

Interactive comment on “NHM-SMAP: Spatially and temporally high resolution non-hydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet” by Masashi Niwano et al.

Anonymous Referee #2

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Review of

NHM-SMAP: Spatially and temporally high resolution non-hydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet

by Niwano and others

Summary

This paper introduces a new regional climate model for use over the Greenland ice sheet. The scientific impact is modest, as a) the modelled period is relatively brief, b)

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there clearly are issues that need to be addressed and c) the model data are not used for improved process understanding. But I presume the authors will at a later stage start using the model for these purposes. The technical quality of the figures is good, as are readability and length (apart from the last section, see below).

Major comments

I. 166: it is unclear what the physical basis is of a parameterization of ice albedo as a function of density. Ice has a near-constant density?

Section 2.2.3 explains how drifting snow sublimation at 2 m is calculated. But what is done with this information? Is a vertical sublimation profile assumed to calculate column blowing snow sublimation? Is the moisture source included in the atmospheric moisture conservation equation, i.e. is the additional water vapour used to moisten the boundary layer? What happens to surface sublimation when drifting snow sublimation starts? Please provide details to answer these questions.

I. 197: once drifting snow transport is calculated, the erosion can be simply obtained by taking the divergence of the transport. It is unclear why the authors claim that this is computationally too expensive? If it is not taken into account, the surface mass balance is locally not closed, this must at least be mentioned.

I. 210: "Ice sheet area minimum" suggests that ice sheet mask is not constant in time?

Section 3.2: How did the authors deal with the mismatch between SMB observation and model period?

Fig. 3: There is a systematic and considerable underestimation of LWin of up to 50 W m⁻², which should lead to too low surface temperature, yet the snow surface temperature is overestimated in the model. I cannot reconcile this?

The summary and conclusions section can be written up much more concisely: just list the main conclusions.

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Minor and textual comments

l. 167: clean firn -> clean ice

Figure 1: ice mask in Canadian Arctic looks funny.

l. 287: Why was downward longwave radiation not used from PROMICE stations?

l. 320: Why is T2m "the most important climate parameter"? Better to leave out.

l. 473: surface melt -> surface melt extent

l. 478: "were almost the same" This is not very scientific. Please quantify or leave out.
The same is true for the discussion in lines 518-520, please provide numbers.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-115>, 2017.