Answer to Tobias Sauter TC-2020-187

We thank Tobias Sauter for his comments. We provide our responses to his comments and describe how we addressed them in the revised manuscript. The original reviewer comments are in normal black font while our answers appear in blue font.

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.

The names of the model experiments have been modified in Table 2, in the text and in the figures. We hope it will reduce the confusion mentioned by the reviwer.

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?

As mentioned by the reviewer, the configuration of CHM used in this paper does not account for the spatial variability in snowfall amount at spatial scales below 2.5 km (the HRDPS resolution) which impacts the simulated small-scale variability of snow depth. At these scales, the spatial variability in snowfall amount results from: (i) snowfall enhancement caused by the interaction of the flow field with the local topography and local cloud formation processes, such as seeder-feeder mechanisms and; (ii) pure particle flow interaction (preferential deposition of snowfall) (e.g. Mott et al., 2018). So far, these local processes have been previously studied using computationally expensive 3-D atmospheric models at high-resolution (below 50-m) that can explicitly simulate these processes (e.g., Mott et al., 2010;

Dadic et al., 2010; Vionnet et al., 2017; Gerber et al., 2019). In the context of 2D distributed snowpack modelling, such processes cannot be directly simulated. Two main approaches can be tested: (i) a precipitation adjustment function depending on the differences between the elevation of the 2.5 km model and the elevation of the high-resolution CHM mesh (Thornton et al., 1997; Liston and Elder, 2006) and; (ii) a parameterization for preferential deposition of snowfall (Dadic et al., 2010).

The precipitation adjustment function was tested by Vionnet et al. (2019) where they downscaled NWP forecast from 2.5 km to 500-m grid spacing in the French Alps. They showed poor performances at high elevations that can be partially related to the value of the precipitation-elevation adjustment factor used in Liston and Elder (2006). The same method was tested to downscale the HRDPS precipitation amount to the high-resolution CHM mesh over the Kananaskis domain. The impact on the simulated snow depth as a function of elevation for three sub-regions is shown on Fig. 1 below. The CHM simulations shown on this figure do not include blowing snow and gravitational redistribution. Introducing the adjustment factor leads to a continuous increase in snow depth as a function of elevation. In particular, compared to the default HRDPS precipitation, larger snow depths are found above 2300 m which correspond to areas that are higher than the HRDPS grid. This shows that accounting for sub-grid effects on snowfall amount can strongly impact the elevation-dependency of snow depth.
The precipitation adjustment function was initially developed to generate a distributed precipitation field accounting for topographic effect from sparse measurements in mountainous terrain. However, the degraded results shown in Fig 1 from including it suggest that it may not be suitable for capturing sub-grid effects within a 2.5-km grid. Therefore, we did not include this correction due to the additional uncertainty that it would introduce.

The parameterization of preferential deposition of snowfall proposed by Dadic et al. (2010) requires estimations of the horizontal velocity as well as the vertical velocity (Eq. 2 in Dadic et al. (2010)). The estimation of the vertical velocity is not included in CHM, and so that this parameterization cannot be implemented. Obtaining the vertical velocity from WindNinja simulations could be useful to drive this parameterization in future studies.

The following sentences were added in the revised manuscript:

- Section 2.2.3: "T*he precipitation adjustment function of Liston and Elder (2006) has been tested but it led to strong overestimation of snow depth at high elevation (not shown), suggesting that this factor may not be adapted to account for the subgrid variability of precipitation amount within a 2.5 km grid."*
- *Section 2.2.2: "CHM does not simulate explicitly preferential deposition of snowfall (Lehning et al., 2008; Mott et al., 2018)."*
- Section 4.4: *"The parameterization of Dadic et al. (2010) could be tested in CHM but would require an estimation of the vertical wind speed that could be provided by WindNinja"*

