The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-205

Anonymous Referee #1

Referee comment on "Snow dune growth increases polar heat fluxes" by Kelly Kochanski et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-205-RC1, 2021

General comments:

The article seeks to address how snow bedform formation impacts conductive and shortwave radiation fluxes—an important subject that is not fully understood nor modeled. The authors seek to establish quantitative relationships between snow dune morphology and model-available parameters, like wind speed and snowfall amount. The goal, of significant merit, is to produce relationships that can be incorporated into models, enabling the conductivity of the snowpack to be better constrained. The authors have clearly identified a problem of great significance and motivated the need for the work well.

The article has significant flaws that need to be addressed for its conclusions to have validity and, unfortunately, is not publishable in its current format. First, the heat fluxes discussed depend on the spatial distribution of snow thickness. However, at no point does the article validate the modeled distribution with measurements of snow thickness. Neither does the supplement nor do the prior cited works that use these data. Second, the article extrapolates from Niwot Ridge, Colorado to locations as diverse as Arctic sea ice and the South Pole without providing evidence (e.g. comparisons with snow thickness data from these regions) that such extrapolation is justified. For example, the assumption that meteorological variables which were found not to be important at Niwot Ridge will be unimportant in these other locations needs to be justified. Snowpacks in these other regions are subject markedly different temperature, humidity, and wind regimes, and are well known to belong to sufficiently different behavior classes as to merit classification as entirely different snow types (e.g. Sturm et al., 1995). Additionally, the heat fluxes discussed depend on the current state of the snow cover, which is a product of all snow accumulation and redistribution events that have occurred. However, the article considers only single snow redistribution events and does not address how these might be impacted by the preexisting snow surface and how multiple redistribution events' impacts would accumulate to produce a snowpack. Finally, the article as a whole would benefit from some greater attention to organizing. The reviewer felt that results, methods, and background information are interspersed throughout. There is no discussion and the conclusions do not include any key findings from the research or their significance. A bit of rearranging is in order.

Overall, the goal the authors seek - to provide modelers with a quantitative relationship between snow distribution and (e.g.) wind speed - is an excellent goal. MUCH more work and validation would be required before a conclusion on the nature of this relationship would be publishable. The authors appear to have a choice - they could try to publish the existing work with a greatly altered take-home message (e.g. this is what impacts snow distribution during a single storm in prairie environments like Niwot ridge), or they could seek to address their stated goal. However, the reviewer wants to very clearly state: a quantitative generalized relationship between snow distribution and environmental variables would not be publishable without validation in ALL of the climates the relationship is applied to. Thus, years of work are likely required to address the stated goal.

The reviewer recommends that the authors revises the paper to reflect the work done and its available implications. Doing so demands they quantitatively validate the spatial distributions of snow thickness predicted by their model, revise the article's scope so as not to extrapolate. If measurements of snow thickness at Niwot Ridge are not available, there are numerous datasets containing measurements of snow thickness and meteorologic conditions available at Arctic Data Center and other repositories. The key contribution that the presented work makes is to show that a new model, Rescal-snow, might accurately represent snow bedform formation and how these bedforms impact heat flux. The latter can be demonstrated by showing how snow bedforms impact heat flux at Niwot Ridge and other similar locations. Demonstrating that the model makes quantitatively accurate predictions of spatial distributions of snow thickness on sea ice and polar ice caps will require a substantial additional body of work, but such work would be nicely motivated by a paper showing the method works at Niwot Ridge. Finally, the article should be reorganized throughout to clearly defined sections: background, datasets used, methods (including procedures for validating methods), results (including validation and uncertainties), discussion, and conclusions. The revised article with quantitative validation of snow thickness and limiting the scope to not extrapolate could be original, significant, and rigorous. The reviewer therefore recommends the paper be rejected, with a heavily revised version invited for resubmission.

Specific comments by line number:

22: The references here do not align with the sentence. Petrich et al showed that dunes controlled the locations of melt ponds early in the melt season. Liston et al simulated the melt season but did not compare with and without snow dunes. They did not show dunes "control" melt. Popovic et al studied modeling the spatial distribution of melt ponds but did not investigate the impacts on ice melt.

32: The reference Comola et al does not discuss the relative erodibility of snow vs. sand and the reviewer does not believe the statement that snow is more erodible than sand is always true. Sintered snow surfaces will not erode in wind speeds that would erode sand dunes.

60-61: Figure 1 does not show anything as a function of wind speed and time.

65: In line 51 the authors state that the wind was unidirectional at the field site. Hence the justification for excluding wind direction seems to be a product of the specific field site, not a generally applicable feature of snow redistribution.

68: Are these examples all of the data for temperatures below -1 C? If not, how were the data subsetted?

70: How is the uncertainty in the classifiers used?

99: Natural surfaces rarely start flat and bare, is this model only applicable and tested in the case of flat surfaces? How would preexisting surface roughness impact the results?

