We thank reviewer RC2 for making a thorough analysis and interesting suggestions from which the manuscript will undoubtedly benefit. Our responses are reported below in blue.

It was a pleasure to read and review this manuscript. The model development work presented in this manuscript is very thorough and of high quality. I think that it is well suited for GMD, since it concerns the description of the implementation of drifting snow physics in a widely used atmospheric model (MAR). It is likely that the broader scientific community benefits from the improvements made to the model. The implemented drifting snow physics capture most known processes, including sublimation and describes the compaction effect of the surface firn layer during drifting snow. The new drifting snow physics provides a very good agreement with drifting snow fluxes in terms of timing and amount. Additionally, MAR captures SMB gradients in the coastal region well. I can recommend publication after minor revisions, listed below.

Some broader comments:

- I wonder how the statistics would look like when drifting snow events are separated in precipitation and non-precipitation periods (for example based on the 100 m particle concentration, as described in L224). That could show to what extent the description of the firn layer can really accurately predict erosion and drifting snow. I can imagine that cases with precipitation from the atmosphere poses less of a challenge for the model than eroding snow from the firn layer.
The fact that MAR generates drifting snow more easily with concomitant precipitation is inherent to the model itself: since the snow particle ratio qs contains the contribution of cloud particles and qs is used to compute the drifting-snow mass flux and determine drifting-snow occurrences, necessarily a combination of precipitation and wind results in drifting snow in the model. From this perspective, even by keeping the drifting-snow scheme switched off, MAR simulates (weak) transport of snow by the wind just by horizontal advection of snowfall during their residence into the atmosphere.
Indeed we could distinguish between precipitation and non-precipitation periods during drifting snow using for instance the ratio between the surface and 100 m particle concentrations, but the results would be sensitive to the threshold value used to determine a mixed drifting-snow event. By anticipating that, for a given value of the threshold ratio, we can show that drifting snow is better reproduced for mixed events, we could not assess if these events actually involve precipitation. In that case, we would need actual observations of precipitation to calibrate the threshold value and produce more robust results. Though this seems to be feasible for another locations in Antarctica where precipitation profiles are indeed available (Souverijns et al., 2018; Genthon et al., 2018), such observations are not available at D17 and D47 and we would rather keep this idea for another study.

- Another drawback is that the surface density was not validated using field observations, which may not be available, but it’s not so clear to what extent the chosen description is rather pragmatic, simply to serve the drifting snow physics or if it matches actual firn densities in the upper firn layers. If authors have such measurements available (like the ones presented in Figure S2), it could be a valuable addition (for example by adding MAR simulated surface density to Figure S2 in the supplement), but I don’t consider it essential for publication of the manuscript.
We agree that an evaluation of surface snow properties could be a lacking aspect in our evaluation exercise. Measurements of firn density in the upper firn layers are however quite limited in Antarctica, and this is all the more true for measurements in thin surface layers in Adelie Land, as would be required here. When available, density measurements are given for firn samples to 0.5 to 1 m in thickness that would not enable an evaluation of the simulated density of the surface layer bounded to a maximum of 0.02 m in thickness. To our knowledge, only the very few measurements presented in Fig. S2 would fit these requirements. These observations show surface density values around 200 kg m^-3 at the beginning of the drifting-snow episode, which are inevitably not captured by the model in which the minimum snow density value at deposition is set to 300 kg/m3 for practical purposes (see our response to comment #11 by RC1)..
In that sense, the chosen description of drifting-snow compaction does not necessarily enable a correspondence with actual snow surface densities, but rather merely serves the drifting-snow physics

to ensure a realistic time evolution of surface snow density and capture the associated feedback for snow erosion. The discussion on the pragmatic nature of this parameterisation has been included in the text: "*By fixing rho_0 and parameterising u*t as an increasing function of rho_s (Eq. 1), Eq. (11) does not necessarily enable a correspondence with actual snow surface densities, but rather merely ensures a realistic time evolution of surface snow density. It also prevents large (positive) values of the difference u*- u*t to endure through time and thus acts as a negative feedback for snow erosion.*".