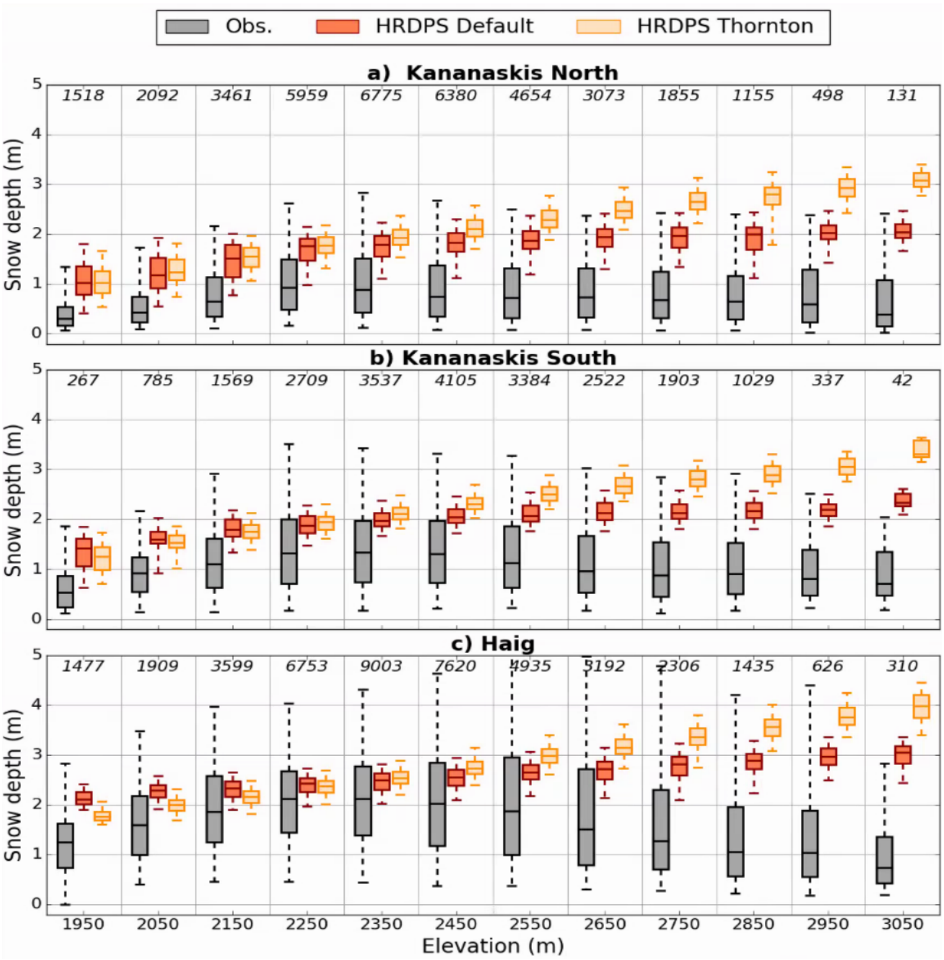

Figure 1: Boxplots showing the distribution of observed and simulated snow depth per 100-m elevation bands for three sub-regions and two CHM simulations. HRDPS default: no correction of the precipitation amount; HRDPS Thornton: Adjustment of the precipitation amount using the correction factor proposed in Thornton et al. (1997).

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?
Wind Ninja is a computationally efficient wind model compared to a more complex model such as a CFD-model or an atmospheric model in Large Eddy Simulation mode. Nonetheless, in the context of this work, running CHM over a full snow season at an hourly time step requires 8760 (24*365) distributed driving wind fields. In a different study (in preparation), WindNinja was used to downscale separately the 8760 low-resolution HRDPS wind fields to 50-m resolution over the 1000 km^2 of the Kananaskis domain following the method described in Wagenbrenner et al. (2016). It took almost 19 days of wall-clock time whereas the downscaling method proposed in our paper took 4.5 hours of wall-clock time. Therefore, our approach brings a substantial improvement (100x) in computational cost. An article comparing the two downscaling methods is in preparation.

(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?

In the initial development of the downscaling method, WindNinja simulations were carried out over the Kananaskis domain using different input wind speeds. The idea was to generate a wind field library containing different large-scale wind speeds as in Mott et al. (2010). However, results showed that very similar transfer functions (Eq 1 in the paper) were derived from these different WindNinja experiments. We suspect that it results from a linear behavior of the WindNinja solver. For this reason, only one input wind speed was used when building the wind library as in Barcons et al. (2018).
Using different wind classes would be highly relevant if a mass- and momentum-conserving model was used to build the wind library. Indeed, such a model would be able to simulate significant flow features that depend on the intensity of the background wind, in particular the formation of recirculation zones and their spatial extension.
Information we have added in the revised manuscript on this subject:

- Section 2.2.4: "*Only one value for the initial wind speed was used to build the wind library due to the insensitivity of the transfer function to the initial wind speed found with WindNinja.*"
- Section 4.3: "*Improvements in the wind downscaling could be achieved using such models to generate the library of wind fields, as proposed by Barcons et al. (2018). Different conditions of atmospheric stability could also be considered (e.g., Gerber et al., 2017) as well as different input wind speeds that affect significant flow features such as flow separation.*"

(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?
Barcons et al. (2018) determined that a circle of radius of 500 m (averaging area of 0.78 km^2) gave the optimal performance (RMSE, Skill) for their downscaled wind field when compared to wind mast observations. They downscaled WRF output at 3-km grid spacing over complex terrain in Mexico. This resolution is similar to the 2.5 km resolution of the HRDPS used in our paper. For this reason, we decided to use a similar averaging area than Barcons et al. (2018). For computational reasons, we decided to adopt a square instead of a circle for the shape of the averaging area and ultimately selected an area of 1 km^2 (characteristic length, L = 1 km).

