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Interactive comment on "Meteorological, elevation, and slope effects on surface hoar formation" *by* S. Horton et al.

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We thank the reviewer for these helpful and constructive comments that significantly improved our manuscript. The reviewer's comments are in italics, followed by our response.

General comments

The authors of this work present research attempting to link weather, observation, and simulations for surface hoar formation. This is a very important endeavor. In general, I believe the methods to be poorly detailed and the results somewhat misleading. The authors collected a large set of field data as well as examined extensive weather data.

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Interactive Discussion
Discussion Paper

The methods presented in Section 2 are not detailed enough to reproduce the work. More importantly, the authors present the work as a sensitivity analysis, which given the information provided is not an accurate statement.

Additionally, there are various statements in the work that elude to the importance of influence of parameters, namely, the papers major finding: "moisture content of the air appears to have a larger impact" (1864:8). However, no mention to how this parameter was deemed important, this type of statement must be backed up by a quantitative rigorous statistical methodology.

We agree that we misleadingly presented our weather data analysis as a sensitivity analysis. Our intent was to show how weather conditions affected when and where surface hoar formed. We feel this was important to address when mapping layers with gridded weather data. However, sensitivity analysis should measure the change in output for a given change in input. Our compilation of weather data did not systematically change the inputs, making it difficult to measure model sensitivity (e.g. sensitivity coefficients). Slaughter (2010, p. 176) demonstrate a suitable method to measure the sensitivity of modelled vapour fluxes.

To reflect the limits of our analysis, we have chosen to change our interpretation throughout the manuscript. We now present weather conditions associated with surface hoar formation, rather than stating formation was sensitive to specific inputs. Fig. 2 has been changed to boxplots to indicate that direct weather data was used, as opposed to our previous figure which may have implied the inputs were systematically changed. We removed statements suggesting "moisture content of the air" was the most important input, and instead report the range of values for each input that were favourable for surface hoar growth or shrinkage. Since there is a high level of interaction between the inputs, statements about relative importance would require detailed quantitative analysis. Preliminary work not included in this manuscript suggest our data set would produce similar sensitivity results to Slaughter (2010, p. 199). Our revised interpretation still provides sufficient evidence to argue that meteorological conditions caused certain regions and elevation bands to have larger/smaller modelled surface hoar.

We have also expanded our methods by adding a sub-section to clearly explain the analysis of weather data (Sect. 2.4 Analysis of meteorological data) and added details throughout the methods section to make our work reproducible.

Specific/technical comments

1. *1858:25 Vapor does not sublimate. Sublimation = solid to vapor, deposition = vapor to solid, evaporation = liquid to vapor, condensation = vapor to solid*

   Changed as suggested.

2. *1859:4 Slaughter also preformed field studies of surface hoar: http://www.ingentaconnect.com/content/igsoc/jog/2011/00000057/00000203/art00006*

   We now cite this study in several relevant sections. The weather associated with modelled surface hoar formation in our study (Sect. 3.1 / Fig. 2) generally agrees with the weather conditions reported in this field study (e.g. Fig. 10 in Slaughter (2011)).

3. *1860: Section 2.1 Were the specific locations, aspects, sky view, etc. recorded for each site? If so, this should at least be mentioned. However, it may be appropriate to build histograms of the data so the reader can understand the distribution of the observations sites. For example, slope angles ranged from 20 to 30 degrees, was the distribution of slopes uniform or does it favor certain values.*

   The specific details of each field site were recorded, including: longitude, latitude, elevation, aspect, slope incline, and subjective ordinal scales for sky view factor,

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sun exposure, and wind exposure. These parameters are listed and described in our expanded methods Sect. 2.1. We qualitatively describe the distribution of these parameters, but do not present histograms.

4. *1861:7 Where does 239,152 number come? You state the simulation occurs at 393 grid points (225+168) and that data was pooled for every hour for 26 days. If this was done for 12 virtual slopes, then the total number of simulations should be (225+168)*(26*24)*12 = 2,942,784. This indicates that you are omitting a significant portion of the data, why?*

   We explain our data set size more clearly in Sect. 2.4. The analysis only considered flat field simulations, which resulted in roughly 12 times less data. We could have included simulations from virtual slopes, however many of the inputs would be repeated (e.g. air temperature, humidity, wind speed) since only radiation inputs differ on virtual slopes. We also omitted time steps when surface hoar was not present on the surface (e.g. when it was snowing), so that our analysis would focus on conditions that directly influenced the growth or shrinkage of crystals. Our revised analysis uses a similar approach, but explains the methods more clearly. We also extended the study period to cover six months (October – March) to capture a broader range of meteorological conditions.

