

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

Etienne Vignon, Nicolas Chiabrando et al.

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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 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 #1

This study aims at developing and evaluating a parameterization of wind-driven snow transport for global climate simulations using the atmospheric general circulation model ICOLMDZ. The parameterization approach is similar to that in the regional atmospheric model MAR. However, the mass mixing ratio of blowing snow is a separate prognostic variable to distinguish blowing snow from precipitation. Additionally, a double-implicit numerical method is proposed to compute blowing snow sublimation, leading to stable and accurate results despite the large time step needed for global simulations. Using a limited-area simulation over one year, the blowing snow parameterization is compared with in-situ measurements of blowing snow (FlowCapt sensors) and standard meteorology at two Antarctic sites. The frequency and timing of blowing snow events are similarly well reproduced as in the MAR model although the modeled event frequency differs from the measured one by a factor of two in some months. A direct quantitative comparison of the horizontal mass flux of blowing snow is not possible as the measurements cover the lowest one or two meters of the atmosphere while the first grid level of the model is at a height of approximately 8 m. Nevertheless, the authors extrapolate linearly the measured mass fluxes to estimate the vertically averaged mass flux in the layer corresponding to the first model layer, at least at one site. By comparing this estimate with the model result at the first grid level, they conclude that the modeled particle mass flux

has a reasonable order of magnitude. Finally, the authors use a model set-up for global simulations and show that blowing snow clearly decreases (increases) the surface mass balance in the escarpment zone (at the coast) of East Antarctica.

We gratefully thank the Reviewer for the thorough and insightful review of our manuscript. We truly appreciated all the comments, which have substantially helped us improve the study. Please find below our detailed responses to each comment.

General comments

This paper addresses relevant questions as blowing snow is a wide-spread and frequent phenomenon in Antarctica and other snow-covered regions. The text is generally well structured. The results are well illustrated by figures and mostly discussed appropriately.

(1) As emphasized by the authors, however, a quantitative validation of the modeled intensity of blowing snow is very challenging due to the coarse vertical grid resolution. I see the following problems with the model-measurement comparison and consequently with the conclusion that the ‘[blowing-snow flux] amplitude is also fairly well reproduced’ (1.456):

(a) The horizontal mass flux of blowing snow does not decrease linearly with height. The authors mention the exponential decay in the saltation layer (1.313). In the suspension layer, the mass flux is also expected to decrease non-linearly with height as the particle concentration can be approximated by a power-law function (e.g., Gordon et al., 2009; Mann et al., 2000; Sigmund et al., 2025) and the wind speed and particle speed profiles by logarithmic functions. Therefore, the linear extrapolation used in the present study appears inappropriate. [Please see our answers to your comment 2a.](#)

(b) The authors consider the mass flux of blowing snow simulated at the first grid level as ‘a mean value over the full first model layer’ (1.309). However, as the mass flux decreases non-linearly with height, the mass flux at the first grid level (center of the grid layer) is expected to be lower than the mean value over the first grid layer. Even if the observation-based mean value over this layer was accurate and the same value was modeled at the first grid level, it would imply an overestimation of the particle mass flux and concentration in the model, which would propagate to higher grid levels through the diffusion-sedimentation equation.

[We agree with this rationale. The mass specific content of blowing snow at the first model level is by definition - such as all scalar variables of the model - the mean value within this layer. Then one can raise the following questions: Should we consider a subgrid vertical distribution of the blowing snow content \(same thing for wind speed\) for calculating i\) turbulent diffusion and sedimentation in the physics of the model ii\) the transport of blowing snow \(through mass fluxes\) in the dynamics? Such questions are not trivial at all and they can](#)

