Shelf, Antarctic Peninsula using the Met Office Unified Model (MetUM)". In: *Quarterly Journal of the Royal Meteorological Society* 147.739 (2021), pp. 3472–3492. DOI: 10.1002/qj.4138.

[5] Robin S. Smith et al. "Coupling the U.K. Earth System Model to Dynamic Models of the Greenland and Antarctic Ice Sheets". In: *Journal of Advances in Modeling Earth Systems* 13.10 (2021), e2021MS002520. DOI: 10.1029/2021MS002520. URL: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2021MS002520.