Review of manuscript tc-2020-182

Summary

This paper investigates snow cover duration and its temporal trend at 4 well-instrumented snow sites, using observations and a set of snow models which participate in the first phase of ESM-SnowMIP. To further understand the models' sensitivities at these sites, a simplified model depending only on two parameters is also used. The authors show that there is no statistically significant trend in snow cover start dates, whereas there is a significant trend in snow cover ending dates at two mountain sites. The simplified model shows that the magnitude of trends in snow end dates increases for simulations with end dates occurring at a later time in the year, and that a feedback mechanism exists between snow cover ending dates and the timing of warming. The same behaviour is found at one of the sites in the ESM-SnowMIP simulations.

I found the paper well written, with a clear and logical presentation of the methods and results. Most of the conclusions outlined are supported by the results presented (see below in the specific comments). The use of the simplified model allows to better understand the interplay between snow duration and trends, and their dependencies from the turbulent exchange coefficient and timing of warming. As a minor comment, I found a few sentences in the "Results" and "Conclusions" sections possibly difficult to read, and I encourage the authors to rephrase them (see specific comments below). Also, I appreciate the fact that only results that are statistically significant are fully interpreted by the authors, however reducing the number of sites used to draw some of the conclusions. In short, I think that the manuscript should be published after minor revisions.

Specific comments

Ln 127-128: I found this sentence a bit difficult to read. Could you reformulate it to make it clearer, as it seems an important point of the discussion?

Ln 162-165: This sentence is not backed up by evidence, or at least a reference to previous works, even though it is very reasonable. However, because it seems an important sentence to support the feedback argument explained by the authors in the following lines, I would suggest adding a plot (similar to Fig. 2) showing the annual cycle of the energy fluxes, or at least a reference to the literature.

Ln 188-190: I found this sentence too general and not specific enough. Could you please expand it, being more specific on the relationships between snow ending date, timing of warming and strength of aerodynamic coefficient, as found by the 2PM simulations?

Ln 188-197: A limitation of the present study is that these conclusions are derived mainly from one site. More sites, or global land-surface only simulations, are required to evaluate these conclusions on a larger range of climate conditions. I think the authors should state this aspect in a clearer way in the conclusions.

Minor comments

Ln 10: “should be able” → “are able”?

Ln 54: “atmosphere model” → “atmospheric model”?

Ln 54: Please add a reference to GSWP3 and the “Global Spectral Model”

Ln 71-72: sensible heat and moisture fluxes are between the surface and the atmosphere, not only from the surface to the atmosphere.

Ln 85: Please, add here how snow start and end dates are computed.

Ln 89: Are Lyon and Boise CRUTS locations as well?

Ln 143: “have trends at lower end” : is it the “higher end”, right? Or “lower” in absolute value.

Ln 146-147: is this sentence implying a comparison with Fig. 2? Is so could you please state it in the text?

Ln 149: could you please specify that you are referring here to the observed snow cover?

Ln 172: I would clarify that only four of the ESM-SnowMIP sites are used in the study.

Comments on figures

Figure 5: is the quantity on the x-axis the mean annual snow cover duration for each model?