We thank reviewer RC1 for making relevant and constructive comments that will help to improve our manuscript. Our responses are reported below in blue.

This paper presents recent developments of the regional climate model MAR to simulate drifting and blowing snow processes over ice sheets and a detailed evaluation of the model over Adelie Land, East Antarctica. The authors propose several modifications to the blowing snow module implemented in MAR that affect the computation of the threshold wind speed for the initiation of snow transport and the simulation of the evolution of surface snow properties during blowing snow events. A configuration of MAR at 10-km grid spacing is then used to simulate the drifting snow climate and surface mass balance in Adelie Land from 2004 to 2018. Simulations are evaluated in terms of near-surface atmospheric variables (wind speed and direction, air temperature and relative humidity), blowing snow variables (occurrence, mass flux) and surface mass balance. This evaluation reveals a good ability of MAR to reproduce the occurrence of blowing snow events and their intensity at two locations. The authors also show improved performances compared to an older version of the model, despite differences in the model configuration between the two experiments. An interesting sensitivity analysis (Sec. 6.2) highlights the importance of accounting for the evolution of the properties of surface snow during blowing snow events and its feedback on blowing snow occurrence.

The paper is well written, and the results are well described. The subject of this study is interesting for a large community studying the surface mass balance of ice sheets. Prior to publication in GMD, certain aspects of the methods and results warrant further discussions and clarifications. They are listed below as general comments and are followed by more specific and technical comments. I recommend the manuscript to be accepted subject to the revisions outlined below.

General comments

1. The main purpose of this paper is to present a new version of the drifting-snow scheme implemented in MAR. This scheme is well described in the paper and is extensively evaluated. In this context, it would be very valuable for the reader to be able to clearly assess the benefit of this new drifting snow scheme compared to the previous version. So far, the authors present in Sect. 6.1 a comparison with previous simulations made with MAR but they differ in terms of horizontal resolution and lateral boundary conditions. Would it be possible to present a clean comparison between the two drifting snow schemes? There is certainly no need to run the whole period but 2 years could certainly be selected as done in Sect 6.2. Such comparison would certainly confirm the results shown on Fig 11 and highlight the benefit of using the new drifting-snow scheme implemented in MAR.

The results presented in Sect. 6.1 and used for comparison have been produced with a former version of the model (v2, developed at the University of Grenoble) which has started diverging with the version used in this study (v3, developed at the University of Liège) more than 15 years ago. Version v2 is unfortunately not maintained anymore and the source code has even not been conserved.

As nothing had ever been published with MARv3 regarding drifting-snow applications the only option we had to highlight improvements within the drifting-snow scheme of the newest version was to rely on previously published work with MARv2 for January 2011, for which only the results for the reproduction of the corresponding paper (Amory et al., 2015) had been preserved. However, the very poor performance of MARv2 in simulating the drifting-snow mass flux as illustrated in Fig. 11 for a few events in January 2011 suggests that a comparison over a longer time period would have likely given a quite similar picture.

We agree though that a more comprehensive evaluation of MARv2 would have required working on similar periods with a similar model setup (number of vertical levels, horizontal resolution, lateral boundary conditions) and focusing on surface mass balance as well to help quantify benefits of using the new drifting-snow scheme implemented in MAR without any ambiguity. Since MARv2 has never been employed for surface mass balance applications, this comparison is however of no consequence for previously published works with that version of the model.

2. The authors explain at P6 L 152-153 that the drifting snow module in MAR uses a formulation for the particle ratio in the saltation layer taken from Pomeroy and Gray (1990) and used in Bintanja (2000a). However, in Eq. 5 of the manuscript, the author uses for the saltation efficiency the formulation

e_salt = 1/ (3.25 u_star). This formulation is not consistent with Pomeroy and Gray (1990, page 1587) and Bintanja (2000). Indeed, in these two papers, the saltation efficiency is written as e_salt = 0.68 / (c_t * u_star) = 1/ (4.2 * u_star) with c_t = 2.8. The formulation given in Eq. 5 is consistent with Pomeroy (1989) as explained in the paper describing the initial drifting snow module in MAR (Gallee et al., 2001). The author should clarify this point and if they are using the formulation given by Eq. 5, I recommend them to use the formulation of Pomeroy and Gray (1990) which can be considered as the reference for the Pomeroy saltation module.