be neglected when refining sufficiently the vertical discretization of the model (not only in the physics but also in the dynamics). However properly tacking this issue would be a considerable amount of work, that goes well beyond the scope of this paper and that could be generalized to any scalar variable that is supposed to vary vertically at the subgrid scale (this is for example the subject of some literature on the treatment of turbulent diffusion at the stratocumulus-topped boundary-layer inversion). The first point would imply rethinking the full numerical resolution of the turbulent diffusion in the model [14], that considers that scalar variables are defined at the middle of the layers. For the dynamics-related aspect, this would imply re-deriving the transport equation which is a huge theoretical and numerical work that might be generalized to any scalar variable with a subgrid vertical variability (common numerical advection schemes such as those developed by Van Leer assume some horizontal variability that can be described with polynomials). Therefore, we unfortunately cannot properly address your comment as we deem it beyond the scope of the present paper and even beyond the modeling of blowing snow. However, we have modified a paragraph in the conclusions to raise this important issue: Implementing a blowing-snow parameterisation in an AGCM where the first model layer typically lies several meters above the surface, limits the ability to resolve strong vertical gradients of blowing snow properties near the ground. Refining the vertical discretisation to better capture these gradients would, however, entail a substantial increase in computational cost. Consequently, the formulation of the surface drag coefficient becomes particularly critical in such models, and accounting for subgrid-scale vertical variability in blowing-snow mass content and wind speed may be necessary to improve the representation of blowing snow transport.

(c) 1. 271 – 273: Did the measurement sites experience net snow accumulation and did the lower FlowCapt sensor get buried gradually during the course of the year?

Please see our answers to your comment 2b.

(2) To improve the model-measurement comparison and increase the confidence in the blowing snow parameterization, I have the following suggestions:

(a) Instead of extrapolating the measured mass fluxes, one could estimate model-based vertical profiles of blowing snow concentration and wind speed between the snow surface and the first grid level, using the parameterization assumptions and findings from the literature. For example, the particle concentration can be interpolated between the saltation-suspension interface and the first grid level, using a power-law function of height (e.g., Gordon et al., 2009) or a logarithmic profile as implied by the bulk flux parameterization for the vertical blowing snow flux at the lower boundary (Eq. 5). By multiplying the particle concentration and wind speed profiles, it is possible to estimate the mean particle mass flux over the layer covered by the FlowCapt sensor(s) and achieve a more consistent model-measurement comparison.

Thank you very much for this comment and comment 1a which make us re-

consider how we compare observed and modelled blowing snow fluxes and in particular how we assume the blowing snow flux to decrease with increasing height. We now have to assume an exponential decay of the flux even in the suspension layer and there is two ways of proceeding:

1. Computing a near-surface profile of the modelled blowing snow flux assuming a logarithmic wind profile and an exponential profile of blowing snow mass concentration (your suggestion);
2. Assuming an exponential decay of the flux $F(z) = F_0 e^{-z/H}$ and computing the mean flux over the first model layer – using this relation – with observations (F_0 and H being estimated from the two FlowCpts measurements à 0.5 and 1.5 m). This is equivalent to the current methodology but assuming an exponential - rather than a linear - decay of the blowing snow flux.

The first method is unfortunately not feasible in an offline way (i.e. from model outputs) because we do not have access to the particles concentration in the saltation layer at each time step. This is because surface snow density - and therefore our estimate of erosion threshold and mass concentration in the saltation layer – can change within one time step when the fresh snow is completely eroded. We therefore adopted the second method. Section 3.1.2 has been modified to account for these changes and figures 3, 5, 6, 7 and Table 1 have been updated. Note that the overall results’ interpretation and conclusions are unchanged. The paragraph describing our extrapolation method in Sect. 3.1.3 has been rephrased as follows:

‘The vertical profile of the particle mass flux follows an exponential decay in the saltation layer [8, 9] which results in an overall exponential decay of the flux with increasing height [7, 5, 13]. An exponential extrapolation of the form $F_b(z) = F_{b0} e^{-z/H_b}$ is therefore used, F_{b0} and H_b being determined with the two 2G-FlowCaptTM measurements’

(b) If the FlowCapt sensor was partially buried and changes of surface elevation were monitored, it would be best to scale the FlowCapt measurement of the partially-buried sensor to obtain the particle mass flux vertically averaged over the wind-exposed part of the sensor. The model-based estimate can be averaged over the same height range, which changes with time.

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 1 – 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. [3] 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 FlowCaptTM 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. [3], 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.

(c) Recently, Nishimura et al. (2024) published almost three months of blowing snow profile measurements at Mizuho Station, East Antarctica, which had been partly analyzed in Nishimura and Nemoto (2005). The profile was measured using four snow particle counters. As the uppermost sensor was at a height of 9.6 m, this dataset offers an excellent opportunity to evaluate more directly the modeled mass flux at the first grid level.

