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# *Interactive comment on* "Multi-scale snowdrift-permitting modelling of mountain snowpack" *by* Vincent Vionnet et al.

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The work of 'Multi-scale snowdrift-permitting modelling of mountain snowpack' by Vionnet el al. deals with the spatial and temporal evolution of snow cover in high mountain areas. The study focuses, as clearly mentioned in the well-structured introduction, the (i) added value of a wind downscaling approach, (ii) the role of lateral snow redistribution, and (iii) the use of remote sensing data. For this purpose, the authors developed a model chain that combines established models and parameterizations. This research design was applied and validated for the Kananaskis Valley in the Canadian Rockies.

The research priority of the study is nicely summerized in the introduction and shows the reader the scientific challenges in this research area. These questions are taken up throughout the paper and are finally answered in the conclusion. The description of the methods is a little sparse in some parts, but with the given references it can be easily followed and reproduced by an interested reader. Since these are well established methods and approaches, I think that no further work is necessary. Only the wind downscaling approach raised some questions which can be answered with little effort (see comments below).

The model experiments based on a stepwise model falsification are well thought out. However, abbreviations were not catchy for me and led to confusion and I had to scroll back and forth to check with Table 2.

The results of the downscaling and snowpack simulations are well structured and show sufficiently the strengths and weaknesses of the different approaches and experiments. In the subsequent discussion these results are put into context. For me as a reader all questions that came up in the beginning were answered sufficiently. Also nice is chapter 4.4 where the limits of the approach are discussed.

In summary I think the work fits well to 'The Cryosphere'. The structure follows the classical structure and is easy to understand for the reader. Furthermore, I don't see any concerns in the technical realization and the conclusions. These are also supported by good illustrations. Based on this reviwer, I recommend the publication of the study with only minor revisions.

More specific comments

Section: Atmospheric Forcing

P7L212: Precipitation plays a particularly important role in snow dynamics and is difficult to capture in most applications. I don't doubt that the HRDPS sufficiently accounts for the large- scale precipitation effects on average, but don't the strong topographic variations lead to strong subgrid-scale gradients (< 2.5 km), which in turn reduces the variability on the small scale?

Section: Wind field downscaling

The general downscaling approach is comprehensible, and the combination of a wind library and transfer function seems to be reasonable. While reading through the section I asked myself at some points why the following steps were implemented in that way:

(i) Diagnostic wind models are computationally efficient. This efficiency would allow for separate simulations for each time step. Why not following this approach?

(ii) As far as I can see the wind velocity at 40 m above ground was set to 10 m/s for each simulation. Why weren't different wind classes introduced here? In my understanding the background wind has a significant influence on the flow features (e.g. flow separation, gap flow, bluff body formation etc.). Have you checked different boundary conditions?

(iii) As in the study by Barcons et al. (2018) the characteristic length, L, was set to 1000 m. How was this length determined? Do we not expect very different lengths for different topographies? How sensitive are the simulation to this length scale?

(iv) Are the wind fields still mass consistent when two micro-scale wind fields are linearly interpolated? Maybe a mass correction might be necessary.

(v) In the WindNinja model a spatially constant roughness length was assumed, which is due to the nature of the model. Later in the same paragraph it is described that the prognostic wind velocity at 10 m takes into account the interaction with the vegetation by adjusting the logarithmic wind profile. I doubt that surface properties are homogeneous at a horizontal resolution of 30 m. Wouldn't it be useful to consider surface properties of a defined upstream fetch when adjusting the wind speed?

(vi) The fact that mass-consistent models cannot represent flow separation and other flow features is the major deficit of such models. The approach to adapt the transfer function using the Winstral parameter seems to be a good way to start. I just wondered why a value of 0.25 was used for the transfer function. From a fluid dynamic point of view, flow separation zones usually lead to a flow reversal and not to a reduction of the wind speed. Maybe the simulations could be improved by a dynamic value.

(vii) Due to the limited number and location of stations, there is no real evidence that downscaling leads to an improved characterization of the wind field. However, this could be shown by the means of the snowpack simulations and the comparison with the ALS and Sentinel data. To be more concise, I would recommend a Experiment using the HRDPS simulations directly with the snowdrift scheme and recirculation parametrization (see comment below).

Section: Snowpack simulations

It would be interesting to run the snowpack simulations without wind downscaling but rather drive the snow drift module and recirculation parametrization with the HRDPS fields (without WindNinja). I think it would be helpful for the community to see the importance of high-resolution wind fields.

Minor comments

P3L76: You need commas berfore and after ‘inspired by Ryan (1977)’.

P6L174: The abbrevation ‘PBSM-3D’ has not been introduced.

P14L443: As mentioned in a previous comment it would be useful to correct the HRDPS precipitation.

P16L496: Are these correlations significant?

P17L538: Maybe I missed something, but there is no experiment where the sensitivity of snow drift simulations in CHM is shown without the WindNinja fields.

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