The formulation for the mixing ratio in the saltation layer detailed in Eq 5. gives a maximal value of the mixing ratio for intermediate values of the friction velocity. This is a potential limitation of this formulation that has been identified by Bintanja (2000a). Bintanja (2000b) tested another formulation where the mass concentration in the saltation layer increases monotonically with the friction velocity (see Eq. 2 in Bintanja (2000b)). Did the author test this approach in their model and how does it compare with the flux measurements collected at D17 and D47? I recommend the authors to add in their manuscript an additional sensitivity experiment and a related discussion about the validity of this formulation and its potential limitation.
Thank you for pointing out this mistake. We have corrected the reference made in the text and the formulation of Pomeroy and Gray (1990) will be adopted in the next model version. We have verified that no significant differences in the near-surface drifting-snow mass flux occur by adjusting the formulation for qsalt (see more details below).

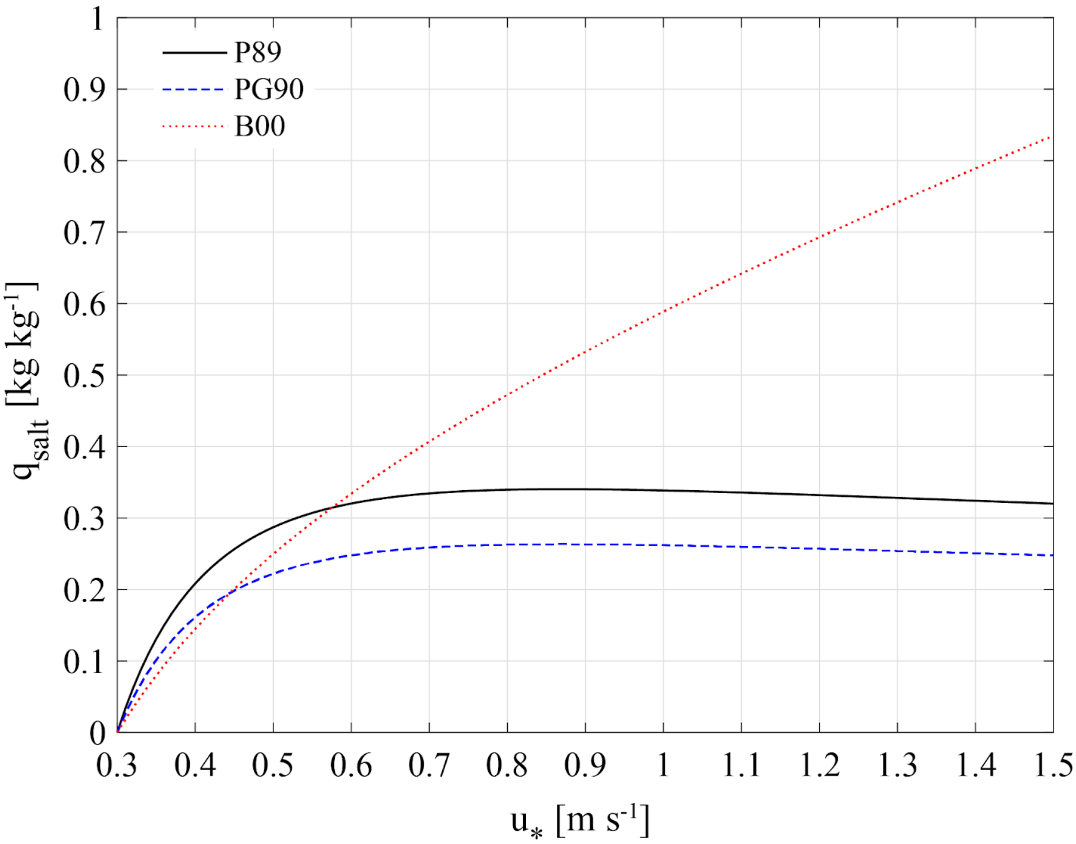

Figure R1. Evolution of qsalt as a function of u* for the parameterizations of Pomeroy (1989), Pomeroy and Gray (1990) and Bintanja (2000b), respectively indicated as P89, PG90 and B00.

Figure R1 illustrates that a significant sensitivity to the parameterization of the saltating mass flux could be expected with the alternative proposed by Bintanja (2000b), especially for u* > 0.6 m s^{-1} from which the formulation starts to diverge from the two others. However, the comparison of the snow mass fluxes resulting from the two formulations of PG90 and B00 with the control run P89 shows very small differences (< 2%) in the cumulative snow mass transport at D47 and D17 and similar drifting-snow

frequency values (see Table 4 in the revised version). This is caused by the limitation made on the erosion of only the mass available in the surface layer per time step (see our response to specific comment #6), which is strong enough to reduce the sensitivity to the formulation of qsalt.
We have summarized this finding in a paragraph that has been added to Sect. 6.2 and Fig. R1 is proposed in the supplement.