- It's demonstrated that the drifting snow is simulated more accurately, but the good agreement for SMB is not compared to earlier model versions. It could be an addition to Fig. 9 to show results from previous versions of MAR.

Excepted the results at D17 shown in Section 6.1, previous results with MARv2 are unfortunately not available anymore (see our response to general comment #1 of reviewer RC1) and have moreover never involved SMB products.

Specific comments:

- Abstract L12/13: I would consider the statement that the MAR drifting-snow physics can serve as a basis for other models maybe a bit prematurely, since I think it would be important that the surface firn properties are validated (particularly density) against ground-truth.

We have removed this sentence from the text and the abstract.

- Introduction:

some statements could be a bit expanded upon and made more concrete:

L20 - Maybe add a quantitative amount of sublimation found by the cited studies

This part of the introduction rather discusses the definition of atmospheric sublimation as an independent SMB term or not, without quantifying it. Moreover, the cited studies do not quantify it, they only describe it in terms of SMB from a different angle than the one considered here. We are currently working on the quantification of atmospheric sublimation in a fully dedicated forthcoming paper, which will surely be a much more appropriate context for such specifications.

L43/44 - "Arbitrary adjustments of model parameters favouring one can be made at the expense of the other (e.g., van Wessem et al., 2018)" I suggest to briefly summarize what they found.

van Wessem et al. (2018) evaluate the performance of RACMO2.3p2 (new version) compared to a former model version (RACMO2.3p1) in representing SMB and drifting-snow observations. The authors show that an improved representation of the SMB is obtained with the new model version by notably halving some saltation coefficient, efficiently halving the modelled snow mass transport vertically integrated over the whole drifting-snow layer, and reducing the agreement (from a positive to a negative bias) with observations when compared to the observed mass transport integrated over the first 2 meters above ground (see their Fig. 10).

However, we wish to keep this paragraph concise with an equivalent level of details for each reference in order to not lose the main focus of the paragraph, which is to comment on the linkages between SMB and drifting snow in a general modelling context. So we would rather keep the paragraph in its current version.

L55/56 - "from their numerous interactions with the atmosphere and the snow surface organized in a complex system of positive and negative feedback mechanisms" Please expand what the cited studies found in this regard

These studies were initially cited here as they both contain a description of part of the feedback mechanisms mentioned here (i.e., negative snow density feedback, surface roughness feedback, positive/negative buoyancy feedback) but do not discuss the model sensitivity to these feedbacks. A significant number of them (including those described in both publications) are accounted for in MAR and described along Sect. 3.3 in the initial version of the manuscript when appropriate. We hope to have clarified the sentence by rewriting: "*Numerical challenges associated with modelling drifting snow at the regional scale also arise from the numerous interactions of drifting-snow particles with the atmosphere and the snow surface organised in a complex system of positive and negative feedback* *mechanisms. The difficulty involved in capturing the resulting strong non-linearity of drifting-snow processes depends on the representation and number of feedbacks accounted for (Gallée et al., 2013) and is mirrored through a high sensitivity of model results to parameter choices and significant discrepancies between simulated and observed snow mass fluxes (Lenaerts et al., 2014; Amory et al., 2015; van Wessem et al., 2018).*".

- Introduction L40-42: I think it is also important to realize here that the need to explicitly describe drifting snow processes also increases with the tendency towards finer meshes of the atmospheric models used to study Antarctic SMB.

Thanks for this relevant remark. We have added your comment at the end of the corresponding paragraph.

- Section 2.1: I always prefer that the reader get some information about computational efforts. A simple sentence can be sufficient, for example that running MAR over the 15years on the 80x80 grid cells took XXX CPU hours, or something similar.

We suggest the following complementary information: « *The time step is set to 60 s, for a computational cost of 72 CPU hours per year of simulation in the chosen configuration.* ».

- Section 2.1., L97: How I interpret this sentence is that one simulation was run from 1994-2004, and that the firn state in 2004 then served as a basis for all the other simulations (including the sensitivity study). Is that true? Maybe make this explicit.

The wording is indeed a bit clumsy. We have simply started the simulation 10 years before our period of interest (i.e., in 1994, or a spin-up time of 10 years) for the snowpack to reach a stable state.

