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

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PREAMBLE: We very much thank S. Déry and J. Lenaerts for their thorough analysis of our article and for their valuable comments, annotations and suggested improvements. They had been carefully considered and most of them are accounted for in the revised manuscript. Comments and corrections regarding the writing of the manuscript are also widely accepted. Answers and explanations to all detailed questions and annotations raised by the reviewers are provided in the following.

ANSWERS TO COMMENTS BY S. DERY

RC2: 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.

AC: A table providing the mean meteorological conditions (temperature and relative humidity with respect to ice) during the simulation period has now been provided for both sites (Table 4). Unfortunately, there are no snow depth measurements available at the stations for this period.

RC3: 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?

AC: We agree with the reviewer, that the self-limiting characteristics is an important issue for detailed sublimation studies. However, a detailed description of the sublimation process is not within the scope of this study, so that we used a simplified description of the sublimation process. The decision to neglect the feedback mechanism is based on the following reasons; (i) The air mass closed to the surface is often saturated due to the maritime climate (see Table 4 and Section 2), so that local sublimation rates are generally low. In this regard there are often stratus formation and foggy conditions near the summit. (ii) As shown by Déry et al. (1998) and Xiao et al. (2000), air temperature near the surface decreases with sublimation rate, fetch and time. Assuming a homogeneous and fully developed turbulent wind field the cooling effect can be up to 0.55 K within a horizontal length scale of 10 km. If we assume a predominantly flow direction from north-east, the distance from the lateral boundary *to the ice fringe is about 5-15 km. The limited domain (limited by the computational cost), therefore, requires a 'perfect' boundary condition such as vertical profiles of temperature, humidity and snow drift flux in order to obtain reliable sublimation estimates. This, however, is not given. The uncertainty in the lateral boundary conditions is probably of the same order as the effect of the feedback mechanism. Since the total effect remains unclear we noted, that "Neglecting the feedback mechanism on the atmospheric profiles can therefore lead to an overestimation of snow drift sublimation" (see 5.4 Sublimation). Despite the overestimation the mean sublimation rates only vary between 1-2% (see Section 8.2). Going hand in hand with the decision to neglect the self-limiting characteristics there is no need to include the humidity transport equation in the model. However, it is planned to include the self-limiting nature of sublimation as well as spectral particle size distribution in future model versions.*

RC4: 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.

AC:
(i) The heat transfer is a function of the laminar and turbulent Prandtl number. The equation is now given in Equation 4. The corresponding Prandtl numbers have been included in Table 3.

(ii) Turbulence exchange is based on eddy-viscosity, which is derived from the turbulent kinetic energy equation (see Eq. 5) and the equation for dissipation (see Eq. 8). The equation for the turbulent kinetic energy contains a buoyancy production term, which serves as a source or sink term depending on the thermal stratification (Eq. 7). Similar, the equation for dissipation is extended by a buoyancy term (Eq. 9). This term characterizes how fast turbulence decays depending on the stratification. The numerical treatment of the wall-functions is not discussed in detail, since this is not an major issue of this work. The influence of snow particles on the turbulent kinetic energy and dissipation rate is not included in the model.

(iii) The value assigned to the kinematic (1.73e-5 m^2 s^{-1}) is included in Table 2. According to equation 8 the turbulent viscosity is derived from the turbulent kinetic energy and dissipation and is therefore a property of the flow (and is not predetermined).

(vi) References have been included in Table 3.

(v) Values given in Table 3 for the density of snow (250 kg m^{-3}) and constant fallout velocity (0.02 m s^{-1}) are values from 'old' model runs. The table has now been updated. The proposed model considers the mean particle radius rather than a varying spectra with height. This is a simplified model assumption just like the constant fallout velocity. This assumption of a constant fallout velocity has been adopted from Naaim et al. (1998) and Beyers et al. (2004).

