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

Etienne Vignon, Nicolas Chiabrando et al.

October 30, 2025

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 #3

This study aims to incorporate blowing snow physics in ICOLMDZ global climate model to improve the representation of Antarctic SMB. The manuscript is well-written and structured. The study is appropriate and fits within the scope of GMD and timely. However, the main novelty of the paper wrt 'intermediate-complexity' blowing snow parameterisation for a GCM needs more explanation, justification, and rewriting. The authors mention that increasing the grid resolution near the surface would 'unreasonably increase' the computational cost, however contrary to the approach used in typical RCMs, it appears that in the current approach the authors also run the blowing snow model at atmospheric heights (all model levels) where there would be no blowing-snow, which is also computationally not efficient. In addition, it appears there are few inconsistencies wrt to the parameterisations and observations, which need to be addressed before the manuscript is accepted for publication.

We sincerely thank the referee for their careful and constructive review of our manuscript. Please find below our responses to each comment, along with explanations of the revisions that have genuinely helped improve the paper.

The manuscript would also benefit from proof-reading. There are multiple typographical errors in very important places, which makes it a little difficult to read.

A careful proof-reading was performed and we hope no typographical errors remains in the revised version of the manuscript.

Major comments

1. Line 100: It is not clear to me what exactly author's mean by intermediate-complexity, please elaborate in comparison with other implementations in meso-scale models.
This is an important point that indeed deserves clarification in the manuscript. We use the 'intermediate complexity' terminology to emphasize that our blowing snow scheme does not rely on a sophisticated surface snow scheme that explicitly account for densification effects associated with snow erosion (such as SNOWPACK in CRYOWRF for instance). Moreover, we want to stress that we consider a relatively simple one-moment treatment for the blowing snow water species (unlike a 2-moment treatment in CRYOWRF and Méso-NH) and that one scheme does not include an additional vertical discretization of the surface layer such as that in CRYOWRF and Méso-NH). We have rewritten the parameterization introduction paragraph as follows: We therefore follow an intermediate-complexity approach in the sense that the parameterisation does not require a very sophisticated snow scheme - such as SNOWPACK for CRYOWRF for instance [6] - and does not include an additional discretization of the surface layer such as in Vionnet et al. [8]. Such as in MAR [2] and RACMO [3], a blowing snow flux is directly calculated between a fully parameterised saltation layer near the surface and the first model level at a few meters above the ground surface. However, the specific content of blowing snow particles in suspension q_b (in kg kg^{-1}) is treated as an independent water variable in the model - unlike in MAR for instance - to properly distinguish the blowing snow contribution to precipitation and radiative effects from that of typical clouds. q_b is advected by the dynamical core and vertically transported by turbulent diffusion. However, we keep a one-moment treatment for the blowing snow water species and does not consider an additional prognostic estimation of the number of blowing snow particles [8, 6].

2. Line 115: What's the justification for the use of threshold friction velocity of 0.211 m/s? In Gallee (2001) u*t0 is a variable (Eq. 3 in Gallee 2001) and subsequent implementations in RACMO and other models use this (although with some assumptions wrt snow dendricity etc). Using a constant u*t0 would influence the quantity of blowing snow in the model. Where is this number coming from, please justify.
Thank you very much for pointing this mistake out. In fact, the equation was recopied from a report in which we did a sensitivity experiment with a fixed value of u_{*t0}. In the code, this variable is not constant and does depend on the surface drag coefficient (as in Gallée, Guyomarc'h, and Brun [2] and Amory

et al. [1]):

$$u_{*t0} = \frac{\log 2.688 - \log 1.625}{0.085} C_D^{0.5} \qquad (1)$$

However; what is hidden in the $\log 1.625$ is the fact that we assume that the snow grains have fixed dendricity and sphericity values $d = s = 0.5$, such as in Amory et al. [1]. The text introducing u_{*t} has been corrected accordingly.

3. Equation 3 seems incorrect, it must be $0.08345u{*}^{1.27}$ (Pomeroy and Male 1992, Eq. 37). What exactly was implemented in the code?
Thank you for pointing this mistake which has been corrected in the manuscript. The implementation in the code was however correct.

4. Line 204: Are the idealized simulations performed within the global model run or is it an offline run? And what is the 'oscillating behaviour' being talked about here?
These simulations were realized with a toy (offline) model of Eq. 11. The wording 'oscillating behaviour' was awkward, we meant that the proposed numerical scheme is numerically stable. The corresponding sentence was rewritten in that regard, and the caption of Fig. 1 was modified to specify that the curves shown correspond to a toy model of Eq. 11.

5. Line 216: Agree, but there is no description about the particle sizes considered in the study which is a critical parameter influencing the blowing snow flux. Please include that in the revised paper.
This is indeed an important aspect, especially for blowing snow sublimation. As a first approach, we assume a mono-disperse population of blowing snow particles whose radius is constant. This is a tuning parameter that can be controlled in the namelist file and whose default value is 50 μm. Following a comment by another reviewer, we have also implemented the height-dependent blowing snow radius formulation of [5] but this option has not been fully evaluated yet. This is now better explained in the manuscript in Section 2.5.

