

Response to Reviewers

Reduced Surface Hoar in a Warming World

Rudisill et al., *The Cryosphere*

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We thank the editor and reviewers for their careful reading of the manuscript and for their constructive comments. Below we address each comment in turn. Reviewer comments are shown in blue; our responses follow *green text*. We made several notable changes to the manuscript. Several areas where methods-like content appeared in the results were moved to the Methods section. We also improved the presentation of several figures. A cascade of figure labeling errors meant that some figures were incorrectly cited; we have fixed all of those cases. In addition to the reviewer comments, we also noticed that we erroneously referred to the latent heat of sublimation as the latent heat of vaporization (but this did not change the results). This has been updated. To aid the reader, we have also added a table of the commonly used abbreviations in the paper.

Reviewer 1

General comment:

Reviewer 1: This manuscript is the first report mentioned climate change effects on surface hoar events. Stability correction is a critical factor in discussing surface hoar formation, and this paper focuses on its parameterization. As for the observation results of the surface heat balance (Fig. 2, Fig. 3), the findings are essentially consistent with those of Hachikubo and Akitaya (1997) and Stossel et al. (2010). Although I would assess the novelty as limited, the observed data are valuable, and I do not see any issues with the content. This is likely the first instance of upper-air meteorological observations during surface hoar formation, and it is interesting that latent heat flux was demonstrated to be downward from the upper troposphere to the snow surface. I feel like I've read an impressive paper in a while. I can recommend its publication, because the manuscript will be of interest to the readers of EGU sphere. Minor revision will be needed, my comments are as follows:

Because I am a field researcher rather than a modeler, I will focus exclusively on observation results.

Response: Thank you for your careful review of the manuscript. We agree that some of the fundamental observations are similar to Hachikubo and Akitaya (1997) and Stossel et al. (2010). We would like to point out, though, that the insight that there is a sensitivity to antecedent snow conditions via a snow thermal conductivity mechanism was not brought up or tested in those papers (though we do not show field evidence of this, only model results).

Comment 1:

Reviewer 1: I have a question regarding the Stössel box. When surface hoar forms, the relative humidity profile reaches its minimum at the snow surface, as shown in Fig. 4. In addition to surface hoar, another “frost”, which generates near-surface faceted crystals, should also form under the box. Removing this frost is considerably difficult. I understand this should have been addressed in this study, but I would like to confirm that the frost on the underside of the box is properly removed before each measurement. Based on this point, I believe that the automatic measurements described in the section of Discussion are practically impossible.

Response: Thank you for your excellent question. We have conferred with the field observers and they confirmed that, on a handful but not all of the observations, there was frost on the bottom of the box which they wiped off before weighing the box. They did not record the times that this occurred, though, and this is an interesting point to consider in future work. We have added a line to the discussion of the Stössel box observations to discuss this. We have removed the language about the automatic weighing device, since it is out-of-scope to provide a full accounting of what such a device might look like or how it could operate.

Comment 2:

Reviewer 1: An infrared hygrometer is required to measure latent heat flux using the eddy covariance method. When surface hoar forms, it is likely that frost will also form on the window of the infrared hygrometer. How was this phenomenon overcome?

Response: Thank you for bringing this up. We conferred with the ARM experts on infrared hygrometers to get their perspective (<https://armgov.svcs.arm.gov/connect-with-arm/organization/instrument-mentors/list>) on this problem. They let us know that if the “AGC” (Automatic Gain Control, comparable to ‘signal strength’ in newer models) is at its highest bit value, H₂O/CO₂ readings are typically set to a bad value, which likely happens if frost is present. Before computing the 30 minute fluxes, we discarded any of the 5-minute flux data that had H₂O quality flags signalled. The additional stationarity tests of Foken and Wichura (1996) also serve as another check, and times with frost interfering with the fluxes likely do not meet these criteria. That being said, we cannot fully rule out the possibility that some frost was present at times and interfered with the flux calculation. The multiple lines of evidence (comparisons against SUMMA, comparisons against theoretical bulk transfer coefficients and gradients) suggest that in many cases valid overnight fluxes were being measured in many circumstances despite the host of challenges for this site and conditions (e.g., complex terrain, weak turbulence). We have added a line that states this in the Flux section in the methods.