We fully agree with the reviewer that the optimal value of this characteristic length certainly depends on the complexity of the topography as well as the initial resolution of the input wind field. Indeed, the maximal value for the characteristic length is the resolution of the input wind field since above this value the transfer function starts including features that are already resolved in the input wind field. Conceptually, this characteristic length should be large enough to cover the distance between the main sub-grid topographic features that are not captured in the input wind field. High-resolution wind simulations (for example at 50-m resolution) could be used to study the dependency of the characteristic length on the complexity of the terrain.

The choice of characteristic length, L, influences the spatial variability of the wind speed. As L increases, the transfer function incorporates the local wind fluctuation induced by the micro-scale terrain features. Figure 2 shows an example of near-surface wind field obtained when downscaling the HRDPS wind field with three values of L (0.5, 1 and 2.5 km). By construction of the downscaling approach, the wind direction is not influenced by the value of L. On the contrary, the differences between the downscaled and the HRDPS mesoscale wind speed (shown on Fig. 3 in the manuscript) depend on the value of L. The smaller the value of L, the more the downscaled wind speed coincides with the HRDPS wind speed. In contrary, as L increases, the transfer function includes more local wind fluctuations around the HRDPS mesoscale wind field. This can be observed around ridges where the downscaled wind speed is larger with L = 2500 m than with L = 500 m and 1000 m.

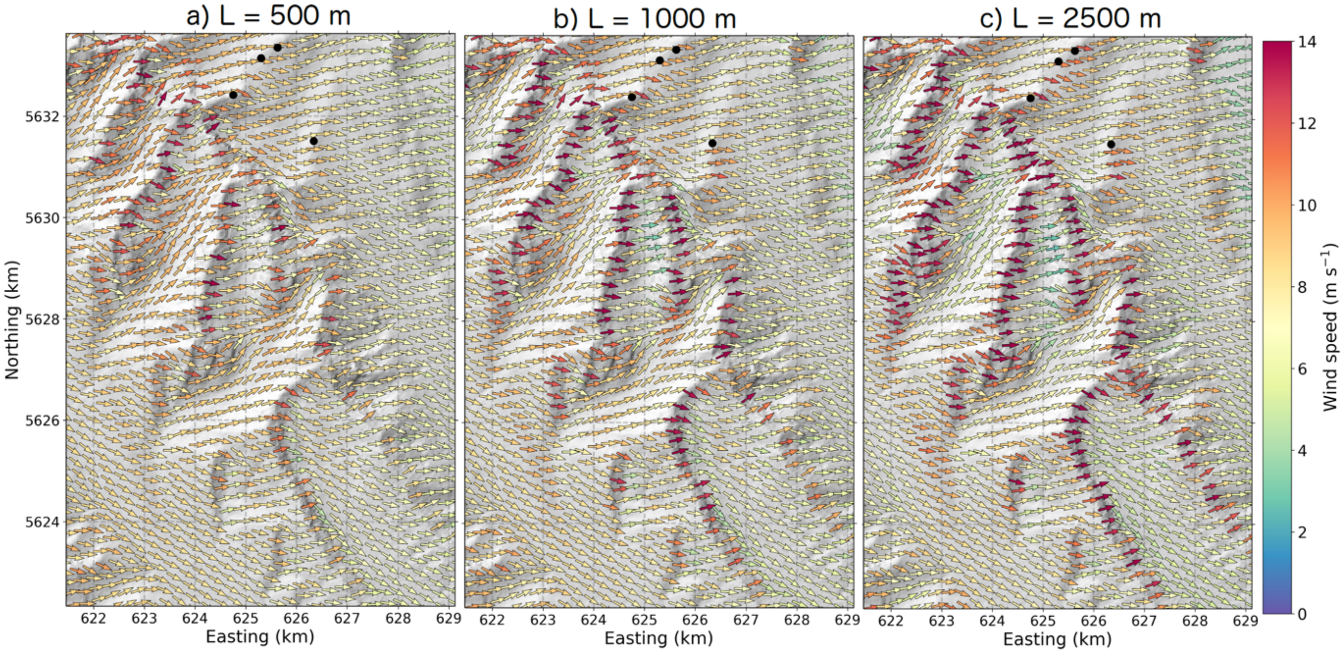

Figure 2: Near-surface wind field on 10 September 2017 at 18 UTC from HRDPS downscaled to the CHM mesh using WindNinja (HRDPS+WN). The parameterization for the formation of recirculation zones on leeward slopes is not used here. Three values for the averaging length, L, are tested to compute the transfer function: (a) 500 m, (b) 1000 m and (c) 2500 m.