We have rewritten the sentence to make it clearer: "*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*".

For the sensitivity experiments, we only re-run the model from the simulation obtained with the control setup at the beginning of each year of investigation. This is now also explicitly mentioned for clarity: "*Simulated drifting-snow frequency and transport is evaluated for each experiment at site D47 for year 2010 and D17 for year 2013, restarting from the simulation obtained with the control setup.*".

- MAR has been used before with the snow cover model CROCUS (Vionnet et al. (2012), https://gmd.copernicus.org/articles/5/773/2012/gmd-5-773-2012.pdf). Maybe section 2.2 should detail why instead of CROCUS, SISVAT was used. It's not so clear to me since apparently CROCUS is part of SISVAT, or some routines of CROCUS are used by SISVAT? Particularly, why is SISVAT more suitable than CROCUS for modeling drifting and blowing snow?

The representation of snow in SISVAT was inspired from the developments made in CROCUS in its early version (early 90's) and significantly diverged later. The model presented in Vionnet et al. (2012) contains already much more sophisticated versions of the original routines from which SISVAT has been inspired. Similarly SISVAT is the original and current surface scheme of MAR and has also evolved with it. Today CROCUS and SISVAT are two different models that have been adapted to the needs of their users, so currently SISVAT is empirically (and naturally) more suitable than CROCUS for modelling snow transport with MAR. Significant differences exist between the two snow models and relate to different application contexts involving also compromises made on the computational cost (1-D, high-resolution simulation with CROCUS or local study case with the version coupled to the atmospheric model Meso-NH against coarser resolution, continent-wide investigations with MAR over climatological periods). Implementing the actual version of CROCUS in MAR would surely be an interesting work, that would however requires significant resources,developments, adaptations (for instance dendricity/sphericity in SISVAT vs specific surface area in CROCUS for the description of snow, refreezing accounted for in SISVAT and neglected in CROCUS), tests and reflexion on the level of sophistication required to optimize the simulations and preserve plausibility together with a reasonable computation time for 50 to 100 years of continent-wide simulation at tens of kilometres resolution.

- Section 3 could benefit from a few introductory sentence of how it is structured. I was surprised for example that the section "Initiation of drifting snow" did not describe how snow was eroded from the firn layer. Maybe rename to "Threshold friction velocity for initiation of erosion". Currently, it's a bit difficult to understand the logic between the different subsections.

Section 3.1 has been renamed as suggested, a schematic sketch is now provided in the manuscript (see Fig. R1) and an introductory paragraph as been added to the text:

"*This section describes the drifting-snow physics currently implemented in MAR. Details on the computation of the threshold friction velocity for snow erosion, snow-transport modes, interactions of drifting snow with the atmosphere and the surface, and then snow erosion and surface roughness are successively provided in the following subsections. A schematic sketch (Fig. 1) provides a general overview of the drifting-snow scheme.*".