103: How is the upwind boundary handled? If the authors are allowing blowing snow to

exit the downwind boundary but not providing a source of blowing snow at the upwind boundary then the simulated domain would have too little snow and distributions would change as a function of distance from the boundary.

107: How are the maximum unstable wavelength and the saturated flux of blowing snow grains observed? Authors do not describe measuring them in the observations section. In general, the article does not contain enough details about the data collection to assess whether there is actually enough data to support the claims.

109: Why choose snow grain density of 800 kg/m^3? Snow grains are pure ice so ~900 would be a more typical value.

110: Where does the surface roughness length come in? I thought this was a flat, bare surface? Or if this is the surface roughness of the snow covered surface? Shouldn't that evolve with the snow cover? Details are needed.

116: Which measurements are authors matching lengths and heights to? As discussed above, authors need to quantitatively show how well the spatial distribution of snow thickness produced by this model matches real, measured spatial distributions of snow thickness. Additionally, authors state these uncertainties here but don't go on to discuss their impacts on your results. Reviewer would think an uncertainty of ±50% would have a large impact in the confidence of the results.

117: Other scientists and modelers need to use the results in dimensional units and authors use dimensions in equations and figures throughout the article. Reviewer does not see how presenting results in non-dimensional forms reduces the impacts of uncertainties

for anyone who might to apply this work.

118: What does ‘sigma’ refer to here? Please define all symbols.

120: This statement raises significant questions about what exactly the model is doing and it’s validity. The difference between 5, 10, or 15 cm of snow on Arctic sea ice is large. As mentioned before, authors need to validate the snow thicknesses the model is producing and quantify the impacts of the uncertainty in the model on your results.

123: Authors have not mentioned sublimation or compaction so shouldn’t average snow depth be fully determined by the precipitation rate and time? Or is this discrepancy due to blowing snow and your boundary conditions? If so authors need to justify why the boundary conditions are physically reasonable in the locations you apply your model to. Revising the scope to Niwot Ridge only would greatly simplify this.

129: Why 35 cm thick ice? Due to high growth rates of thin ice, most of the ice in the Arctic is not 35 cm during the winter. This would be generally recognized by the sea ice community as an unreasonable value. The choice of such thin ice could exaggerate the importance of snow thickness variations. Also, this 10 cm water equivalent, ice equivalent, or something else?

131: References missing for these values.

134: Need to provide more details on Monte Carlo simulation. Also some organizational housekeeping - reviewer expects to find these uncertainty values in the results section, not before the relevant equations are presented.

140: 35 cm thick ice would be growing rapidly in the Arctic unless air temperatures were very warm. The use of a steady-state model here needs to be justified or ice thickness needs to be changed.

146: Volume scattering is the dominant process by which shortwave radiation interacts with snow. So sea ice covered by snow with thickness of less than approximately 10 cm will have an albedo intermediate between that of pure snow and bare ice. Given the snow thicknesses you describe (earlier the article describes 2 cm thick snow patches) this effect should be included. The Delta-Eddington parameterization used in Icepack (Briegleb and Light 2007) would be an appropriate place to start.

Figure 3 (a): Earlier authors describe snow patches as having a thickness of approximately 2 cm. However, at wind speeds of 1.3 snow dunes that are clearly greater than 2 cm in thickness. Please resolve the inconsistency when validating the snow thicknesses produced by your model.

167: Inconsistent with figure 3. In the figure, the orange lines appear to indicate taller dunes than the purple lines.

172-194: A model developed exclusively from data at Niwot Ridge, Colorado should have

considerable justification and validation (including comparison with snow thickness observations) for why it's snow thickness predictions are valid in this range of locations and conditions. Based on the data presented, this section must be completely removed and is flatly unpublishable. Focus the article on heat fluxes at Niwot Ridge and similar locations that are supported by data.

192-3: Inconsistent with figure 3 again. Largest dunes appear in figure to be at highest wind speeds and precipitation rates.

Figure 4: Reviewer cannot tell what they are supposed to learn from this figure. There are no color scales. Subfigure c does not seem related. The caption refers to (f), which is not present.

195-207: The article may not be published presenting these equations in this way, without any assessment of their uncertainty or generality. Reviewer recommends removing this section altogether. If it remains, it must include a clear description of under what conditions these equations are applicable (including the context of the data they depend on) and rigorous assessment of their uncertainties. The paper must clearly assert the major limitations of this study and may not imply that the equations can be used by modelers to represent conditions far beyond those the equations were derived or validated on.

209-216: This conclusion does not state any key findings from the research, their significance, or next steps. The article also lacks a discussion section. The reviewer feels that organizing around a more traditional format would benefit the paper.

References:

Briegleb, B. P., , and B. Light, 2007: A delta-Eddington multiple scattering parameterization for solar radiation in the sea ice component of the Community Climate System Model. NCAR Tech. Note TN-472+STR, 100 pp.