We thank the reviewer for pointing us to this dataset we were not aware of. Indeed, it seems extremely relevant to evaluate meteorological variables and blowing snow fluxes at heights corresponding to typical GCM first model levels. We have added the reference in the last section of the paper when acknowledging the value of data collected along meteorological masts in Antarctica.

I recommend major revisions to take these suggestions into account and address the following comments. With revisions, the study has the potential to become a valuable contribution to the research field.

Specific comments

(3) l. 41-45: It would be worth to mention the paper of Saigger et al. (2024), which describes an intermediate-complexity parameterization of blowing snow in the WRF model. Please also provide examples on how an intermediate-complexity parameterization differs from more complex ones.

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 accounts for densification effects associated with snow erosion (such

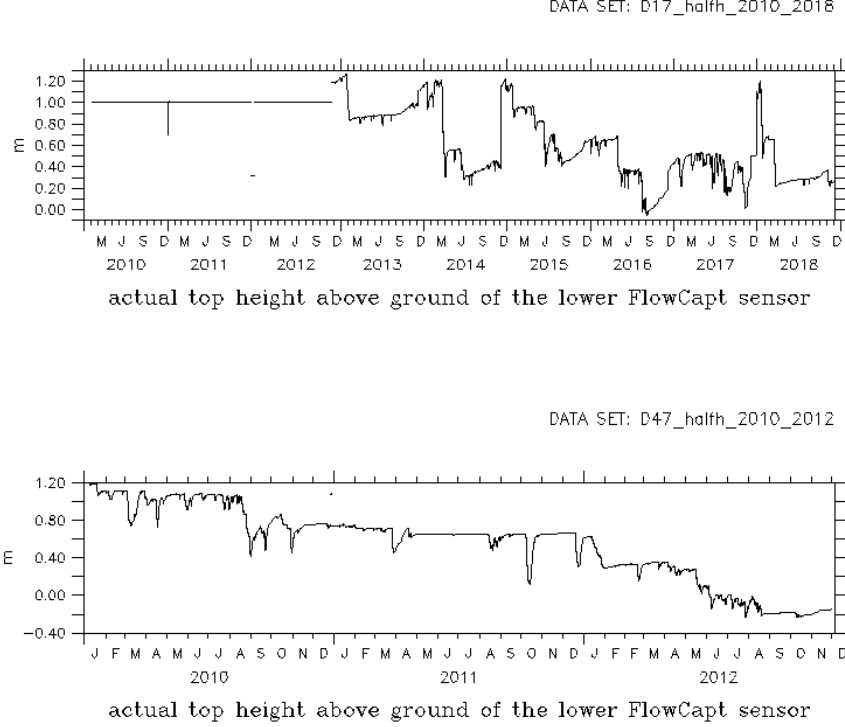


Figure 1: Time series of the top height of the lower FlowCapt sensor at D17 (top panel) and D47 (bottom panel) stations as provided by the Amory [2]’s dataset. This height is estimated thanks to a SR50 acoustic depth sensor, except in 2010, 2011 and 2012 at D17 where the reported value corresponds to the most recent visual inspection (only during the summer season) of the station.

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 modified the parameterization introduction paragraph and added a reference to Saiguer et al 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 [12] - and does not include an additional discretization of the surface layer such as in Vionnet et al. [16]. Such as in MAR [4], RACMO [6] and WRF [11], 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 [16, 12].

(4) l. 101: The vertical transport of blowing snow does not only occur through turbulent diffusion but also sedimentation. Although the individual terms of the prognostic equation for blowing snow are presented later, I propose to add the prognostic equation for q_b (combining the left-hand-sides of Eqs. 6, 8, 13 and the advection term) to better guide the reader.

This is a very good idea. We have added the full evolution equation at the end of the introduction paragraph of Sect. 2.2 (general concepts of the parameterization) and added the word ‘sedimentation’ referring to the vertical transport.

(5) l. 113: Should $(\rho_i/\rho_{s0}-\rho_i/\rho_s)$ be an exponent as in Amory et al. (2021)? Otherwise, the threshold friction velocity is not u_{*t0} but zero for new snow ($\rho_s = \rho_{s0}$).

The exponential was missing in the equation (with the density term in exponent), thank you very much for pointing this mistake. The equation has been corrected. The corresponding code was checked and was correct.

(6) l. 143: Is an exponent missing in Eq. 3 and is the associated citation of Pomeroy (1989) correct? Both is inconsistent with the corresponding description of MAR below Eq. 5 in Amory et al (2021).