Specific comments
1. Abstract L1: I understand that the authors want to use the term "drifting snow" throughout the paper as mentioned in the first paragraph of the introduction. However, I recommend being more general in the abstract and to use the term "drifting and blowing snow" to avoid any confusion for the reader who would start by reading the abstract.
Many authors have referred to drifting and/or blowing snow for describing different processes (saltation, combined or not with suspension, wind-driven snow transport > 2 m and/or < 2 m, local erosion combined or not with horizontal advection, etc..) leading to a potential confusion of the actual meaning of this term when not properly defined.
We understand your concern about using more generic wording in the abstract. The paper has however been conceived around an explicit definition of drifting snow (as explained in the introduction). Including blowing snow in the first sentence of the abstract might possibly introduce confusion because of several instances of drifting snow declined in compound words such as drifting-snow scheme, drifting-snow physics, drifting-snow frequency, drifting-snow event etc (that would also include blowing snow in a different, more general meaning). For a matter of consistency of the abstract with the rest of the paper, rather we suggest to specify the height of particles transport we refer to: "*Drifting snow, or the wind-driven transport of cloud- and surface-originating snow particles below and above 2 m above ground […]*".

2. P 4 L 95-96: the initialization of the snow-related variables can certainly be presented in Sect. 2.2 focusing on the snowpack model. The authors should also detail what they are using as initial conditions for the variables describing the snow microstructure.
The following paragraph has been added to Sect. 2.2:
"*The snowpack was uniformly initialised with snow grain shape parameters of fresh snow (see Sect. 3.3 for definition) and a density of 500 kg m^-3 assuming a null water liquid content. The initial surface snowpack temperature is set to the reanalysis near-surface air temperature and then discretised along a predefined layer-thickness profile as a function of distance to the surface to determine the temperature of internal snowpack layers. The model was then run from 1994 so that the snowpack had reached equilibrium with the climate preceding the period of interest (2004-2018) after a spin-up time of 10 years.*".

3. P 7 L 183-184: the drifting snow module in MAR treats drifting snow particles in the atmosphere using the cloud microphysical scheme of MAR despite differences of size and shape between drifting snow particles and cloud snow particles (e.g. Nishimura and Nemoto, 2005). Could the author briefly discuss how this assumption affect the simulation of sublimation?
All the following information is now detailed in the manuscript in Sect. 3.4.:
Phase changes of atmospheric water species in MAR are resolved within each atmospheric level according to the microphysical processes described in Lin et al. (1983). In particular, sublimation (their Eq. (31), p. 1072) is calculated by assuming an exponential size distribution of suspended (cloud and eroded) snow particles (Gallée, 1995)

ns=n0exp(-lambda_s*Ds)

with ns the number of snow particles of diameter Ds per unit volume, n0 an empirical constant that corresponds to the intercept parameter of the size distribution, and lambda_s the dispersion parameter

lambda_s=(pi*rho*n0/rho_a*qs)^(¼)

where rho is the snow particle density set to 100 kg m^{-3}, rho_a is the air density and qs is the snow particle ratio (kg/kg). Snow particles are considered as graupel-like snow of hexagonal type and the spectrally-averaged snow particle diameter Ds is prescribed as a constant following Locatelli and Hobbs (1974).

Not distinguishing on the origin of particles despite differences in shape and size between cloud and eroded snow particles (Nishimura and Nemoto, 2005) can affect the estimation of sublimation according to the predominance of one type of particles over the other in the actual airborne snow mass. An overestimation of atmospheric sublimation rates within drifting-snow layers mainly consisting of eroded particles can be expected, which would be partially counterbalanced by the increased negative feedback of sublimation and all the less pronounced as the relative contribution of cloud particles prevails.
Note that, despite the prescription of a constant snow particle diameter, the seasonal cycle in air relative humidity at 2 m is well captured by the model (see Le Toumelin et al. (2020) - their Fig. 4a).

4. P 8 L 229-230: the wind-tunnel results of Sommer et al (2018) suggest that wind hardening is only found where snow has been deposited. Based on this result, I recommend the authors to reformulate their sentence to explain that their parametrization aims at representing the post-deposition increase of the grid-average density in a grid cell exposed to snow transport.
We have reformulated the first sentence as "*The post-deposition increase in snow density through wind hardening is accounted for in the model by increasing the grid-average density of the uppermost snowpack layer in each grid cell exposed to drifting snow*".

