Review “A novel numerical implementation for the surface energy budget of melting snowpacks and glaciers” by K. Fourteau, J. Brondex, F. Brun and M. Dumont

Review by M. Lehning

General:

The paper presents a review on how to numerically implement the surface energy budget into a certain class of snow and ice models. The paper is very well written and in general presents the material in a clear manner. It is overall considered to be a useful contribution to the scientific community dealing with snow and ice modelling despite its rather theoretical setting, in which conclusions on existing snow and ice models are only possible in a limited way.

In this context, it is mandatory that existing snow and ice models that have schemes that come close to the solution presented here are discussed in sufficient detail. In particular, since for example SNOWPACK uses a finite element method (FEM), for which the nodal temperature is explicitly solved at the surface, it already achieves both aspects of the paper, an explicit surface and a tight coupling with internal heat transfer merely by construction of the FEM. This is true for the original version of SNOWPACK, which is now more than 20 years old. Moreover, the statement in l.81 is not a fair representation of the current state of snow models, since also efforts have been made to implement a coupled solver in SNOWPACK that does not generate temperature overshoots. This was crucial for sea ice simulations, where an additional complexity is created by the fact that the melting point of the snow and ice is a function of salinity, and that salinity in turn is impacted by the phase changes. This means that a simple approach of allowing overshoots to occur and then setting back the temperature to fusion value is not suitable any longer. This has been presented in Wever et al. (2020) and should be discussed in the current paper. The proper acknowledgment of the state of art is necessary and as a consequence limits the novelty of the proposed approach here. It is not acceptable to say “we don’t discuss FEM models” as the authors do. This neglect is even more surprising since an overlapping group of authors proposes in another paper to use the FEM method for snow modelling (Brondex et al., 2023).

A second major point to address is the inconsistency and incompleteness with respect to the phase change (fusion) implementation as suggested. If I understand the set-up correctly, you explicitly implement the fusion process at the surface and keep the temperature solution at the phase change temperature with your variable switching formulation supported by the truncation method. But you don’t do so below the surface, which generates an inconsistency for the sub-surface heat flux. For example, for the case of shortwave penetration into snow and ice, you would generate temperatures above the melt temperature below the surface, which would lead to an upwards heat flux towards the surface, which is at the melt temperature. But heat would flow downwards in reality. This inconsistency is not even mentioned in section 6.4 and probably has consequences for energy conservation. While the tight coupling and explicit surface are sufficiently investigated with sensitivity cases in the paper, the same needs to be done for this fusion treatment. The effect needs to be quantified and compared to the more classical “overshoot” solution.

Minor comments:

1) At least I am more used to the terms “melt” temperature and “heat” capacity instead of “fusion” and “thermal”.
2) Eq. (3) does not contain heat advection by precipitation.
3) l. 108: Not true, SNOWPACK does not do a separate SEB, see above.
4) l. 126: “result” not results.
5) l. 284: “equation” not equations.
6) I don’t understand the argument here: “Note that the method used to downscale the data does not guarantee physical consistency of the variables. This allows us to take into account shortwave, longwave and turbulent energy fluxes at the top of our domain”.
7) Figures 3,4: These uncertainties should be discussed in light of typical snow/ice model errors.
8) l. 438: why “model 2” now, not clear?
9) l. 450 ff. should the reference not be a hundreds (900) of seconds consistent with typical time steps used?
10) l. 460: should it be “worse” instead of better?
11) l. 491: can you explain the deterioration?

References:

Brondex, J., Fourteau, K., Dumont, M., Hagenmuller, P., Calonne, N., Tuzet, F., and Löwe, H.: A finite-element framework to explore the numerical solution of the coupled problem of heat conduction, water vapor diffusion and settlement in dry snow (IvoriFEM v0.1.0), Geosci. Model Dev. Discuss., 2023, 1–50, https://doi.org/10.5194/gmd-2023-97, 2023.

Wever, N., Rossmann, L., Maaß, N., Leonard, K. C., Kaleschke, L., Nicolaus, M., and Lehning, M.: Version 1 of a sea ice module for the physics-based, detailed, multi-layer SNOWPACK model, Geosci. Model Dev., 13, 99–119, https://doi.org/10.5194/gmd-13-99-2020, 2020).