The 1.27 exponent was missing in the equation, thank you for noticing. It was corrected. The corresponding code was checked and was correct.

(7) l. 179: ‘radius r_b ’: Do you assume a constant particle radius at all heights? This should be clearly stated and the value of the radius should be specified.

Thank you for pointing this shortcoming. We indeed assumed a monodisperse population of blowing snow particle with constant particle radius (so constant also with height). We deliberately do not use an empirical law for the decrease of r_b with z (such as as in Saigger et al. [11]) as this dependency is very context-dependent. Developing a new version of the parameterization with a full 2-moment treatment of a non-monodisperse particle size distribution is an avenue for improvement. In the current version of the parameterization, r_b is thus tuning parameter, whose value can be changed in the namelist file and that is equal to $50 \mu\text{m}$ by default. This is now specified in the manuscript in Sect. 2.5. Following your comment we have implemented the height-dependent radius of Saigger et al. [11] as an option in our model, but testing it is left for future work.

(8) l. 180: In the cited publications, I could not find Eq. 8 but only similar

equations. When trying to derive Eq. 8, I arrived at a slightly different equation. In contrast to Rutledge and Hobbs (1983), the ventilation factor seems to be missing; or is it included in the terms A' and B' ? Where does π in the denominator come from? I assume that the final units should be $\text{kg kg}^{-1} \text{s}^{-1}$; is the air density in the numerator needed?

Thank you very much for the very careful proofreading of the equations. Indeed some mistakes were present (ρ and π should not be there) due to an insufficient care taken when re-copying the equations in the manuscript. The equations have been corrected in the manuscript. Note that the code was based on the correct version of the equations and is correct. Please also note that we do not consider a ventilation factor here due to the relative small fall velocity of blowing snow particles (as in Muench and Lohmann [10] for ice crystals) and because the fall velocity (upon which the ventilation factor is based) is a tuning parameter that we want to control the sedimentation process only.

(9) l. 197: When rearranging Eq. 10 to obtain Eq. 11, did you forget the fraction $6\rho/(\rho_b \pi r_b^2 (A' + B'))$?

Thank you very much for noticing this mistake. It has been corrected. The corresponding code has been verified and it is correct.

(10) l. 207 – 209: How does the time scale τ_m affect the simulation? As this time scale is 10 min or lower and the typical time step is 15 min, the blowing snow particles melt and evaporate within one time step in the considered situation. Does τ_m influence the radiative effect of blowing snow in the time step? This is a good point but as you notice, the melting time scale is close to the time step value. Subsequently, blowing snow particles melt in a few time steps when they enter a layer with positive Celcius temperature. We could have chosen to make them instantaneously melt, but the continuous formulation is preferable especially for numerical aspects. No specific sensitivity test was performed to assess the effect of τ_m upon the radiative effect of blowing snow. In fact, we can reasonably expect no major impact as small blowing snow particles do not survive more than a few minutes at positive temperatures.

(11) l. 265: Can you describe the terrain surrounding the measurement sites? This might be relevant for the comparison of modeled and measured wind speeds.

Following your comments, we have added a paragraph in Section 3.1.2:

‘The topographic channelling of the gravity-driven near-surface flow gives coastal Adélie Land the most intense sustained surface winds on Earth [Parish’2006] and very frequent and intense blowing snow events [2, 15]. This region consists of a sloping snowfield with no major relief but a break in slope at nearly 210 km inland at about 2100 m a.s.l., downstream of which D47 and D17 are located.’

(12) l. 286 – 288: The sentence sounds like you use a weighted average to combine observed and modeled SMB into a final estimate. After checking Agosta et al. (2019), however, I assume that you perform a weighted average

of SMB observations that fall into the same ICOLMDZ grid cell. Please clarify. Apart from that, if an observation spans a longer period than the 5-year simulation period, do you only use the observations during the simulation period or the whole observation period?

Our sentence is misleading and it has been clarified as follows:

We then perform a weighted average – by weighting with the observed accumulation duration – of SMB observations that fall into the same ICOLMDZ grid cell, as in Agosta et al. [1]

Moreover, only observations during the simulation period have been used. This is now clarified in the manuscript in Sect. 3.1.2.