5. P 8 Sect. 3.5: the authors should detail at which step is treated the deposition flux (sedimentation) from the atmosphere to the surface. My understanding is that it happens at step 5, but I think it could be more specific.
We have reformulated the description of step 5 to improve clarity: "*The drift fraction is obtained from Eq. (8). Snow is deposited at the surface and surface density is adjusted according to Eq. (7)*".

6. P 9 L 249-250: Only the surface snow layer can be eroded in the new drifting snow implemented in MAR. It is not clear why the erosion of the underlying layer is not allowed if the surface layer is totally removed during one time step. Is this choice motivated by technical reasons to simplify the code or is there another motivation?
The main motivation is indeed to i) avoid numerical instability related to the erosion of multiple layers that hinder the surface temperature from reaching equilibrium and ii) prevent significant rearrangements of the snowpack per time step increasing the computational cost. Furthermore this choice is also related to the discretisation of the snowpack, which is not directly based on mass but on layer thickness. The maximum thickness of the surface layer is set to 2 cm and the model timestep is 60 s. With a maximum surface density (for an erodible layer) of 450 kg m^{-3}, removing the entire layer within one time step leads to an erosion rate of ~1 cm min^{-1}, which can be thus regarded as an upper bound. While no observations are currently available to assess this limit, we consider this hypothesis as acceptable since it enables a good agreement between observed and simulated snow mass fluxes.
The motivation for this choice is now better explained in the corresponding paragraph: "*A different feature of the current drifting-snow scheme of MAR contrasting with earlier versions is that, instead of being simultaneously distributed over several upper snow layers, the influence of erosion and deposition at each model time step (60 s) is restricted to the uppermost snow layer only, under the consideration that only the surface snowpack layer can exchange momentum and mass with the atmosphere. For deposition, this reduces the computational cost by preventing rearrangements of several snow layers per time step. For erosion, this avoids numerical instabilities related to the likely removal of several snow layers deeper in the snowpack while the computation of the surface temperature and energy balance is based on the surface layer only. Snow layers with different characteristics may thus be deposited or exposed successively at the top of the snowpack during a drifting-snow event, thus influencing the simulated surface albedo.*".

7. P 11 L 313: The FlowCapt sensors are sensitive to the impact of snow particles on the tube. Drifting snow particles in the saltation and suspension layers will therefore influence the value measured by the

FlowCapts. However, the mass flux in the saltation layer is ignored in Eq. 10 when computing the value of the modelled drifting snow flux that is compared with the FlowCapt value. Can the author justify their choice? Could it be a reason for the underestimation of observed near-surface drifting snow transport shown on Fig. 6?

The mass removed from the surface through erosion is expressed by the turbulent flux of surface snow particles (Eq. 6 of the manuscript) which indeed corresponds, from a theoretical perspective, to the snow mass suspended from the saltation layer. However, since we want to account for the actual airborne snow mass (including erosion) to calculate the snow mass flux that is actually simulated by the model, saltation needs to be excluded from the calculation as it is not explicitly resolved by MAR and only serves as a lower boundary condition for the suspension layer. This numerical approach could be responsible for a misrepresentation of the observed near-surface drifting-snow transport as shown on Fig. 6, but more generally the ability of the model in representing drifting snow results from the balance between all the factors influencing the airborne snow mass, including other choices (and possibly even more influential) made in the microphysical, turbulence and surface schemes.

8. P 12 Fig. 1: how is defined surface snow density since the thickness of the surface snow layer is evolving with time?

Surface snow density corresponds to the density of the uppermost layer of the snowpack. This is now specified in the figure caption.

9. P 13 L 361: Does MAR include a parameterization for wind gusts, and could it be evaluated using the wind speed data available at D17 and/or D47?

The source code of MAR indeed includes initial developments for a parameterization of wind gusts. However as it is an off-line procedure that still requires development, we think that it may lie beyond the scope of our study, especially given that observation data are only available at the half-hourly timescale.

10. L 19 Sect 5.4: In this section, the authors evaluate the mean annual surface mass balance (Fig. 9). Did they also consider evaluating the ability of MAR to reproduce the inter-annual variability of surface mass balance and how does it compare with the ability of the model to simulate the inter-annual variability of the drifting snow transport?

Figure R2 shows the comparison between observed and modelled annual SMB for the period 2004-2018 following the same methodology described in the paper. The analysis year by year illustrates the ability of MAR to reproduce the inter-annual variability of the SMB conjointly with drifting-snow transport. (Figs 5,6 in the initial manuscript). Similar conclusions than for the analysis of the mean annual SMB can be drawn here: MAR underestimates the SMB over the first tens of kilometers of the transect by locally simulating a persistent ablation zone, and better captures the SMB gradient further inland.