6. Fig 3: Elevation changes due to snow deposition makes the acoustic tubes submerged and the flux might not be representative of the average flux at 1 m and 2m. Did you account for the elevation changes? For explanation, see Amory (2020) and Gadde and van de Berg (2024) Eq. 11.
Thank you for raising this important point which has also been noticed by the other referees. Indeed the lowermost FlowCapt sensor regularly gets partially buried – as illustrated in Figure **??** (see response to Referee 1 above) – and the accumulated snow height can be estimated thanks to a SR50 depth acoustic sensor. However, the SR50 was deployed in December 2012 at D17, and only few information about surface elevation is available in 2011, that is during the analysis period considered in the present study. In fact, the station the instruments are raised back manually to original heights at the beginning of each summer field campaign so the flux is likely subject to an underestimation especially in winter and spring. Unfortunately, no scaling correction can be properly applied

on D17 data. At D47, as the SR50 was operational throughout the 2011 year, we apply the same correction as in Amory et al. [1] to compute the flux vertically averaged along the wind-exposed part (h) of the sensor (of full height H): $F_{b,corrected} = F_{b,measured} \times H/h$. All the figures and tables have been modified accordingly. A new paragraph has also been added in Sect. 3.1.3 to explain the correction:

Throughout the year, the lowermost FlowCapt^{TM} gets partially buried due to snow accumulation. At D47, a SR50 acoustic depth sensor monitored the surface elevation continuously between 2010 and 2012 showing that the wind-exposed part of the $H = 1$ m high sensor was $h \approx 0.6$ m in 2011. Building from Amory et al. [1], the measured flux has therefore been scaled at each time step by H/h to obtain the particle mass flux vertically averaged over the wind-exposed part of the sensor, consistently with the sensor calibration principle which implicitly assumes integration over its full exposed height H, requiring correction when only a fraction h is exposed. At D17, the SR50 sensor was deployed in December 2012, thus after the 2011 analysis period considered here. No correction can therefore be applied for this station which likely results in a underestimation of the flux magnitude. As the D17 instruments are raised back manually to original heights at the beginning of each summer field campaign, the underestimation is likely more important during the winter and spring season but this cannot be properly quantified.

7. Figure 5: Why was this not plotted for D17? Please include the figure in revised manuscript for consistency.

A panel for D17 has been added in the figure for consistency and it is now commented in the main text along with that on D47

8. Line 375 and Figure 5: Blowing snow flux has non-zero value at lower velocities when compared to the observations, this perhaps has to do with the assumption of constant threshold friction velocity of 0.211 m/s.

Please see our answer to your second comment about the constant u_{*t0} value. The fact that our model slightly overestimates the magnitude of the blowing snow flux at D47 for low wind speeds is indeed noticeable. Those conditions occur either far from snowfall event or during weak snowfall events. This overestimation can thus be attributed to the surface snow densification parameterization and/or to the LMDZ precipitation scheme that would generate an excess of fresh snow at the surface during such events. The text has been modified accordingly: .

At low wind speed - which generally corresponds to situations far from snowfall events or corresponding to weak snowfall events - the model tends to overestimate the flux which might be attributed to a too slow surface snow densification or excessive simulated snowfall by the LMDZ precipitation scheme leading to an excess in surface fresh snow. At high wind speed values, blowing snow flux observations show a more pronounced slope.

9. Line 487-488: Is the blowing snow variable defined at all the model levels

i.e. 95 model levels for the LAM run and 60 model levels for the bigger run? This seems like an overkill. Observations and meso-scale simulations are pretty consistent that the blowing snow phenomenon is mostly a lower boundary layer phenomenon. See Palm et al. 2017, RACMO results from Gadde and van de Berg (2024) (Fig. 5b). Gadde and van de Berg (2024) use only 16 grid points for the blowing snow model, with finer grid near the surface and results show good agreement with the observations without significant computational overhead. Please add in the discussion reason for not taking the standard approach of including the physics comparing it with your approach.
We completely agree with the referee that blowing snow is first and foremost a near surface process even though ground-based and satellite radar measurements have evidenced Antarctic blowing snow layers exceeding 1000 m (e.g., [7, 4]). The additional computational cost of blowing snow has two origins: the new physical parameterizations (in the physics of the model) and the advection of a new water species (in the dynamical core). Most of the additional cost in fact comes from the advection (not shown). We indeed could limit the treatment of blowing snow processes to the first layers above ground surface (stopping the vertical loops at a given model level for instance) but the overall computational gain would be very limited. Currently, there is no possibility in our dynamical solver DYNAMICO to constrain the transport of tracers over a subdomain. This might be some prospect development work but that goes well beyond the scope of the present paper. We have added in Sect. 3.1.1:
'It is worth mentioning that the additional computational cost of blowing snow mostly comes from the advection of a new water species in the dynamics rather than the treatment of the new parameterizations (surface snow erosion, turbulent transport, sedimentation and sublimation) in the physics part of the model. In the global configuration, this additional cost is about +4 %.'

10. Add the computational cost of Blos vs No-Blos simulations.
The additional computational cost of adding blowing snow in global runs is about 4%. This is now mentioned in the main manuscript.

Minor comments

1. Line 45 : climate global run's constrains → global climate run's constraints
Corrected.

2. Line 26: (- that we will hereafter combine into the single denomination of blowing snow for convenience -) too wordy - rephrase with 'hereafter blowing snow'.
We have rephrased as follows: ''Hereafter, we will combine blowing and drifting snow into the single denomination of blowing snow for convenience.'

3. Line 113: Density terms need to be an exponent according to Gallee (2001)?? Equation number is also missing.

Equation number has been added. An exponential was indeed missing in the equation, thank you for noticing. This has been corrected.

4. Line 295: closet → closest

Corrected.

5. Line 345: resp.???

Changed to 'respectively'.

6. Line 423: tens of K? or few tenths of K?

Corrected.

7. Line 510: While you mention that the code can be downloaded freely from the LMDZ website, it seems it is really not that straightforward. I tried to have a look at the blowing snow parameterisation, but could not figure out where to download the svn version that you used. If possible, please share the code/physics modules used in an easily accessible public repository for the benefit of the readers.

Thank you for raising this difficulty. This point was also raised by the Executive Editor. We now share the version of the model used to produce the results through a zenodo repository. Please see the answer to the Executive Editor comment for more details.

References

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