Comment 3:

Reviewer 1: I would like to confirm whether the specific heat of air (c_p) is a function of temperature and pressure in the turbulent flux calculations. In Fig. 2, the atmospheric pressure is approximately 700 hPa. Compared to zero meters above sea level, sensible heat flux is relatively smaller at high altitudes such as mountainous regions.

Response: Thanks for your comment. We made sure to update the value of air density in the sensible heat flux calculations computed by SUMMA. This is not actually dynamically adjusted in SUMMA by default; I made a github comment about this in the model code. Please see: <https://github.com/CH-Earth/summa/issues/593>. We state that this adjustment was made in Line 234 of the updated manuscript (and a slightly different line number in the original manuscript). The specific heat capacity dependency on pressure and temperature is much smaller, however, so this is treated as a constant. We now state that explicitly in the text on Line 109. This paper discusses some the temperature dependency of c_p <https://doi.org/10.5194/acp-20-15585-2020> which may matter for some applications in atmospheric sciences, but is again small for this application.

Comment 4:

Reviewer 1: There is no explanation for Fig. 1e in the figure caption; there are two instances of (d). The relationship between (a) and (e) is unclear. Latitude and longitude information might be required for Fig. 1a.

Response: Thank you, we have now renamed the second (d) to (e). We also updated the map to have the lat/lon of the M1 site labeled. We would prefer to keep the units of map (a) in Easting/Northing since it is easier to understand the distance between the two primary sites if the units are in km.

Small points:

Reviewer 1: L285: Fig. 3c is wrong, 3d is correct?

Response: We are referring to both 3c (reduction in Rnet during clouds) and 3d (the change in turbulent fluxes). We have updated the reference.

Reviewer 1: L309: (Fig.9))?

Response: Thanks for noticing that; we meant to refer to Figure 4. This has been updated.

Reviewer 1: L373–: It is incorrect that the explanation for Fig. 8 appears before that for Fig. 7 in the text. The figure cited in Section 3.4.1 is listed as Fig. 7, but Fig. 9 is correct. The original Fig. 7 is not cited in the text. The figure citations throughout the manuscript should be checked again.

Response: Thanks for noticing this. The error arises because we did not reference Fig. 7 in the paragraph between the original lines 365–370, nor Fig. 10 in section 3.4.2. We have

updated that in the updated manuscript and have also gone through and reviewed all figure references to make sure they are in chronological order and are correctly cited.

Reviewer 1: L481–485: I do not understand the meaning of “apparent paradox”. I do not find it obvious that surface hoar accumulation increases with increasing wind speed. As Hachikubo (2001) *Ann. Glaciol.* demonstrates, an increase in wind speed promotes sensible heat, raises the snow surface temperature, and reduces Δq . On the other hand, under calm conditions, only molecular diffusion of water vapor occurs, which is insufficient to maintain the surface hoar growth. Therefore, it is clear that an optimal wind speed range exists.

Response: Thanks and we tend to agree with that framing of the question. The paradox arises from just thinking about the moisture flux alone, uncoupled to the snow surface temperature energy balance. In that context one might expect surface hoar flux to increase with wind speed. But we agree this might not be a real “paradox” so we have removed that line from the text. It now says “The fact that only low...”.

Reviewer 1: L526–531: This is a comment. The snow surface is a “layer”, not a “plane”. Even when sublimation dominates in the energy budget, surface hoar may still be maintained on the snow surface (Hachikubo and Akitaya, 1998, *Ann. Glaciol.*; Stössel et al., 2010).

Response: Thanks for this feedback. We have updated the text to make this distinction more clear.

Reviewer 2

General comment:

Reviewer 2: This study investigates the formation processes of surface hoar using various meteorological observation data. It also simulates future changes in surface hoar using a numerical model SUMMA. Although the observation area is limited, the study clarifies the formation processes of surface hoar in the study area based on detailed observations. The analysis is also detailed, and I found the study to be carefully conducted. I have no objections to the overall content of the study, but there is room for improvement in the quality of the writing. I hope that revising the manuscript with reference to the comments below will help improve its quality.

Response: Thanks for your feedback and careful review of the manuscript. Combined with reviewer 1 and the editor comments, we have made substantial edits to the manuscript to improve the writing and clarity.