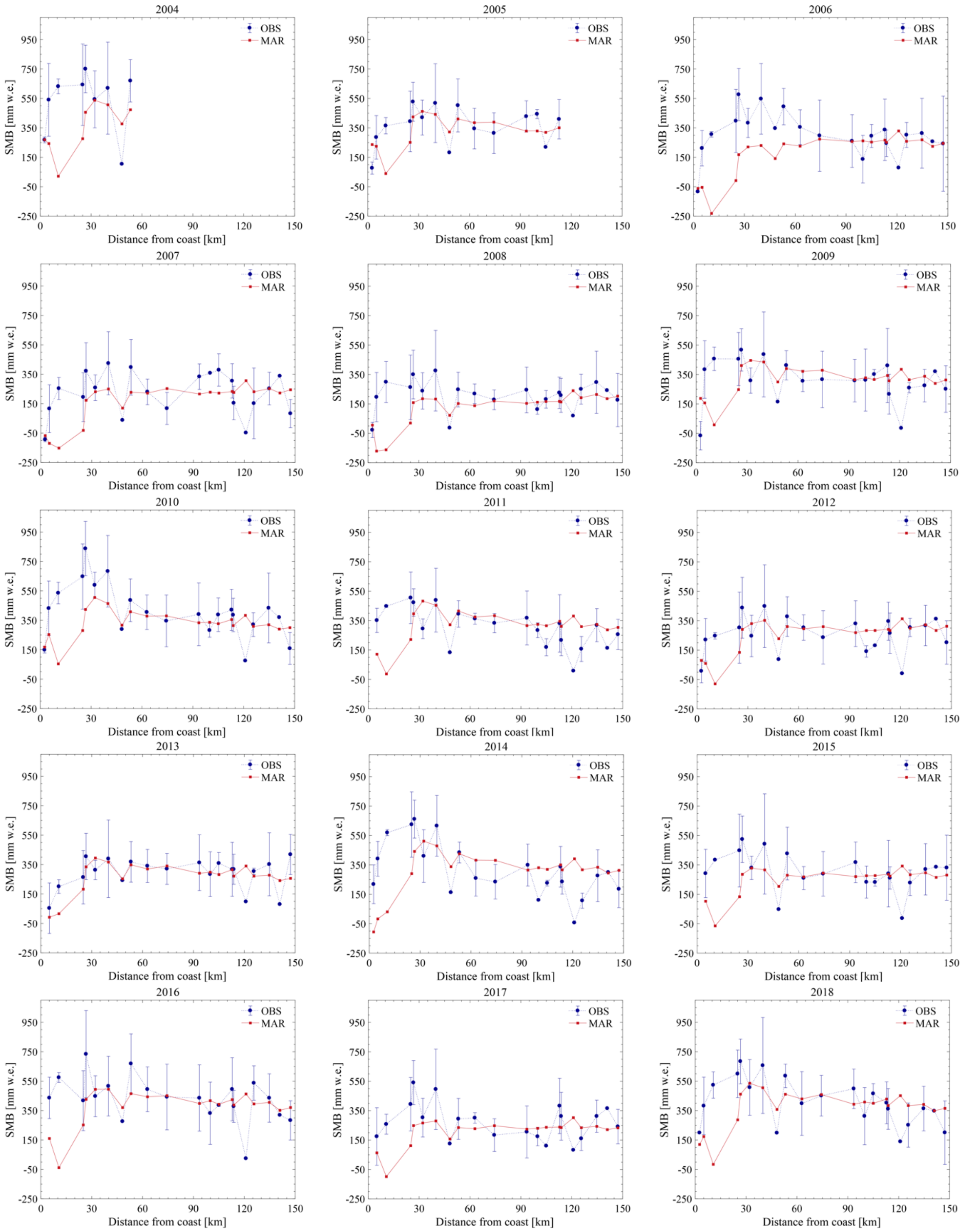

Figure R2. Simulated (squares, red solid curve) vs. observed (circles, blue dotted curve) annual SMB from 2004 to 2018 along the stake transect. Observed annual SMB values are averaged on MAR grid cells with no interpolation nor weighting. The spatial resolution is 10 km. Distance along the transect starts at the coast, and is computed as the average distance of all annual observations contained in each grid cell. The vertical dashed bars represent one spatial standard deviation of the observations.

