Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-368-RC2, 2021

Geoscientific Model Development Discussions
Open Access
EGU

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 #2

Received and published: 23 February 2021

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.

Printer-friendly version

Discussion paper

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.

- 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.

- 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.

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.

- 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

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.

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.

- 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.

- 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 15 years on the 80x80 grid cells took XXX CPU hours, or something similar.

- 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.

- 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?

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

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?

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.

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?

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?

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.

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).

L285: "further inland" is rather qualitative. Maybe add how many kilometers inland is meant here.

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.

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?

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.

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)).

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?

Technical corrections:

- Two comments to make the abstract better comprehensible: L6: I suggest "drifting-snow compaction of the uppermost firn layer." L7/8: I suggest "and a rewrite of the parameterization for the threshold friction velocity, above which snow erosion initiates".

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

- 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.

- L134: word missing after "lowest model".

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

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

- L397: "an estimation"

- L475: "Improvements ... are illustrated"

- Fig. 7: is the horizontal axis the observed or simulated wind speed?

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

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-368, 2020.