Major Comment 1:

Reviewer 2: When surface hoar forms, does the roughness length of the snow surface change, and consequently the bulk coefficients as well? Looking at the result that C_d deviates from the theoretical curve when Ri is high, could this indicate that surface friction changes

due to the formation of surface hoar? In this study, z_0 , z_h , and z_q appear to be assumed to be 2×10^{-4} m. I think it would be better to discuss how the use of these assumed roughness lengths may have affected the results of this study.

Response: Thanks for this comment. It has been speculated in previous work that this may occur (Hachikubo, 1997). While we do prescribe values of z_0 , z_h , and z_q in SUMMA, we would like to point out that these values were vetted against the observational data in Figure 6.

Major Comment 2:

Reviewer 2: Related to the comment above, if the formation of surface hoar increases friction and enhances turbulence, would this make surface hoar more likely to form? I thought it would be interesting if there were a process similar to positive feedback in surface hoar formation. This is more a matter of my own curiosity than a criticism of the manuscript, but if the authors can develop some discussion on this point, I would appreciate it if they could add it to the manuscript.

Response: We think that this could be an interesting possibility, however we do not think that we have sufficient data to confirm/deny that this is something that we have observed, and the model we use does not have a dynamic snow surface roughness (nor do any snow models, as far as we are aware) to test this assumption.

Major Comment 3:

Reviewer 2: The manuscript does not provide enough information on the experimental settings used for the sensitivity experiments and future projection experiments with SUMMA. Please describe these settings more carefully in the Methods section. Also, as I point out in a later comment, some experimental settings are currently described in the Results section, which is not appropriate. Please revise this point with reference to the comments below.

Response: We have reviewed the manuscript and have made the following changes. We moved the description of the thermal conductivity sensitivity tests, which previously were in Sec. 3.4.2 to the methods section. We also added some additional description of how WUS-D3 data were used to force SUMMA. The actual model names were never listed in the main text (except for the caption of Fig. 11), which may have caused confusion, so we now define that in the methods section as well. The terms "BOC", "MOC", and "EOC" were also not articulated in the methods, so we now do that and as a consequence we now refer to those terms with the acronym to avoid redundancy. With these changes we believe that we have substantially improved the manuscript.

Specific Comments:

Reviewer 2: L40–41: Sublimation is more likely to occur when air temperature is low and the air is dry. Do previous studies suggest that surface hoar formation is more likely to occur under higher air temperatures and moist air conditions? If so, it would be useful to describe such temperature and humidity conditions in this paragraph.

Response: Thanks for this observation — we note in Line 50 that Feick, 2007 found that dry katabatic winds inhibited surface hoar. Slaughter, 2011 also suggested that humid winter nights were beneficial for surface hoar formation. However, in our review of the surface hoar literature, we found that it was difficult to characterize definitive statements about the climatological relationship between surface hoar and atmospheric humidity. Therefore, we think it is best to leave this in the discussion section where we go into this in more detail (the paragraph starting on Line 516).

Reviewer 2: L68: I think Section 2 would be easier to read if the structure of the section were briefly described here. Alternatively, a brief overview could be added at the beginning of Section 2.

Response: Thanks for noticing this. We have provided a description of Sec. 2 which we hope lets the reader better navigate the content of this paper.

Reviewer 2: Section 2.1: Since many abbreviations are used throughout the manuscript, it would be helpful for readers if a table summarizing the meanings of all abbreviations were added to the Supplementary Material.

Response: We wholeheartedly agree and have now added two tables to the appendix that describe the acronyms. **Note to editor: We have made this into two Latex tables, but they can be merged into one for publication**

Reviewer 2: Section 2.1.1: Starting directly with the equation for the snow surface energy balance felt somewhat abrupt. It would be helpful to add a brief explanation at the beginning of this subsection as to why the discussion starts with the surface mass balance. At present, it gives the impression that this is written simply as a convention. I think it would be useful to briefly describe the relationship between surface hoar and the surface mass balance.

Response: Thanks for this recommendation. We have added a sentence to the "Governing Equations" section that sets up the discussion of the energy budget.

Reviewer 2: L84: It would be useful to add the values of the emissivity and the Stefan–Boltzmann constant.

Response: We have added the Stefan-Boltzmann constant. Since the emissivity is not a constant per-se, but rather assumed constant for this study and many others, we discuss this value in Section 2.1.3 and provide a citation to Huang et al. 2016 as justification for the constant 0.98 value for emissivity. We think that it is better to keep the discussion of the emissivity value in that section.