11. L 23 L 485: the sensitivity analysis presented in Sect 6.2 illustrates well how the evolution of the properties of surface snow during drifting snow events influences the frequency of these events and the amount of snow transported during these events. The authors test three values for the density of fresh snow ranging between 250 and 350 kg m-3. These values are much larger than the typical values for fresh snow used for snow model in mountainous environments (e.g. Helfricht et al., 2018). It is clear that the polar and mountainous environments differ but it would be interesting if the authors could better justify their choice for the range of values of fresh snow considered in this study. To what extent, are these values already partially integrating the effect of fragmentation on wind-blown snowflakes (Comola et al., 2017)? And as a consequence, what is the recommendation of the authors to properly separate between a representative value for fresh snow density and a useful parametrization to handle post-depositional increase of snow density during drifting snow events?
In addition to the different environmental conditions between polar and mountainous regions that could justify prescription of different values for fresh snow density, the value of 300 kg m^-3 for the simulated fresh snow density in polar regions is frequently assumed (e.g., Lenaerts et al., 2012, Fausto et al., 2018) in the absence of a sophisticated firn compaction model. In this context, MAR is no exception and the value chosen for the density of fresh snow rho_0 in the model is a compromise between the simplified representation of the snow microstructure and firn compaction, and the role also played by rho_0 in the parameterization of the threshold friction velocity u*t (Eq. 1 and drifting-snow compaction rate Eq. 9). We could indeed prescribe a fresh snow density value at deposition that differs from the reference density value used in Eq. (1) and that would be more in line with observations or theoretical considerations, but we wanted to preserve the consistency between the two parameterizations (Eqs. 1 and 9) and keep the current definition of rho_0. This notably requires adopting values for rho_0 that are probably above typical values for fresh snow as u*t becomes a more restrictive criterion for erosion with decreasing rho_0 (Fig. 12), and consequently that already partially integrate the effect of post-deposition processes. Ideally (and here is our recommendation) different approaches of higher complexity could be envisaged, for instance, by enlarging the set of snow microstructural properties accounted for (e.g., Lehning and Fierz, 2008) and removing the dependency to rho_0 in the computation of u*t, prescribing fresh snow density as a function of atmospheric conditions (e.g., Vionnet et al., 2012), and/or including a more detailed representation of post-depositional processes including wind fragmentation, wind hardening during drifting snow, internal compaction and metamorphism. But all of this would require adaptation of the snowpack model in SISVAT and additional development experiences to properly account for and/or improve the representation of these processes. Our approach is much simpler and based on the current version of SISVAT accounting for its current level of complexity. In that sense, our choice made for the fresh snow density value is relative to the choices made in the drifting-snow model (in particular, to the current representation of rho_s, u*t as a function of rho_0 and rho_s, and drifting-snow compaction) but is undoubtedly perfectible. The sensitivity experiments on u*t illustrate this idea of contextual dependency; changing u*t for two other parameterizations found in the literature degrade the model performance. This does not mean that the two other parameterizations of u*t are less recommendable but that the control parameterization of u*t is more adapted to the current model version (including all the choices involved in parameterizing drifting snow). And most likely, so it is for the other parameterisations in their original development context.
The following sentence has been added to the text (P25 L534-536 in the revised version) when describing the sensitivity experiments on u*t : "[…] *that result from the combined sensitivities to each of the three parameterisations. This notably requires adopting values for rho_0 that are probably above typical values for fresh snow as u*t becomes a more restrictive criterion for erosion with decreasing rho_0 (Fig. 12), and consequently that already partially integrate the effect of post-deposition processes. Contrasting results between D47 and D17* […]".

12. P 26 L 518: Note that the parameterization of Vionnet et al. (2012) for the threshold wind speed has been mainly used in applications of the Crocus snowpack model in polar environments. In alpine context (e.g., Vionnet et al., 2014), only the erodibility index given by Eq 3 of this manuscript is used.
Thank you for this clarification. Note that the adaptation of the erodibility index for polar applications is also specified in Appendix B (or Appendix A of the revised version).

Technical comments

Text

Abstract L2: it is not clear why the term “mostly” is used here. Drifting snow originates from particles raised from the surface of the snowpack (https://glossary.ametsoc.org/wiki/Drifting_snow).

The term « mostly » is used here to highlight the different types of particles that can contribute to the horizontal snow mass transport. Even if the definition found in the glossary of the AMS provides a theoretical context for describing drifting snow, in a natural environment drifting snow conditions are likely to involve particles raised from the surface mixed with particles directly originating from clouds, and in practice the application of this definition outside of clear-sky conditions (i.e., drifting particles only originating from the surface) would hardly account for the process as a whole. As an illustration, drifting snow has been shown to be frequently associated with low-pressure systems and concurrent precipitation in the Alps (Vionnet et al., 2013) and in coastal East Antarctica (Gossart et al., 2017) due to the increased availability of loose snow.

