

The Cryosphere Discuss., referee comment RC2  
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## **Comment on tc-2021-205**

Anonymous Referee #2

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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-RC2>, 2021

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The manuscript "Snow dune growth increases polar heat fluxes" by Kochanski et al. discusses the role of snow dunes (which form under the influence of snow transport by wind), on the heat flux through the snow cover. Based on an extensive dataset acquired at a field site in the Colorado Rocky Mountains, it was demonstrated how wind speed during snow fall, as well as time after snowfall explains the snow surface forms found in the dataset. Using the Rescal-snow model, snow redistribution and dune formation is simulated. These simulation results are used to calculate conductive fluxes through the snow cover with spatially variable thickness, and it is then compared to the value for a uniform distribution. Similarly it is analyzed how the radiative fluxes are altered when snowfall is organized in dunes instead of a uniform snow cover. Layers with low albedo may be exposed for longer when snow is organized in dunes, leading to larger radiative fluxes.

In my opinion, the topic addressed is highly relevant, as the authors discuss in the introduction: in large areas on earth with a snow cover, snow will not accumulate in uniform layers, as is often assumed in modeling approaches, but in some form of bed form due to snow transport by wind. The authors have created some very valuable data and some interesting avenues for analyzing the data. The goal to provide results that can be incorporated in large scale land surface schemes is commendable.

Nevertheless, I found that the manuscript used an unnecessary simplistic approach to address the question how snow bedforms impact energy fluxes. Many factors are not taken into consideration, and some are not even discussed. I think those simplifications severely limit the usefulness of the results to the broader scientific community.

I think the key components that at a minimum deserved discussion, but even better would be taken into consideration in the analysis:

1) Ultimately, it is the energy balance that matters. So the assumption that the difference between the temperature at the surface and at the bottom is constant for scenarios with varying heat fluxes (implied in L177-178) is not justifiable. If the surface layers consist of low density snow, with low thermal conductivity, the surface temperature decreases to compensate for the lower heat flux through the snow, which in turn will increase the heat flux until balance is achieved. It would probably require some simple 1-dimensional thermodynamic modeling to quantify these effects.

2) The role of wind speed on snow density during accumulation is not discussed, but a flat snow surface, which built during low wind speed conditions, will have lower density than when dunes are present. Density will generally increase with wind speed (see for example Groot Zwaaftink et al. (2013)). The density will strongly impact the thermal conductivity of the snow layers (see for example Fig. 1 in Calonne et al., 2011). Some data on this has been published and could/should be incorporated in this analysis. In fact, the density of surface layers varies with surface topography. See for example Fig. 5 in Weinhart et al. (2020) and Fig. 8 in Wever et al. (2021). The situation is highly complex, because with lower wind speeds, low density snow may fill the areas in between the dunes, resulting in low density snow with low thermal conductivity, compensating for the shallower snow depth in these areas.

3) The role of aerodynamic roughness length is not discussed. When reading the title, I actually immediately thought that that was an important impact of growing dunes on heat fluxes. See for example Amory et al. (2017). So I was a little surprised that this was not discussed at all.

I also found it a questionable simplification to focus the analysis mostly on an extreme case with 35 cm sea ice with 10 cm snow on top, with sea ice having very low albedo. This case has little relevance, for example, for firn on ice sheets, where the firn layer can be 100 m deep and albedo variability is smaller (for areas without melt). So I do not expect the results presented in Fig 4e for shortwave radiation to hold for locations like South Pole, Greenland Summit and W. Antarctica (as is kind of suggested in Fig. 4c).

Another major issue is that I do not comprehend how the effect of the uncertainty in snow thermal conductivity only impacts the conductive heat flux by 1% (L135).  $K_s$  set as 0.3 +/- .15. If we take equation 1, and determine the ratio  $Q_c(k_s=0.45)/Q_c(k_s=0.15)$  (i.e., +/- 1 standard deviation), that will give a factor 3 in conductive heat flux (assuming only the snow part). I think it is pertinent in such analyses to take the effect of wind speed on snow density, and thus thermal conductivity into account. Note that this will be supportive of the claim that heat fluxes increase with dune growth, since higher snow density with higher wind speeds results in higher thermal conductivity, generally spoken. On top of that, the thermal conductivity of snow is not only a function of snow density but also grain types (Fig. 1 in Calonne et al., 2011). There will be a systematic bias with respect to the analysis here. For example, a shallow flat snowpack, which must have low density, can easily form depth hoar with low thermal conductivity. Such processes at a minimum should be discussed in the manuscript.

Unfortunately, I actually think that the revisions needed for this manuscript are more than typical for "major revisions".

Minor comments:

1) L131  $k_s$  and  $k_i$  are not explicitly defined, but I assumed this was thermal conductivity for snow and ice, respectively. Please make sure that all symbols are properly introduced.

2) Caption fig. 4: "e-f" -> "d-e".

3) The conclusion section is hardly adequately summarizing the conclusions from the study and needs to be completely rewritten.

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