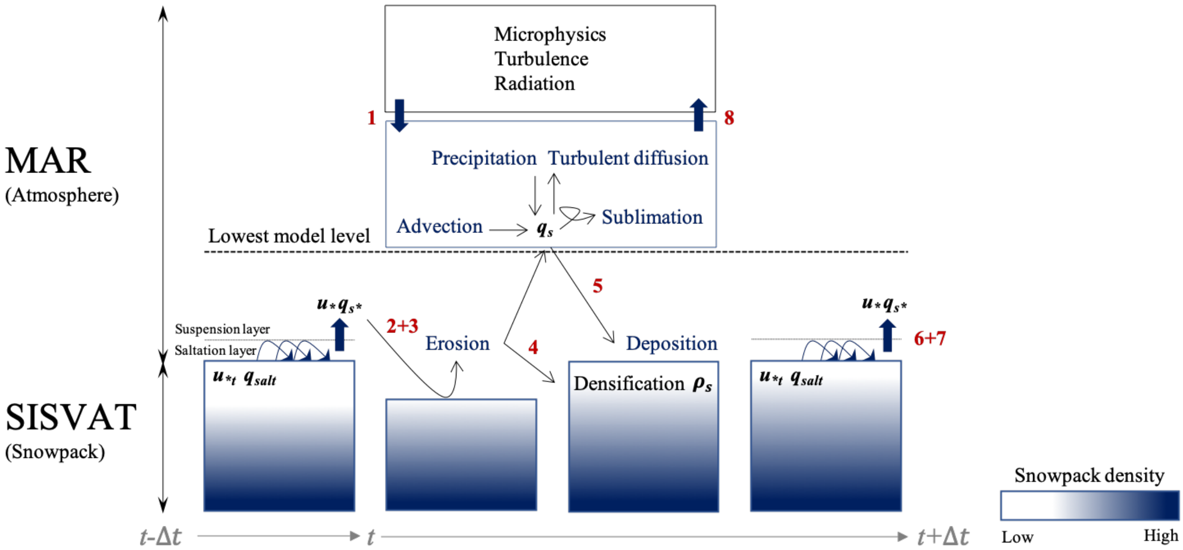

Figure R1. Schematic illustration of the drifting-snow scheme in MAR. Model variables are marked in bold black. The blue arrows denote mass and energy exchanges and drifting-snow processes are indicated in blue. The different computation steps listed in Sect. 3.5 are reported in red.

- Section 3.1: I think this section already needs to refer to Appendix B.

Instead of referring to Appendix B (which gives details about the tested parameterisations for u*t not mentioned yet at this stage of the manuscript), and also following a suggestion made by reviewer RC1, we suggest to refer at the end of the Sect. 3.1 to the sensitivity analysis provided in Sect. 6.2 where a reference to Appendix B (or Appendix A in the revised version) is made.

- I know that Eq. 1-4 have been published before (Gallee et al., 2001), but I noticed that Eq. 3 corresponds to the "fresh snow" category in Eq. 1 in Gallee et al. (2001). However, it is commonly known that the snow surface in Antarctica can consist of old snow (see for example Picard et al. (2019)). What is the rationale that here, only the fresh snow category is used?

One motivation to simplify the parameterisation of u*t was to remove the discontinuity between the two members of the former parameterisation (or ensure continuity) over the range of modelled surface snow density values by generalizing the parameterisation for the fresh snow category to all snow categories. The discontinuity in the original version of Gallée et al. (2001) was indeed identified as a cause of instability during the development phase of the new version presented here, and the density value at which the switch from one member to the other was allowed appeared as a highly sensitive tuning parameter. Another objective, as explained in the text, was to minimise the dependency of u*t on variables for which virtually no information was available so that u*t depends only on surface density (similarly to what was done in Lenaerts et al. (2012) with RACMO and Liston et al., (2007) with SnowTran-3D). This is achieved in the model by prescribing a snow density at deposition which corresponds to a pseudo fresh-snow density and already partially accounts for the influence of post-

depositional processes, taking into account the involvement of the fresh-snow density value rho_0 in the determination of u*t (see our response to comment #11 by RC1).
Although the current parameterisation of u*t corresponds to what has been conceptually described in Gallée et al. (2001) as the "fresh-snow" category, the contribution of old snow to u*t is accounted for by adjusting the surface snow density, which determines u*t, at each time step according to the proportion of drifting snow relative to fresh snow (Eq. 9 in the revised version). Surface layers mainly constituted of fresh snow are thus characterised by low density values, and thus lower u*t than less erodible layers of higher density including a higher proportion of older snow.

- Section 3.2: L178/179 discusses the upward surface flux, but I understand that this is the flux from the saltation layer (which is not explicitly treated by the dynamical core of MAR) to the suspension layer. So when I understand correctly, there are three components: the firn layer, the saltation layer (both not considered by the dynamical core), and the suspension layer, which is from the lowest model layer in MAR upward. I assume Eq. 6 then describes the flux between the saltation layer and the suspension layer. I can recommend a sketch here to better illustrate this. A schematic sketch would probably improve the readability of Section 3.
Your assumption is right. Saltation is not explicitly resolved by the model and the mass actually removed from the surface then corresponds to the upward mass exchange between the saltation and the suspension layers. This is now clearly mentioned in the text (Sect. 3.2) and a schematic sketch is provided in Fig. 1 in the revised version to better illustrate it (see Fig. R1).