Moreover, cloud and eroded particles are accounted for in the snow mass flux estimates provided by both the FowCapt sensors and the model. While no clear consensus can be found around the semantics of drifting snow in the literature, in this study we thus chose to include the transport of cloud-originating particles in our definition of drifting-snow transport to guarantee the agreement between what, in our case, is measured by the instruments and simulated by the model. We tried to leave no ambiguity on that matter in the manuscript by referring to erosion when only the contribution of the surface to the snow mass flux is discussed and to drifting snow otherwise, as well as by referring to near-surface drifting-snow transport when quantities below a height of 2 m are described.

Nevertheless, we have adapted the first sentence of the abstract (see our response to specific comment #1) and completed the definition of drifting snow in the introduction: *“[…] erosion, deposition, horizontal and vertical transport of wind-driven cloud (i.e., that have not yet reach the surface) and eroded (raised from the surface) snow particles and their concurrent sublimation are all referred to as drifting-snow processes.“.*

P3 L 63-64: are there any references that support this affirmation about the performances of the former physical parameterization of drifting snow processes in MAR?

This assertion only refers to unpublished preliminary experiments that served as a motivation for this study. We rephrased as “Unpublished *preliminary experiments with former physical parameterisations of drifting snow in MAR […]”.*

P5 L 125: I recommend the authors to mention here that the sensitivity to the formulation of the threshold wind speed for snow transport is quantified and discussed in Sect. 6.2.

This has been added to the text.

P5 L 134: do the authors mean something like “the wind speed at the lowest prognostic level of the model”? The formulation “: : : in the lowest model : : :” is not clear.

This was a typo, thank you. We have reformulated as suggested.

P 12 L 343: replace m s-1 by m s$ˆ{-1}$.

Corrected.

Figure

Figure 2: A density plot could be used to make the plots easier to read to the large numbers of points.

Thank you for this ingenious alternative. Figure 2 has been replaced with density scatter plots:

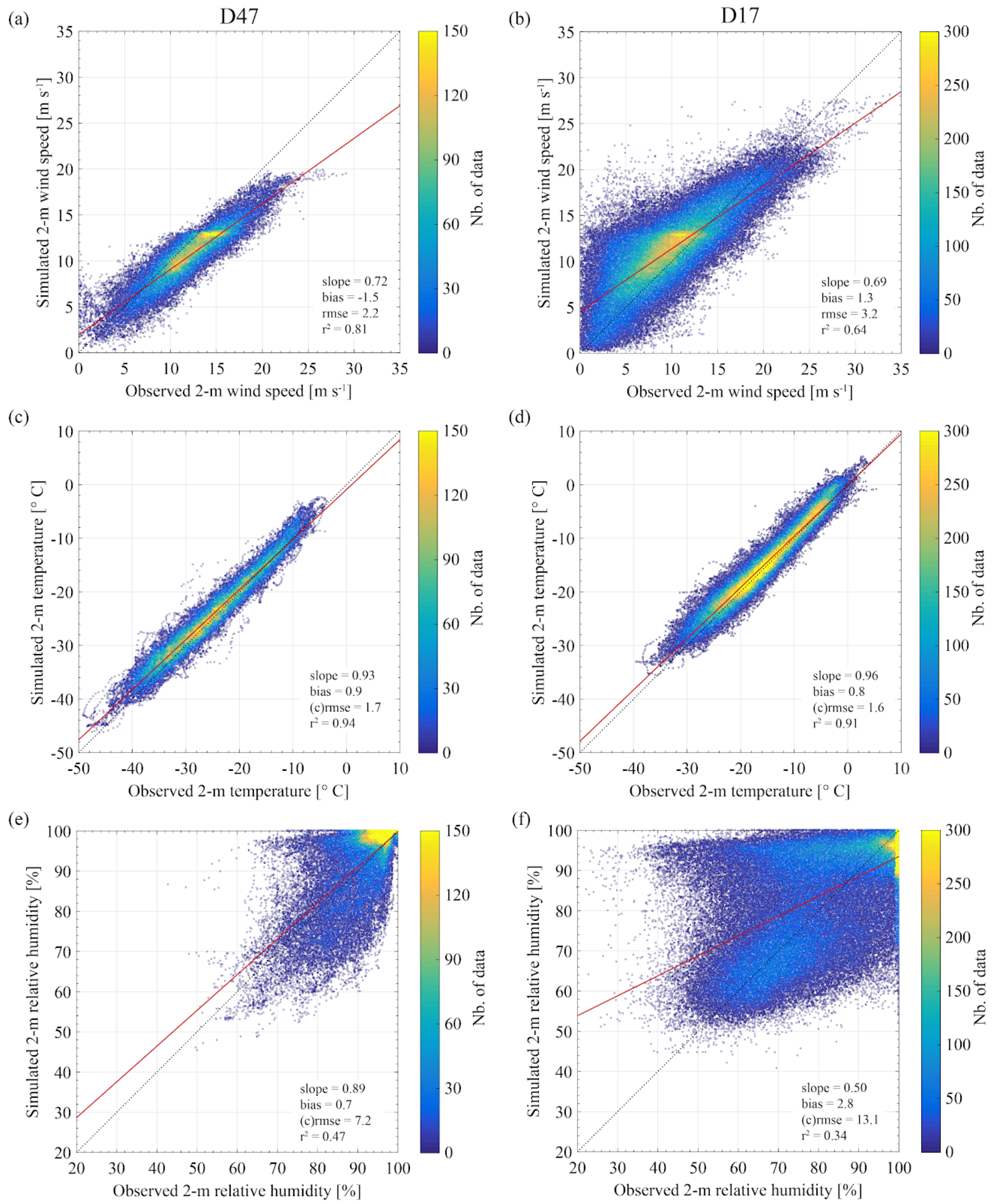

Figure R3. Density scatter plots of observed vs. simulated half-hourly wind speed (top), air temperature (middle) and air relative humidity with respect to ice (bottom) at 2 m height for stations D47 (left) and D17 (right). The coloured lines show the 1:1 line (dashed black) and the best linear fit (red).

Figure 6: it is not easy to make the distinction between the points for D47 and D17. Maybe use two figures.

Figure 6 now shows one panel for each station:

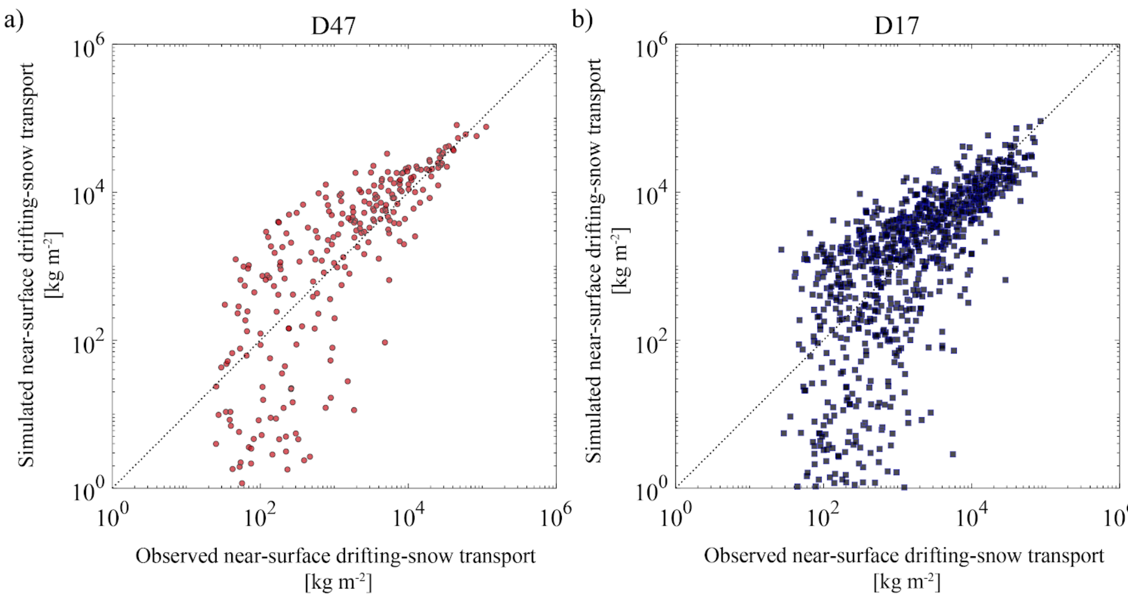

Figure R4. Scatter plots of observed vs. simulated near-surface drifting-snow transport for each drifting-snow event at D47 (red circles) and D17 (blue squares). As in Amory (2020a), a drifting-snow event is defined as a period over which the observed snow mass flux is above the detection threshold of 10^-3 kg m^-2 s^-1 for a minimum duration of 4 h.

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