Reviewer 2: L85: Was a fixed value used for the albedo α ? Could the formation of surface hoar affect the albedo?

Response: No, we use the SUMMA model's albedo using several parameterizations that treat albedo as a function of solar zenith angle, air temperature, and the time since the

last snowfall, but water vapor deposition or surface hoar is not considered. We agree that surface hoar may influence albedo, and list that in the first sentence of the introduction as a motivation for studying surface hoar. We now discuss this in the last section of the discussion section and we reference Flanner 2006 as well as the Armstrong, 2008. Both describe the mechanisms through which surface hoar may tend to increase albedo. We think that this could be an additional important mechanism through which declining surface hoar may impact the snow energy balance.

Reviewer 2: Section 2.1.2: At first, I thought that Eqs. (3), (4), (6), and (7) might be unnecessary if the purpose was only to explain how F_q is calculated. However, since quantities such as C_h are used in the Discussion, it may be helpful to add a brief statement that these equations are used to interpret the behavior of the water vapor flux.

Response: Thanks for this comment. We agree and first thought that it might not be relevant for the paper to list these. We have added a sentence to Line 112 to provide some justification.

Reviewer 2: Eqs. (2) and (3): C_h and C_q are reversed.

Response: This has been fixed.

Reviewer 2: Eq. (4): In this equation, the denominator is written as ρ . Since the left-hand sides of Eqs. (2) and (3) are expressed using the power of -1 , would it be better to write this term as ρ^{-1} in the same manner? Also, the friction velocity $u_\star$ does not appear to be explained in the main text.

Response: Thanks for finding this. We have written the ρ using the power of -1 now. We also have defined the friction velocity.

Reviewer 2: L102: The value used for L_v , which is related to F_q , should be stated. Is it treated as a constant, or is it estimated from calculations?

Response: This is treated as a constant. The value is 2,838 KJ/kg which we now include in the text. We now have a sentence stating this. Please note that this has also been updated to the latent heat of sublimation (phase change of water vapor to/from solid), not vaporization (phase change of water vapor to/from liquid). The correct value was used in the code but the wrong name was in the paper. All instances have been corrected.

Reviewer 2: L102: In the text, L is written as an uppercase letter, whereas it is written as a lowercase letter in Eq. (2).

Response: This error has been corrected.

Reviewer 2: L119: What assumption is made for z_h ?

Response: We meant to state that all the roughness coefficients are assumed to be equivalent. This has been updated in the text.

Reviewer 2: L154–155: It would be useful to add the latitude and longitude information for SAIL and SPLASH.

Response: This has been added to Line 156. Based on the other reviewer comments we also added lat/lon information to Figure 1 and added additional gridlines to Figure 1.e to make the location more clear.

Reviewer 2: Table 2: The term “Fluxes” alone is not sufficiently clear. Please describe what flux each of the three symbols represents.

Response: We have updated the table and separated out the fluxes so they are each defined.

Reviewer 2: L224: The manuscript should specify which variables were used as atmospheric forcing inputs to SUMMA, such as air temperature, humidity, and precipitation.

Response: We have added additional description to Sec 2.4.2 and Line 251. The complete set of variables used to run SUMMA are wind speed, downwelling shortwave radiation, downwelling longwave radiation, barometric pressure, and precipitation.

Reviewer 2: L224: Was the ensemble mean of WUS-D3 used as atmospheric forcing? If this was an ensemble simulation, how many ensemble members were used?

Response: No, we actually ran SUMMA nine separate times with each model forcing. The different colored dots in Figure 12 and 13 are the bin-means for each SUMMA run with the respective WUS-D3 forcing. We have updated the text in Fig 12 to better articulate this point. Line 274 was also changed to highlight that SUMMA was run for each WUS-D3 forcing (yielding 9 sets of model output) rather than once for an ensemble mean forcing.

Reviewer 2: L233: Was the SUMMA simulation conducted at the same spatial resolution as the atmospheric forcing data? It would be easier to understand if the spatial resolution of the model were described earlier in the manuscript.

Response: We ran SUMMA for a single point, so it is at the same resolution as the forcing dataset, which is approximately 9 km.

Reviewer 2: L244–245: Why was a 41-year period used for the first half, while a 21-year period was used for the latter half? If the former period is intended to represent the early 21st century, would it not be more accurate to define it as 2000–2020?