- Eq. 5: It's not clear how the units are treated here. All units are declared following meter and second, yet q_salt is expressed as kg/kg. Does this mean that some conversions using density is missing from the equation?
The original formulation for qsalt (kg m^-3) is given in Pomeroy (1989) as

qsalt = (e*rho/g*hsalt) (ustar^2-ustarT^2)

in which e = 1/(3.25u*) is the saltation efficiency expressed as a dimensionless coefficient inversely proportional to the friction velocity and rho is the air density. In the model we have divided qsalt by rho for conversion from kg/m^3 to kg/kg. We have made this clearer in the revised version of the manuscript by specifying the dimensionless (or kg/kg) character of qsalt.

Section 3.3: Can the authors derive any quantification of the sublimation of drifting snow from their simulations? L191: Does the particle absorption of solar radiation increase sublimation? Can such information derived from the model simulations?
You are cordially invited to take a look at the paper submitted to TCD by Le Toumelin et al. (https://tc.copernicus.org/preprints/tc-2020-329/) in which a discussion on sublimation of drifting snow from the simulations presented here is already proposed. To prevent redundancy with that paper and digression from the main objective of the paper that would also not fit with the requirements of model evaluation papers imposed by GMD, we prefer not to focus on that topic here. Moreover, drifting-snow sublimation is the subject of an ongoing paper led by the first author from MAR simulations performed at the scale of the ice sheet, which will be a much better basis to discuss and quantify drifting-snow sublimation than the simulations presented in this paper covering only a small portion of the East Antarctic coast.
Yes, drifting-snow layers in MAR are considered as near-surface clouds and treated accordingly so they indeed contribute to the radiative atmospheric budget. Quantifying the influence of this process on sublimation could be done through sensitivity experiments, for instance by investigating the difference in cloud radiative effect within drifting-snow layers between two model runs in which the drifting-snow scheme is respectively switched on and off. See Le Toumelin et al. (2020) for more details on the radiative effects of blowing snow derived from MAR simulations.

L206-210, and L249-250: What is the rationale for restricting erosion to the surface layer only? The original approach in MAR intuitively makes more sense, where the firn can erode until the mass flux is satisfied, or the snow is too dense/bonded to be erosion.

The original approach in MAR used to work actually differently from what is suggested here by the reviewer. Instead of removing mass layers after layers until the snow mass flux is satisfied, the eroded mass (estimated from the properties of the surface layer only) was then distributed downward among the surface layer as well as all the internal snowpack layers determined as mobile from their current properties (that would have individually led to a different mass to erode if they have been considered as the surface layer) with a decreasing proportion with depth, and removed simultaneously from all these layers, though not in contact with the atmosphere (See Gallée et al., 2001 - Sect. 2.2, P5-6). We have disabled this parameterisation under the consideration that only the surface snowpack layer can exchange momentum and mass with the atmosphere (which is now specified as is in the text), and we have restricted erosion to the surface layer mainly for reasons of numerical stability and computational efficiency (see our more detailed response to comment #6 by reviewer RC1). After obtaining a good agreement between modelled and observed drifting snow mass flux with this new criterion, we have considered it as acceptable.

L254: At item 3: maybe explicitly discuss here the scenario that ER is positive (erosion) *and* the scenario that ER is negative (i.e., deposition).
ER is an erosion rate and is thus always >= 0. This is now specified at item 3.
Deposition of snow (from snowfall and/or deposition of relocated snow) is computed at step 5. To improve clarity, and also following a recommendation made by reviewer RC1, we have reformulated the description of item 5 as: "*The drift fraction is obtained from Eq. (8). Snow is deposited at the surface and surface density is adjusted according to Eq. (7).*".

L285: "further inland" is rather qualitative. Maybe add how many kilometers inland is meant here.
As the exact position of the transition from negative to positive net accumulation along the transect can vary from year to year (see Fig. R2 in our response to comment #10 by reviewer RC1), we have corrected for "*a few kilometers inland*".

