Review “Dry snow initialization and densification over the Greenland and Antarctic ice sheets in the ORCHIDEE land surface model”

By Philippe Conesa et al.

Review by Michael Lehning

General:

This paper suggests refined initialization and modelling of snow in a land surface model. The contribution consists of three parts, namely 1) to parameterize an initial snow density profile to replace long spin-up runs, 2) find better models for new snow density and 3) improve the densification routine. The paper describes in detail how these steps are implemented and uses the History Matching (HM) method to calibrate the snow settling routine. An innovative step is the fact from initialization over new snow density to densification a consistent scheme is developed to match available observations in Antarctica and Greenland. A weakness is that it is not explained how other important snow parameters, notably snow temperature, are initialized along with the density. The use of the Ligtenberg firn model for snow density profile initialization is not novel in the current contribution but has already been used in a very similar way for CRYOWRF initialization, which should be acknowledged (https://doi.org/10.1029/2022JD037744). This reviewer found it difficult to understand, which observations have been used for model development and calibration and which (only) for evaluation of the overall model performance. It should be made very clear upfront if you used the same data for both or if a clean cross-validation strategy has been adopted.

A further general point is that the description of small improvements of an existing model is better suited for GMD than for TC, especially as there is less focus on new scientific results than on the model implementation.

Specific Points:

l. 8: maybe better “which may be useful other for other modelling chains”
l. 22: “has contributed 10%”
l. 36 ff: Should discuss explicitly the role of saltation in densification (https://doi.org/10.1017/jog.2017.53), which is confirmed, while the role of vapor transport is still debated (https://doi.org/10.3389/feart.2023.1167760).
l. 89 ff: This argument of excluding older data for Greenland but not for Antarctica is not convincing. First of all, your new parameterizations should be good enough to be able to handle changes due to climate and then there is growing evidence that large changes are also happening in Antarctica with more precipitation for example.
l. 105 ff: Can you explain what the purpose of grid averaging is over Greenland and why you don’t use it over Antarctica, where the grid is even larger?
l. 144: Wrongly expressed! Either “negative energy” or cold content or similar.
l. 182 ff: Can you add some detail to the explanation how the emulator construction works?
l. 207: Correct typo!

l. 232 ff: How do you justify this uniform snow depth given massively different temperatures and accumulation rates? And if I understand your introduction correctly, then you have ice below, which is hard to justify as maximum densities (ca. 600 kg m^{-3}) in your 10m column will be very different from ice still.
l. 244 ff: Also in Greenland and Antarctica, snowfalls with very low densities occur regularly and surface snow densities as low as 30 kg m^{-3} have been measured. Only that they typically don't last very long. This is why event driven accumulation such as in (https://doi.org/10.5194/tc-7-333-2013) are closer to the real process than just a wind dependent parameterization. Have you considered that?
l. 304 ff: You smooth out also "wanted" variability, for example driven by temperature differences, which has an impact on water transport etc. Why don't you add a statistical variability to the profiles?
l. 325: Cancel "to"
l. 365 ff: This result could be simply due to equifinality, as the calibrated sensitivity will compensate for different values of the reference viscosity. This should be discussed.
l. 545 ff: Certainly a negligible contribution in Greenland and Antarctica (see: https://doi.org/10.5194/egusphere-2025-3035).
l. 565 ff: Surface winds and turbulent fluxes are another factor to consider and should at least be discussed next to albedo. A quick comparison against measurements from ablation area could help to better understand.