Response: The 1980–2020 period was chosen as a 40-year baseline to ensure a statistically robust representation of the historical climate mean state, reducing the influence of interannual and decadal variability. We argue that the 2080–2100 period, while shorter, is sufficient to characterize end-of-century conditions as it represents a stabilized future climate state.

Beginning of century nomenclature is still appropriate, we would argue, since it is centered on the year 2000. We would also like to point out that the data is presented in several other ways including by 20-year average, warming level, and in the text is also presented as the slope with respect to temperature change, so there are multiple ways of interpreting the changes that are independent of the in-text statement comparing the end to the beginning of the century.

Reviewer 2: L247–248: If the “SUMMA GCM-forced experiments” refer to ensemble simulations, the number of ensemble members used in the experiments should also be stated. My understanding is that WUS-D3 provides ensemble forcing data, and that SUMMA was driven by these forcing data, meaning that the model outputs are also an ensemble. It would be helpful to include such an explanation.

Response: WUS-D3 is an ensemble in so far as it provides multiple different downscaled GCMs, but other products might be considered a “large ensemble” with many more members (e.g., the CESM large ensemble; <https://www.cesm.ucar.edu/community-projects/lens>). We now list the specific downscaled GCMs in line 264. The second paragraph (line 273) of section 2.4.2 describes the SUMMA ensemble in more specific detail.

Reviewer 2: Results: There are many incorrect figure reference numbers throughout the Results section. I point out several examples in the comments below, but there appear to be others as well, so please check them carefully throughout the section.

Response: Thank you for noticing this, and the first reviewer also found this. We apologize for this oversight. There was a cascade of errors referecing the figures starting on Figure 4. We have gone through and ensured that all the figures are referenced correctly.

Reviewer 2: L252–253: Figure 2 appears to include observational results from the M1 site as well, but this sentence mentions only the KP site.

Response: We only use meteorological data (air temperature, humidity, wind at a single measurement height) for the analysis in Figure 3. This is just because the M1 site has 2 full winters of data, as opposed to 1 winter of data for KP so we have more data points to compute the clear/cloudy composites. We cite this data in the data availability section (Kyrouac et al., 2021) and have now added it to Table 1. The M1 data in the first iteration of the table just referred to the Sonde data (not surface meteorological data).

Reviewer 2: Figure 2: Please clearly indicate which meteorological data were obtained at the M1 site and which were obtained at the KP site. For example, Table 2 states that T_{air} was obtained at both the M1 and KP sites.

Response: We have improved our description of this in the main body of the text, which we think is better than listing the locations in the figure caption in order to avoid repetition. Consequently we removed the reference to M1 in the figure. Please see the previous comment with the note about data at M1 and KP.

Reviewer 2: Figure 2d: The labels $T_{s.4m}$ and $T_{s.9m}$ can be read as 4 m and 9 m, respectively, so it would be better to revise the notation.

Response: Thanks for noticing this. We have changed the legend size, formatting, and changed it to 40 cm and 90 cm respectively so it is easier to read.

Reviewer 2: L258: Looking at Figure 2d, T_{sfc} appears to be higher than T_{air} almost throughout the period. Is this the opposite of what is stated in the text?

Response: Thanks for pointing this out — this is perhaps confusing. The y-axis is inverted such that colder values are higher on the y-axis, and 0 degrees C is on the bottom of the figure. We have added a line to the figure caption to state this explicitly to make sure the reader can follow.

Reviewer 2: L264: Should this be January 27–31?

Response: Thanks for finding this error – we have updated the text to state that it is Jan 27–31.

Reviewer 2: L271–274: Is there a reason why 25% was chosen to define clear-sky conditions?

Response: The 25% threshold is based on results from previous work (Rudisill, 2025) at this same location. We have performed a number of ad-hoc tests changing the threshold, and it does not impact the results significantly. With more data, a more strict clear/cloudy threshold could be imposed, but given that we have only two winters of data, a wider 25% threshold is necessary. We report the number of clear/cloudy days to establish that the samples are reasonably sized to produce the composite plots.

Reviewer 2: Figure 4: I am concerned that panels a–g are not arranged in chronological order. Is there a reason for this?

Response: Thanks for noticing this. This was intentional, since we have ordered the dates by the size of the observed surface hoar events, which is the same way that they are ordered in Figure 8. We have added text to the figure caption to explain this. We think it is probably best to keep this ordering in order to be consistent with Figure 8.