L353: It's a little bit strangely formulated, since Fig. 3 only shows D17, not D47. So Fig. 3 is not really showing that the values are closer to observation than for D47.
We have moved the reference to Fig. 3 earlier in the sentence so the new sentence writes: "*The general underestimation in near-surface wind speed at D47 could be caused by the temperature-dependent parameterisation of z0, locally still yielding too high values, while at D17 Fig. 3 illustrates that modelled z0 values are closer to observations.*".

L401-402: I don't comprehend how occurrences are missed at coarser temporal resolution. I assume that the coarser temporal resolution sums the mass fluxes over the coarser time steps, such that no information is lost?
Drifting snow is assumed to occur when the snow mass flux is above 1 g/m/s2. This threshold is given valid for, and used at, a half-hourly resolution. As monthly frequency values are computed from the ratio of half-hourly drifting-snow occurrences in a month over the total number of half-hourly occurrences in that month, similar monthly frequency values could be obtained from different combinations of false negatives compensating false positives within a monthly interval (for instance by overestimating the duration of some events while other are missed).
We have reformulated the sentence which thus becomes: "*MAR shows better results (higher POD and RI) at D17 than at D47, but also simulates more unobserved occurrences (higher FAR) that compensate for missed occurrences in the calculation of monthly frequency values.*".

L380-382: When the duration of events is underestimated, one would also expect an underestimation of total mass flux in events. It seems a bit in contradiction with what is argued later (L416/417) that the main events are correctly simulated and that the underestimation stems from particularly the low wind speed events. I actually think that there is also quite some uncertainty from the simulated firn properties, as mentioned in L428/429.
We have slightly modified that part of the paper to put more emphasis on the possible influence of the misrepresentation of surface snow properties and their temporal evolution, using the comment already

made on possibly exaggerated surface compaction rate as a concrete example. Starting from L414 in the revised version, the paragraph now writes: "*Nearly consistent underestimation of drifting-snow frequency at D47 could also be caused by a misrepresentation of surface snow properties and their temporal evolution. For instance, surface compaction could be locally too strong in the model [...]*".

Fig. 9: It could be a nice addition to show the elevation or terrain slope angle along the transect here as well. It looks like that the terrain gets steeper near the coast and may also exhibit more variability. That variability probably drives SMB variability (as for example shown in Dattler et al. (2019)).
Thank you for this relevant addition. The terrain elevation along the transect is now part of Fig. 9 and the linkages between variability in SMB, erosion and slope are commented in the text: "*MAR represents the general variability in SMB with a strong increase over the first tens of kilometers from the coast and less variability further inland. The variability in i_E/D is more pronounced where the terrain is steeper near the coast and exhibits more variability in topographic surface slope, suggesting that SMB variability is driven by drifting snow. The mean SMB bias is negative [...]*".

L588: "Both parameterizations are given for rho_s = 450 kg/m^3" I don't comprehend this sentence, since the functions B1-B3 are all using a variable rho_s?
This wording was indeed confusing. We changed it for: "*Both parameterisations are given as valid for rho_s values up to 450 kg/m^3.*".

Technical corrections:
- Two comments to make the abstract better comprehensible:

L6: I suggest "drifting-snow compaction of the uppermost firn layer."
Done.

L7/8: I suggest "and a rewrite of the parameterization for the threshold friction velocity, above which snow erosion initiates".
Done.

- L15/16: I suggest "wind-driven ablation or accumulation", since that's in better line with the discussion in the first paragraph.
Done.

- Fig. 1: the red labels on purple background are very difficult to see, and definitely not easy for people with eye-sight problems / color blindness. Maybe put a white box behind the label, or improve the figure otherwise.
This is very true, thanks. We have put a white box behind the station labels to improve readability.

- L134: word missing after "lowest model".
Corrected.

- L155: even though pretty obvious, I recommend to add the value taken for gravitational acceleration.
Done.

- L239: I suggest explicitly referring to Eq. 1.
Done.

- L397: "an estimation"
Corrected.

- L475: "Improvements ... are illustrated"
Corrected.

- Fig. 7: is the horizontal axis the observed or simulated wind speed?
Good catch, thank you. It is the observed wind speed. This is now indicated in the text and in the figure caption.

- I suggest to incorporate Appendix A in the main text.
Done.

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