5. *1862:15 Should read "...added by deposition of water..."*

   Changed as suggested.

6. *1862:21 Why was the user-defined threshold of 3.5 m/s selected?*

   The threshold of 3.5 m/s was used for several reasons. Firstly, it is the default SNOWPACK setting, likely based on the findings of Hachikubo (2001) who measured negative sublimation rates at high speeds. We considered calibrating the threshold with our field observations, however we lacked detailed wind measurements at different locations to do so. We justify our choice in Sect. 2.3.

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7. *1862:24 Why was the number 12 selected and what was different between the 12 runs?*

   The 12 virtual slopes were vaguely described in the discussion paper, so we have revised Sect. 2.3 to make the choice of virtual slopes clearer. Six slope simulations were used to isolate the effects of slope incline, namely north and south facing slopes with inclines of 15, 30, and 45° (Fig. 9). Another eight slope simulations were used to isolate the effects of slope aspect, namely 30° slopes in eight cardinal directions (Fig. 10). This combines to give 14 slopes (6+8), however since the north and south 30° slopes are duplicated, there are only 12 unique slopes (14-2). The number of slopes were sufficient to show the predominant effects of incline and aspect in the model (e.g. Fig. 9–10).

8. *1863:3 The paper mentions that the sensitivity of SNOWPACK was analyzed, although you fail to mention any specific method for the sensitivity analysis. Were the input parameters (i.e., the weather data) perturbed systematically or some sort of formal selectivity analysis performed on the model?*

   It seems that you generally ran the model with direct input from the weather data and then extracted each timestep to build up a dataset to perform an informal sensitivity analysis. Without defining the input parameter distributions it is not possible to determine the true sensitivity of the system. Thus, the word sensitivity should be avoided as it has a specific meaning. Also, the distribution of input parameters at an hourly rate used to formulate the model input should be reported.

   Was any consideration given to the inaccuracies of the supplied weather data and how these inaccuracies impact SNOWPACK? For example, does the wind speed differing between 1 and 2 m/s produce drastically different growth rates?

   You mention the importance of various results in the remaining portions of the paper; how has the importance of a factor determined?

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We have addressed many of these comments above in the "General comments" section, as they reflect the reviewers general concerns about the analysis of meteorological data. In short, we provide more detail in the methods section and changed our interpretation of the analysis throughout the manuscript to avoid “sensitivity” type statements.

The distribution of input parameters are now shown in Fig. 2 as the box widths are proportional to the square root of the number of observations in each group.

Inaccuracies in the NWP inputs were broadly addressed in Sect. 3.2 “Evaluation of weather forecasts”, but we now discuss the implications of these errors on the surface hoar model at the end of Sect. 3.1. The question of how NWP errors affect growth rates is important, however a formal sensitivity analysis is needed to answer it. Also, a sensitivity or uncertainty analysis of the entire SNOWPACK model would be valuable to the snow cover modelling community.

9. *1864: 3-4 It is stated that “longwave radiation was less prominent” and go on to discuss that the weather surrounding the study where generally clear during the entire study. This highlights a limitation of the analysis presented, without comparing weather conditions throughout a wide breath of conditions it is difficult to determine what conditions are the most influential, thus your results are limited and only apply to narrow set of input conditions.*

   Our revised manuscript no longer makes statements about the relative importance of weather inputs. We also expanded the analysis to cover six months of weather data to cover a broader range of input weather conditions. The analysis shows general conditions associated with surface hoar growth, and suggests some probable interactions between the inputs.

10. *1870: 6-7 “Surface hoar modeled with SNOWPACK was sensitive to the moisture content of the air, where warm and moist air produced the most surface hoar.” This sentence is misleading; it implies that a complete sensitivity analysis was*

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performed, which it was not. This conclusion is only for a very specific set of data using a sampling scheme that is biased to certain conditions given the supplied weather parameters.

Our new interpretation of the weather data avoids claims about model sensitivity and gives a broader conclusion: “Modelled surface hoar growth was associated with warm air temperatures, high humidity, cold surface temperatures, and low wind speeds”.

Interactive comment on The Cryosphere Discuss., 9, 1857, 2015.