Reviewer 2: L309: Figure 9 → Figure 4.

Response: Fixed.

Reviewer 2: L310–311: Please add a statement indicating that this sentence refers to the results shown in Figure 4h.

Response: Thank you for this comment. This has been updated.

Reviewer 2: L318–319: It would be easier to compare this statement with Figure 2c if the specific wind direction were also described.

Response: We have added text that now describes the wind direction to aid in the interpretation of the data in Fig 2c.

Reviewer 2: Figure 5d: The notation on the horizontal axis should be revised, as the overbar and slash are too close together and look like a square-root symbol.

Response: Thanks for this comment. We have now added additional spaces to make it more clear.

Reviewer 2: L330–339: Most of this paragraph describes content that should be included in the Methods section, so it would be better to move it there. There does not appear to be a subsection on the SUMMA parameterizations in the Methods section, so I think it would be useful to add such a subsection.

Response: Thanks for this comment. We have moved some of the text from this section to Sec 2.4 which has been added to the manuscript. We believe this has helped increase the readability of the paper as a whole.

Reviewer 2: Figure 6: The caption does not seem to explain what the gray and light-blue markers and the black circles represent. Also, it seems unnatural for the vertical axis to show only “dimensionless” as the unit. Would it be better to label it as “bulk transfer coefficient (dimensionless)”?

Response: Thanks for noticing this. We now explain the meaning of the blue dots and the black open circles in the figure caption. Concerning the y-axis label, we tried to keep a consistent standard throughout the paper of just labeling the units on the y-axis (except Fig. 10). So we think that it is better to keep this convention for this figure as well.

Reviewer 2: L360–364: This paragraph should also be moved to the Methods section.

Response: We moved sentences about comparing model to observed fluxes to the Methods section. However, we leave in two sentences about how we selected parameters based on the prior results from Sec. 3.3.2 since this logically follows from material presented in the results section.

Reviewer 2: L365–369: Please make it clear that the results described in this paragraph refer to Figure 7.

Response: Thank you for noticing this. We are referring to Fig. 7a–d in this section and have made appropriate citations to the figure and each sub-figure in the updated manuscript.

Reviewer 2: L366: If the correlation coefficients are 0.75 and 0.94, why are the corresponding squared values, R^2 , 0.13 and 0.75?

Response: Thanks for noticing this. We reviewed the code and found that we were using the wrong formula. The pearson-r value is computed using the numpy corrcoef function in

the python programming language. We have corrected the R^2 values in the figure and the text.

Reviewer 2: Figure 7a, b, d: Please ensure that the dashed 1:1 lines do not terminate in the middle of the plots. Also, for panel d, it is better to set the minimum value of the axis to 0, since relative humidity cannot be negative.

Response: We have extended the 1:1 lines throughout the entire range of the plotted values and have set the limits of humidity to 0.

Reviewer 2: Figure 8: It feels unnatural that the dates on the horizontal axis are not arranged in chronological order. I recommend rearranging them in chronological order. It is also difficult to tell which bar each date label on the horizontal axis refers to. In addition, there does not seem to be any explanation of SB1 and SB2 in the legend, so please add one. Please also explain the meaning of the purple V-shaped symbols.

Response: Thanks for pointing this out. We have updated the text to explain that the events are ordered by the size of the event, starting with the 'no surface hoar' confirmed event. We think that this ordering is appropriate for two reasons. First, we are not analyzing day-to-day hysteresis or any type of temporal relationship between events. The second and primary reason this was chosen is that the figure looks cluttered, and it's harder to visually see the relationship between SUMMA and the observations when they are ordered by date. The purple "V" markers are nights that had observed surface hoar. This might seem somewhat redundant, but the intention is to have parity with Figure 2 where we highlight the surface hoar events in the longer time series. We have also labeled the purple V markers in the figure caption now. SB1 and SB2 refer to the Stossel box observations. We describe those terms in the figure caption, but we do not introduce that abbreviation previously. Rather than introduce another acronym definition, we have renamed SB1 and SB2 to "Box1" and "Box2" and define this in the caption.

Reviewer 2: L375–377: This statement is more interpretive in nature, so it would be more appropriate to move it to the Discussion section.

Response: We have moved this line to the discussion section.

