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The Cryosphere
Discussions

Interactive comment on "Snowdrift modelling for Vestfonna ice cap, north-eastern Svalbard" *by* T. Sauter et al.

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General comments

The study presents a new, self-developed snowdrift model and its application to a part of the Vestsvonna Ice Cap (Svalbard), forced by WRF atmospheric fields. The authors describe the model, compare observational and modeled snow depths and analyze the impact of snow redistribution and sublimation on the glacier surface mass balance.

First of all, I clearly applaud the authors for their efforts to develop a model that simulates a difficult two-phase process such as snowdrift, and making it available for public use. However, I have difficulties accepting the manuscript in its current format. Firstly, the English language requires severe revision (use of commas/sentence structure/. . .);

some issues/corrections are given as 'minor comments'. Secondly, and content-wise, I think four major issues arise, which are described below.

Major comments

1. The authors should add a (more) profound description of the model setup (numerical details, quantification of all parameters, . . .), which could be part of an appendix, but needs to be included.

2. Then, a detailed sensitivity study should be carried out, probably in a controlled parameter environment (see e.g. Xiao et al., 2000 for an example on a model intercomparison for snowdrift). Only then we can be confident that the model does show physical behavior and expected interactions between atmosphere and snowdrift. Moreover, we can see the sensitivity of the results to a change in certain parameters (e.g. surface density, threshold friction velocity, etc.), so the authors can proof their choice of a certain value of this certain parameter.

3. Is it Polar WRF that has been used? If not, why not? Since the authors have AWS measurements available, I would certainly recommend to evaluate WRF modeled near-surface wind speed, temperature and specific humidity in more detail (e.g. in a temporal perspective, seasonal cycle, extremes, etc.).

4. My last remark, which is common to one of Stephens comments, concerns the lack of important interactions, most notably the "self-limiting" feedback between snowdrift sublimation and atmospheric humidity in the snowdrift layer. Does your model allow to feedback to the WRF atmospheric fields? If not, then you should think of at least assessing its importance through slight modifications of the applied atmospheric temperature and humidity profiles.

Minor comments

P 710, L 4: but this is not included in this work! P 710, L 25: particles that are P 711, L 1: termed= referred to as/called P 711, L 2: comma after 'transport' and after ‘which’ (this is an returning error in the remainder of the text) P 711, L 4: and here no comma is needed after ‘masses’ P 711, L 9: reformulate ‘early’ P 711, L 19: affirmed = confirmed P 712, L 26: weather systems that originate in the Barentz Sea region to the east P 714, L 1: altitude-dependent P 720, L 7-8: this sentence is unreadable due to the wrong placement of commas. Please revise to something like: ‘If the saltation layer contains no particles, the corrected and uncorrected friction velocities are set equal” P 721, L 16: use ‘snowdrift’ throughout P 722, L 1: remove ‘therefrom’

Figure 2: how do the authors explain the contradictory signal (increase in the radar, decrease in snow2blow) between kilometer 8 and 10?

References

Xiao, J., Bintanja, R., Déry, S. J., Mann, G. W. and Taylor, P. A. 2000: An intercomparison among four models of blowing snow, Boundary-Layer Meteorol., 97, 109-135

Interactive comment on The Cryosphere Discuss., 7, 709, 2013.