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Interactive comment on "Performance of MAR (v3.11) in simulating the drifting-snow climate and surface mass balance of Adelie Land, East Antarctica" *by* Charles Amory et al.

Anonymous Referee #1

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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 the previous version. So far, the author 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.

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.

Specific comments

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.

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.

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?

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 parametrisation aims at representing the post-deposition increase of the grid-average density in a grid cell exposed to snow transport.

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 understating is that it happens at step 5, but I think it could be more specific.

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?

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?

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

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

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?

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?

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 environment. In alpine context (e.g., Vionnet et al., 2014), only the erodibility index given by Eq 3 of this manuscript is used.

Technical comments

Text

Abstract L2: it is not clear why the term “mostly” is used here. Drift-

ing snow originates from particles raised from the surface of the snowpack (https://glossary.ametsoc.org/wiki/Drifting_snow).

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?

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.

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

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

Figure

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

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

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