Reviewer 2: L375–377: The Louis formulation for C_d does not reproduce C_d well under high Ri conditions. How is C_d calculated in the SUMMA model? Even if wind speed is weak, a high C_d would allow momentum to be transferred to the snow surface more efficiently. In such a case, could more efficient water vapor deposition occur?

Response: C_d is calculated in the SUMMA model using Eq. 7. And the curves that we show in the that figure show three different parameterizations, described in Table 1. We select the "Louisinv" function. We do speculate that this is the case—the results would seem to imply that there is more turbulence than predicted at high Ri numbers, which may be why there is more observed surface hoar in some cases than the model prediction. This is discussed in the second paragraph of the discussion section. In future work, we think that

we could use additional eddy covariance observations to investigate turbulence at high Ri numbers, katabatic flows, and potentially other mesoscale circulations such as elevated jets and their impacts on turbulence.

Reviewer 2: L402–403: The expression “decline” is somewhat misleading. Would it be better to state that Δq “closes to zero”?

Response: In this line, we say that the *magnitude* declines which is still technically correct. To aid in the interpretation, we have now put “becomes closer to zero” in parentheses next to this statement.

Reviewer 2: L403–404: The figure reference is incorrect. It should be Figure 9, not Figure 7.

Response: Thank you. This has been updated.

Reviewer 2: L406–408: The abstract states the following, and the wind speed threshold given there is slightly different from the one stated here. The specific values should be made consistent between the two sections: “both SUMMA and observations agree that deposition fluxes are favored when overnight air temperatures are less than -8 to -10 °C and w_{spd} is less than 2 to 3 m s^{-1} .”

Response: Thank you for pointing out this discrepancy. In the abstract we state 2–3 m/s, which we think is more appropriate than simply stating 3 m/s, since the bins we use have a 1 m/s width. So, we have updated these lines to say 2-3 m/s. We have confirmed that other instances of the wind speed threshold are consistent if stated.

Reviewer 2: L412–417: I think this paragraph should also be moved to the Methods section. Overall, some parts of the experimental settings and assumptions are described in the Results section, which makes the manuscript difficult to follow.

Response: Thanks for this feedback — We have moved this section to the end of Section 2.4.2.

Reviewer 2: L417: Is the figure being referred to here Figure 10?

Response: Yes, this was again the result of cascading errors in figure numbering. This has now been updated.

Reviewer 2: Figure 10a: The caption does not explain the difference between the solid and dashed T_{sfc} lines for each value of λ .

Response: We have updated the text to also state the meaning of the dashed lines. This is also shown in the figure legend.

Reviewer 2: L443: Looking at Figure 13b, the trend appears to increase year by year, but the description in the text seems to imply the opposite. I assume the authors mean that the

amount of surface-hoar deposition is decreasing, but since the flux itself is actually increasing, the current wording is inappropriate. Because the same expression appears throughout the manuscript, it would be better to describe this point more carefully or revise the wording.

Response: Thanks for pointing this out. In L443, we stated that the 'magnitude' of the flux is decreasing (meaning it is getting closer to 0). We think this is correct to say, but it might be easy to misunderstand. We now state $\overline{F_{q,tot.}}$ increases (becomes less negative) annually at a rate of ..." We then updated L447 from "The reduction in $\overline{F_{q,tot.}}$ " to "This pattern...". Line 561 in the conclusions stays the same, however. It states, "Despite increasing atmospheric humidity the total amount of water vapor deposited annually onto the snowpack (taking into account both sublimation and deposition) decreases by 81% by the end-of-century under the SSP3-7.0 emission scenario at a rate of 6.1 gm² per degree of warming." This is still correct and the way it is phrased is agnostic to the particular sign convention of $\overline{F_{q,tot.}}$.

Reviewer 2: Figure 13: Because sublimation flux increases under warming, the total flux also increases. However, the deposition flux also appears to show a slight increasing trend. Since the number of nights on which surface hoar forms decreases under warming, could it be that a larger amount of surface hoar forms during each individual formation night? If so, the risk of hazards such as avalanches related to surface hoar may also increase.

Response: This is a great point — we do state in L447 that "...deposition events increase in size slightly up until 2°C...". We also examined changes in the distribution of surface hoar sizes with warming to see if there were significant changes in the distributions (e.g., the historical 95th percentile event becomes more common in the future) but we did not find a robust trend. We are not exactly aware of a study that relates the size of surface hoar events to avalanche hazard. This makes sense in principle, but other factors — such as whether surface hoar survives long enough to be buried by subsequent snowfall — are necessary to consider before relating the two.

