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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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Summary: This study examines the impact of blowing and drifting snow on the 2008/2009 snow accumulation on the Vestfonna ice cap of Svalbard. To this end, the authors develop a new numerical model of snowdrifting (snow2blow) and apply it to their study site. The model is forced with Weather Research and Forecast (WRF) atmospheric fields with the horizontal resolution at 250 m. The simulations are then compared with radio-echo sounds and snow pit measurements and show that the overall patterns of snow accumulation are resolved. Atmospheric patterns lead to redistribution of snow mass from the interior of the ice cap towards the neighboring slopes and the coast. Sublimation during drifting is also found to be important during winter. The authors conclude that both snow mass redistribution and sublimation are important on

the ice cap's mass balance.

This study presents interesting results that fill a gap in our understanding of the impacts of snowdrifting on the Vestfonna ice cap in Svalbard. However, some major and minor comments need to be addressed to clarify the model development, results, and their implications. My report provides guidance on how the authors may revise their manuscript for possible resubmission to the journal.

Major Comments:

1) The language throughout the paper needs much improvement – specific issues are reported under "minor comments".

2) To provide context, it would be useful if the authors could provide (in a table) mean wintertime meteorological conditions for the two meteorological stations near the study site. Information on mean monthly air temperature, relative humidity with respect to ice, wind speed, and snow depth should be provided.

3) Why is the thermodynamic feedback of the sublimation process neglected in the equation for potential temperature (Equation 3), given sublimation is calculated in Equation 8? The blowing snow sublimation process clearly exhibits self-limiting characteristics that strongly modulate sublimation rates, with impacts to the air temperature and humidity profiles (e.g., Déry et al. 1998). Likewise, why does the snow2blow model not incorporate humidity as a prognostic quantity, with consideration of the blowing snow process?

4) Many other aspects of the snow2blow model remain nebulous. For instance, how is the heat transfer coefficient defined? Is atmospheric stability considered in the turbulent exchange coefficient? What are the values assigned to the kinematic and turbulent viscosities? What is the source of the model parameters listed in Table 2? Why is "fresh snow" given a density of 250 kg m-3? Why are blowing snow particles, given their varying spectra with height, given a constant fallout velocity of 0.02 m s-1? How

is the air density evaluated in the model? What is the source of the threshold velocity reported in Equation 10? Is undersaturation with respect to ice considered in the sublimation process (Equation 15)? How are the partial differential equations discretized and what numerical scheme is employed in the integrations? Many aspects of the model formulation are unclear or unavailable, making its evaluation nearly impossible given the content of the present paper. The authors need to expand and describe fully the development of their model and then provide a validation to demonstrate it captures episodes of blowing snow observed in the field.

5) Apart from in situ snow depth information, are observational data on snowdrift frequency available to further validate the numerical model? How well does the snow2blow model simulate the thermodynamic environment (temperature, humidity, wind profiles and distributions) and the vertical distribution of blowing snow mass?

6) Figures 2 and 3 are difficult to interpret given the standardized snow depth. Why not simply plot snow depths as provided by the model and radar measurements? In addition, there is confusion whether these are snow depth or snow water equivalent values (see caption to Figure 3). Can errors between the simulated and measured values of snow depth along each transect be provided in a table? The snow2blow model does not seem to capture properly the peaks and troughs in snow depths.

Minor Comments:

1) P. 710, Abstract: The abstract should contain the period of study.

2) P. 711, line 22: Replace “proof” with “prove”.

3) P. 711, line 26: Insert “cap” after “ice”.

4) P. 711, line 28: Insert a space in “snow-pit data”. It should also read “components”.

5) P. 712, line 9: Insert a hyphen in “marine-terminating”.

6) P. 713, line 14: Replace to read “This study. . .”

7) P. 713, line 27: It should read “0 m and 2.24 m”.

8) P. 714, line 1: Insert a hyphen in “altitude-dependent”.

9) P. 714, line 9: This should read “results”.

10) P. 715, line 6: This should read “gravitational force”?

11) P. 715, line 12: “particle” should be plural and it should read “dampen”.

12) P. 715, line 19: Rewrite as “are typically”.

13) P. 715, lines 24/25: Delete “the Stokes number” and use only “St”.

14) P. 716, line 3: “parcel” should be plural.

15) P. 717, line 5: Replace “Taken” with “Taking”.

16) P. 719, line 3: Please refer to Table 2 here when stipulating the constant value of fallout velocity.

17) P. 720, line 8: Delete the commas after “both” and “uncorrected”.

18) P. 721, line 2: Note that saturation deficits with respect to ice are often small, not large, in the Arctic. The authors need to explicitly account for undersaturation with respect to ice, not water, in estimating the sublimation loss rates.

19) P. 721, line 13: Given that blowing snow sublimation is a self-limiting, why do the authors not consider this prominent feedback in the model?

20) P. 721, line 19: It appears the second longitude should be “W”, not “E” as listed.

21) P. 721, line 20: It should read “a horizontal”.

22) P. 721, line 21: Delete “within”.

23) P. 723, line 4: Insert “km” after “30” and “10”.

24) P. 723, line 25: Insert “âŮęC” after “-37.9”.

25) P. 724, line 19: Insert “%” after “15”.

26) P. 724, line 20: Insert a comma in “drifting snow,”

27) P. 725, line 4: Rewrite as: distribution, it remains uncertain whether. . .”

28) P. 725, line 8: Revise to “at wind-exposed regions”.

29) P. 725, line 11: “Pattern” should be plural.

30) P. 725, line 13: The last part of this sentence needs to be reworded.

31) P. 725, line 18: Please use the appropriate superscripts for units.

32) P. 732, Table 1: In which order are these variables presented? Replace to read “gravitational acceleration”. All parameter values and constants should also be provided here.

33) P. 733, Table 2: What is the source of these values?

34) P. 734, Figure 1: The caption should read “approximate edge. . .”. What do the labeled contours on the map denote?

New Reference:

Déry, S. J., Taylor, P. A and Xiao, J. 1998: The thermodynamic effects of sublimating, blowing snow in the atmopheric boundary layer, Boundary-Layer Meteorol., 89(2), 251-283.

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