(13) l. 292: ‘simulated roughness length’: Prescribed roughness length would be a better wording if it is a constant value for snow and ice surfaces as stated in l. 70.

Following your recommendation, we have changed ‘simulated roughness length’ with ‘prescribed roughness length value in the model’.

(14) l. 367 – 369: For D47, the agreement between modeled and measured RH (Fig. 3f) is not discussed. It should at least be mentioned that lower RH values in the model compared to the measurements can be expected at D47 as the first model level is above the measurement height at this site.

This is indeed something that has to be added. Following your recommendation, the following sentence has been inserted in the main text:

At D47, part of the overall low RH bias can be explained by the difference between the height of the first model level and that of the measurement (≈ 2.2 m).

(15) l. 397: ‘the simulated frequency is more realistic in July, August and December at D47’: In July and August, this may, at least partly, be due to an overestimation of wind speed.

We partly agree. Indeed the wind speed is overestimated during the extended winter at D47, but not only in July and August. It is thus not clear to what extent this wind speed overestimation explains the better frequency during these months. We therefore prefer not modifying the text.

(16) l. 412: ‘turbulent latent heat flux’: I assume this refers only to the flux at the surface and does not include blowing snow sublimation, right?

Yes indeed, this flux only accounts for the sublimation of surface snow. This is now specified in the text.

(17) l. 423: ‘a few tens of K’ should be a few tenths of K if you mean a fraction of 1 K (?)

Yes of course, thank you for pointing this mistake out, it has been corrected.

(18) l. 432 – 434: Does the blowing snow parameterization lead to an improved agreement with the SMB measurements? Or is a direct comparison not meaningful?

This is a delicate point. In fact, the direct model-observation comparison does not show a clear improvement (see Figure 2) when including the blowing snow parameterization, with very close mean bias and RMSE (not shown). Nonetheless, almost all the SMB observations correspond to locations where the simulated SMB is not very affected by blowing snow (see Figure 10b in the main paper). The present comparison is therefore too limited to robustly conclude on the possible beneficial (or detrimental) impact of blowing snow on the Antarctic SMB in global simulation. We have added the following sentence in the manuscript:

The difference can locally reach several tens of $\text{kg m}^{-2} \text{yr}^{-1}$ but the absence of SMB measurements in the regions with the strongest changes prevents us from concluding about a possible improvement or deterioration of the local SMB modelling.

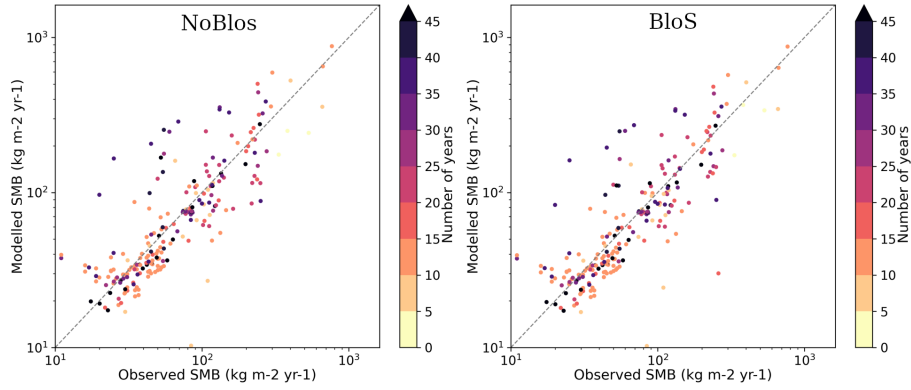


Figure 2: Scatter plot of simulated versus observed SMB during the 5-y simulation period. Left (resp. right) panel shows the simulation without (resp. with) blowing snow.

(19) l. 442: ‘no overall increase in large-scale precipitation’: Do you expect that large-scale precipitation would increase if blowing snow particles were considered as ice-nucleating particles in cloud formation?

This is a very good point and unfortunately we have no clear answer to this question. One may indeed suggest that seeding mixed-phase clouds with blowing snow crystals would enhance ice precipitation production. Unfortunately, the current version of LMDZ does not include a direct interaction between the blowing snow scheme and the cloud microphysics scheme. In other words, blowing snow crystals cannot serve as INP and do not interact with clouds such as through the Wegener-Feidensen-Bergeron and riming processes (see a thorough description of mixed-phase clouds representation in LMDZ in Raillard et al. 2025, DOI: 10.22541/essoar.175096287.71557703/v1). Such a discussion is deemed out of the scope of the present paper and we prefer not adding an additional paragraph to discuss this specific aspect.