Reviewer 2: L457–458: The statement "but some mechanistic descriptions are not accurate from a climatic perspective" seems somewhat excessive. I agree that this study reveals the formation process of surface hoar using detailed meteorological observation data, but it should be noted that the validation is limited to a restricted region. In other regions, the formation processes of surface hoar may differ from those observed in this study.

Response: We now say "We show that several existing theories of surface hoar formation are largely confirmed, but some additional important mechanisms are proposed and nuances of the relationship between climate and surface hoar are explored more than in previous work." We agree that surface hoar formation may have different sensitivities depending on the snow climate.

Reviewer 2: L480–485: In this paragraph, rather than using a shortened explanation such as "The effect is described in part by climatology," please explain clearly and carefully why weak wind speed is important for surface hoar formation. I think the content of this paragraph is one of the important findings clarified by this study.

Response: This sentence now reads "The effect is described in part by climatology: strong winds greater than 2-3 m/s are often associated with cloudy airmasses (Fig ...) which increase H_s directed towards the snowpack, destroying the near-surface inversion and favorable Δq ."

Editor

General comment:

Reviewer : Editor: I would like to thank two anonymous reviewers for providing constructive comments and suggestions. Based on the review results, my current impression of this manuscript is positive. I expect the authors to consider the review carefully and revise the manuscript appropriately.

In addition, I have the following technical comments, which I would like to ask the authors to consider.

Reviewer : Editor, L. 13: It is better to add the definition of Ri in the abstract.

Response: Thanks for this observation. Rather than define the abbreviation twice we now spell out "bulk Richardson number" in the abstract.

Reviewer : Editor, L. 15: You introduce the mathematical symbol w_{spd} without definition in the abstract. I recommend rephrasing the symbol to "horizontal wind speed."

Response: We have made this correction.

Reviewer : Editor, L. 16: "thermal conductivity mechanism": Maybe typo? It is better to rephrase this part to something like "heat conduction mechanism" or "thermal conduction mechanism."

Response: We have changed the wording to "thermal conduction mechanism" and also edited other examples when this is used.

Reviewer : Editor, L. 115: "The bulk Richardson. (Ri) number" → "The bulk Richardson number (Ri)."

Response: This typo has been corrected.

Reviewer : Editor, L. 145: "a nights where where": Typo.

Response: Fixed.

Reviewer : Editor, L. 190–191: "Snow height (Snowh)": This abbreviation should be introduced and defined at L. 145.

Response: Thanks for finding this. It has been updated.

Reviewer : Editor, L. 226: “CMPI6”: Typo?

Response: Fixed.

Reviewer : Editor, Figure 6: When I first saw this figure, I was a bit confused because I felt “0.51” was indicated on the x-axis. But I understand you want to indicate 0.5 and 1 here. Can you improve the x-axis presentation?

Response: Thanks for this comment. We have rotated the x-tick labels to make this more clear.

Reviewer : Editor, Figure 11 caption: Some readers might not understand the meanings of the model identifiers such as “r2i1p1f2” and “r1i1p2f1.” I recommend explaining the meanings of these identifiers in the caption or in the running text (maybe at L. 224–232). Related to this point, you do not introduce these GCMs in the running text. I think it is helpful for readers to list and briefly explain these GCMs in Sect. 2.4.

Response: Thank you for this finding this. Based on this comment and the reviewer comments, we have now explained the meaning of the models in the text and articulate which specific models we use from WUS-D3. Previously the specific models were just listed in this figure caption.

Reviewer : Editor, L. 427: “WUS”: Please indicate its definition here.

Response: Since we only use WUS once (not in the context of “WUS-D3”) we now just spell out “western USA”.

Reviewer : Editor, Figure numbers: To increase readability, please number figures in the order in which they are mentioned in the manuscript:

- L. 309: Figure 9 is referenced before Figs. 5, 6, 7, and 8.
- L. 373: Figure 8 is referenced before Fig. 7.
- L. 427: Figure 11 is referenced before Fig. 10.

Response: Thanks for finding this. We made a series of errors referencing the figures starting with figure 4, including some incorrect figure references noticed by the reviewers. We have fixed those errors and now make sure that all the figures are referenced consecutively.