(vi) The flow is assumed to be incompressible and density is derived from the kinematic and dynamic viscosity. However, density variations due to temperature changes are derived from the Boussinesq assumption (see last term in equation 1). The model on the other side does not account for the total density of the air-snow mixture and

treats snow as a passive tracer. Assuming cold air having a density of 1.32 kg/m^{-3} (1000 hPa) and a particle density of 0.2 kg/m^{-3} in the lower surface layer, the resulting mass flux error will be less than 7%. The evaluation of density in the model is now described in Section 5.2. For the sake of simplicity we decided, for now, to neglect density changes due to the air-snow mixture.

(vii) The reference Walter et al. (2004) of Eq. 10 was added.

(viii) A detailed description of the discretization and numerical schemes would go beyond the scope of this paper and the interested reader can contact the official OpenFOAM web page. However, we have included a new Section (5.5) with a brief discussion on the discretization schemes and PISO algorithm, so that the reader can understand the principle approach.

(ix) The section on sublimation (Section 5.4) has been completely revised and the development fully described. Unfortunately, we do not have field observation of blowing and drifting snow. However, in order to show the general model behavior and the comparison with other models the snow2blow has been integrated with an ideal case similar (described in the new Section 6) to the one described in Jingbing Xiao et al (2000). In this context we have also added three more figures (fig. 1, fig. 2 and fig. 3) which show vertical profiles of blowing snow drift density, turbulent viscosity and local sublimation rate. We are convinced that the ideal case study provides sufficient validation and proves that the modeling results are comparable to other models.

RC5: 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?

AC: Observational data on snowdrift frequency and thermodynamic environment

(vertical profiles) are not available. For comparison the model has been applied to the ideal test case (see previous comment). However, it is planned to compare model results with highly resolved Laser Scan data in the near future.

RC6: 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.

AC: The figures 2 and 3 are standardized because we are rather interested to reconstruct the spatial distribution of the snow cover and not the absolute amount. For this reason we prefer to keep the figures as they are. The absolute amount strongly depends on the WRF input data and the choice of snow density. Indeed, the profiles are derived from the snow depth data and not from the SWE. However, this does not affect normalized plots, since we assume a constant snow density. We agree, that the caption was confusing and changed it accordingly. Standard deviation and mean values of the measurements, the snow2blow model and the WRF are now given in Table 5. We are aware that the results are not perfect. The issue now is discussed in more details in Sections 8 and 9. In particular along the slopes the model does not erode enough snow. Therefore, in this region the model result show an overestimation.

ANSWERS TO COMMENTS BY J. LENAERTS

RC7: 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.

AC: We have included a more detailed description of the snow2blow model and discussed the numerical issues in Section 5.5 "Discretization". In particular Section 5.2, 5.4 and 5.5 have been updated and widely extended. There are three new Sections on discretization (5.5), an ideal case study (6.1) and the evaluation of the case study (6.2). Relevant parameters are now listed in Table 3 including references.

RC8: 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.

AC: We agree with the reviewer that the validation of the model is an important issue. As suggested, we set up an ideal case study similar to Xiao et al. (2000) using the same initial and boundary conditions. The vertical profiles of blowing snow drift density, turbulent viscosity and local sublimation rate are presented in Figure 1, 2 and 3. We belief the ideal case proves the correctness of the snow2blow model and also the choice of the parameters presented in Table 3.

RC9: 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.).

AC: The atmospheric fields have been modeled with Polar WRF (as mentioned in Section 7.1). One of the co-authors is about to submit an article on the detailed validation of the data set in the next weeks. Therefore, we only briefly evaluated and discussed the WRF runs in Section7.2. Monthly mean values of relative humidity with respect to ice and temperature are now provided in Table 2.

RC10: 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.

AC: Please see the comment in RC3 on the self-limiting feedback mechanism for a more detailed answer. The snow2blow model is simply run offline and feedbacks to the WRF atmospheric fields are not possible. The error introduced by the missing feedback mechanism is believed to be small due to the low mean sublimation rates (1-2%). We are aware that we have to include such effects in the next model version.

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