(20) l. 456: ‘the moistening effect of the surface layer is underestimated’: This conclusion is not sufficiently discussed. You mention in l.368 that ‘the model fails to capture periods of saturation [at D17]’ but is it clear that this is due to an underestimation of blowing snow sublimation? Or could there be other reasons such as an overestimation of air temperature?

We agree, and this is a delicate point. We certainly underestimate the blowing snow sublimation but is this due to an overall underestimation of the blowing snow concentration near the surface (but we have no reliable quantitative reference) or is this more attributable to an inadequate parameterization of the sublimation (issue related to calibration or to the underlying hypothesis regarding the particle size distribution ...)? The sensitivity to the γ_{sub} was assessed and it is in fact possible to increase the near-surface relative humidity with higher γ_{sub} values but this deteriorates the statistics of blowing snow events’ detection (due to overly long residence time of blowing snow particles). We now therefore specify (in the main text) that this bias is likely due to an underestimated sublimation.

(21) l. 457: ‘During winter, wind speed, snow flux amplitude and occurrences at D47 are well simulated’: This statement contradicts l. 394 – 395: ‘the July value [of the blowing snow flux intensity] - very close to the FlowCaptTM measurements between 0 and 1 m - is likely overly strong’.

Thank you, we agree, this statement was not fully consistent with the Results section. It has been reformulated as: During winter at D47, the monthly mean wind speed is overestimated by about 1 m s^{-1} and a mean cold bias ranging between 1 and 2 K is noticeable. The snow flux occurrence fits well the observations in July and August but the amplitude is probably overestimated.

Technical corrections

(22) Typo in short summary: ‘Simulations avec evaluated using measurements in Antarctica.’

Corrected.

(23) l. 99: ‘specific content of blowing snow particles in suspension qb’: I suggest to provide units (kg kg^{-1}) or call it the mass mixing ratio to be more precise.

It is a specific content, namely a mass of blowing snow per mass of humid air, not a mixing ratio. We now specify the units kg kg^{-1} .

(24) l. 113: Equation number is missing.

Thank you, this has been corrected.

(25) Caption of Fig. 6: averaged should be averaged.

Corrected.

(26) l. 151: U should be defined in the text below Eq. 5.
We now specify ‘ U the wind speed at the first model level’

(27) Caption of Fig. 1 (4th line): T , P , and RH_i should be defined here as they have not been defined in the main text yet. A full stop is missing after ‘converge’).
The caption has been modified accordingly.

(28) l. 205: The ‘reference curve’ should also be explained in the main text, not only in the figure caption.
We now specify ‘the reference curve corresponding to the solution with a 1 s time step.’

(29) l. 208: I assume that T in Eq. 12 is air temperature but it is not defined in the text.
Thank you, ‘ T ’ is now defined in the main text.

(30) Figure 5: Black and blue dots are difficult to distinguish. Can you use different colours or show separate plots for model and measurement results?
Figure 5 has been modified using different colors and symbols to make it clearer and facilitate the distinction between model and measurements results.

(31) l. 397: ‘despite but underestimated’: Please check meaning and grammar.
Thank you. The end of the sentence now reads ‘while the simulated frequency is more realistic in July, August and December at D47 but is underestimated the rest of the year.’

(32) Caption of Fig. 8: ‘Net surface radiative flux’ should be net shortwave surface radiative flux. The dash-dotted line in panel e should be explained. Please mention the sign convention for the surface turbulent fluxes (positive = downward?).
The caption of the figure has been corrected following the three recommendations.

(33) l. 422: a reference to Fig. 9b is missing.
The reference to Fig. 9b has been added, thank you for noticing.

(34) l. 480 – 483: Please check the grammar.
The sentence has been rephrased as follows: ‘Then, we expressed the concentration of particles in the saltation layer $q_{b,salt}$ using a formula from the saltation model of Pomeroy (1989) in which the particle mass flux in the saltation layer is assumed uniform in height. Such a model is in contradiction with the well-documented exponential decay of the particle mass flux.’

(35) l. 671: There is a typo: ‘dOI’.

Corrected.

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