Based on Fig. 2, we can expect a strong impact of the characteristic length on the simulated snow redistribution in the upper slopes. This was not investigated in the context of this paper since we focused our study on the impact of process representation on snowpack simulations at snowdrift-permitting scales and the development of a relevant evaluation framework. A future study on the impact of characteristic length on snow redistribution at the mountain range scale would be highly relevant. The following sentence notes this sensitivity in the revised manuscript (Section 4.3):
"Sensitivity tests revealed that the wind field in the upper slopes strongly depend on the value of the radius of influence with a potential large impact on simulated snow redistribution."

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

This downscaling approach does not include a mass term. There is no inward or outward flux of air from a cell. This is true for the method detailed here, as well as other 'speed-up' style approaches such as Liston and Elder (2006), Essery (1999) and Barcons et al., (2018). Certainly, the inverse problem could be constructed so-as to determine what initial condition would be required from WindNinja to produce the final output. However, this would almost certainly be numerically ill-posed. The approach detailed here is designed to be an approximation to the underlying mass conserving CFD simulation, and so if such an inverse problem were constructed the velocity field would almost certainly show some discrepancy in mass, i.e., it would not be divergence-free. Specifically, the mass conservation can be affected at two stages due to interpolation:

- Prior to the simulation, when applying the rasters of the wind library (u and v winds components, transfer function) to the triangles of the unstructured mesh using the *mesher* code (Marsh et al., 2018).
- At CHM runtime, when linearly interpolating and recombining the selected microscale wind components including the local terrain effect to obtain the downscaled wind direction

If a mass correction was applied, it should be done at runtime on the unstructured mesh used by CHM. This could substantially increase the computational time of the simulation and remove part of the benefit of the downscaling approach. That is, it would almost certainly be more accurate and more correct to track the flux of mass across computational cells, which would imply running the full CFD model.

(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?
We agree that they are limitations in the representation of the interactions between the near-surface wind field and the vegetation in the downscaling approach proposed in our study. So far, fetch effects due to the presence of upstream vegetation are not taken into account when adjusting the wind speed. But they are included in the blowing snow redistribution scheme as described in Marsh et al. (2020). The mass concentration in the saltation layer is reduced in regions where flow is developing. PBSM-3D would benefit from a more accurate representation of the wind speed in these regions. The best solution is certainly to account for a spatially variable vegetation cover (and associated roughness) directly in Wind Ninja.
Information have added in the revised manuscript on this subject:

- Section 2.2.2*: "Upwind fetch is calculated for each triangle of the mesh using the fetchr parameterization of Lapen and Martz (1993)* ***and is used to reduce the mass concentration in the saltation layer in regions where flow is developing****."*
- *Section 2.2.4: "Wind speeds were then adjusted to 10-m wind speeds using the Prandtl-von Kármán log-linear wind profile and modified to include vegetation interactions using the vegetation cover of the triangle as defined in Sect 2.2.1.* ***Fetch effects due to the presence of upstream vegetation are not taken into account 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.
The constant value of 0.25 used in this study in based on the initial developments of Winstral et al. (2009). It was taken as the average value from Eq. 6 in Winstral et al (2009) for values of Sx between 21.5 and 30° that characterize the reduction of wind speed found for this range of values of Sx.

We agree with the reviewer that flow separation zones usually lead to a flow reversal (e.g. Raderschall et al., 2008; Gerber et al., 2017). However, the wind speed in reversal zone is usually lower than the wind speed at the crest. The transfer function using the Winstral parameter aims at capturing this effect. The recent study of Menke et al. (2019) has measured the ratio, R, between the maximum wind speed in recirculation zones and the wind speed in the inflow at a crest. This ratio is similar to f_{down} used in our downscaling method to reduce the wind speed in areas prone to flow recirculation. Figure 10 in Menke et al. (2019) shows how R depends on the Richardson number (used to quantify the atmospheric stability). Their results show that R typically ranges between 0.1 and 0.5 for unstable atmospheric conditions and between 0.05 and 0.35 for stable atmospheric conditions. R tends to decrease with increasing stability in a stable atmosphere. As reported by Menke et al (2019), ratio of less than 0.3 are observed for wind speed greater than 12 m s^{-1}, characterized by neutral or slightly stable atmospheric conditions. The study of Menke et al (2019) is mentioned in Sect. 4.3 in the revised version of the paper:

"A constant value of 0.25 is used for the transfer function in recirculation zones. This value falls within the range of values reported on Fig. 10 of Menke et al. (2019) for the ratio, R, between the maximum wind speed in recirculation flow and the inflow wind speed at the crest. Menke et al. (2019) found that R tends to decrease with increasing stability in a stable atmosphere and it presents values lower than 0.3 for inflow wind speed greater than 12 m s^{-1}. This suggests that a dynamic value based on atmospheric stability could be used for the transfer function in recirculation zones."

The absence of modification of the wind direction in the recirculation zones was already discussed in the initial version of the manuscript and it was kept unchanged in the revised version of the paper.

(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).
We agree that the evaluation of simulations of snow redistribution driven by different wind fields using distributed snow observations provides only indirect information on the quality of the driving wind field. The reverse is also true and has been explored for alpine ridges by Musselman et al. (2015) and so the relationship between the quality of the wind field and the quality of the snow redistribution field is not straightforward. We used this method in our paper to assess the role and relevance of the parameterization of the wind speed reduction in leeward areas. However, we do not believe that a CHM simulation driven by HRDPS wind fields simply interpolated to the CHM mesh and including the recirculation parameterization will bring new results for the community. Indeed, such experiment would result in a spatially homogenous wind field (except on leeward area) that does not include the wind perturbations generated by the topography at the resolution of the CHM simulations. All the studies using snowdrift permitting models (mentioned in the introduction of our paper) included a minimal downscaling step to account for the effect of the local topography on the wind field at the resolution of the simulation. Indeed, the simulated snow transport and redistribution is a direct consequence of the spatial variability of the wind field (e.g., Musselman et al., 2015). Based on the literature, the minimal topographic adjustment than can be applied to the input wind field is the correction using terrain-based parameters implemented in Liston et al. (2007).
In our paper, the downscaling with WindNinja can be considered as a necessary step prior to any simulation of wind-induced snow redistribution with CHM. It would be relevant to compare the simulated snow redistribution obtained with the HRDPS+WN+Rc wind fields with the redistribution obtained with the Liston et al (2007) approach. This will be the topic of a future study.

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.
As discussed in our answer to the previous comment, the importance of high-resolution wind fields to drive snow-redistribution models has been already shown in many previous studies mentioned in the introduction (e.g. Gauer, 1998; Liston et al., 2007; Lehning et al., 2008; Bernhardt et al., 2010; Mott and Lehning, 2010; Schneiderbauer and Prokop, 2011; Sauter et al., 2013; Musselman et al., 2015; Vionnet et al., 2014; 2017). For this reason, we opted to not include the additional simulation recommended by the reviewer. Instead, we think that the next step to our study would be to extend the studies of Mott and Lehning (2010) and Musselman et al (2015) and to evaluate in detail the impact of different downscaling method and resolution on snow redistribution at the mountain range scale.

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

P6L174: The abbrevation ‘PBSM-3D’ has not been introduced.
The abbreviation is now defined at the beginning of the paragraph describing the drifting and blowing snow scheme implemented in CHM:
“CHM also includes a 3-D advection-diffusion blowing snow transport and sublimation model (Marsh et al., 2020a): the 3-D Prairie Blowing Snow Model (PBSM-3D).”

P14L443: As mentioned in a previous comment it would be useful to correct the HRDPS precipitation.
Please see our answer above about this topic.

P16L496: Are these correlations significant?
The correlations are signification and the p-values have been added in the revised manuscript.

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.

We use this sentence since we tested in our paper the sensitivity of the snow drift simulations to the parameterization of wind reduction in leeward areas. This sentence has been rephrased as follows: *"Results of blowing snow redistribution simulations in CHM were sensitive to the quality of the driving wind field,* ***in particular the impact of recirculation areas****, at the mountain range scale (> 100 km^2)."*

References (not included in the initial manuscript):

Liston, G. E., & Elder, K. (2006). A meteorological distribution system for high-resolution terrestrial modeling (MicroMet). *Journal of Hydrometeorology*, *7*(2), 217-234.

Menke, R., Vasiljević, N., Mann, J., and Lundquist, J. K.: Characterization of flow recirculation zones at the Perdigão site using multi-lidar measurements, Atmos. Chem. Phys., 19, 2713–2723, https://doi.org/10.5194/acp-19-2713-2019, 2019.

Thornton, P. E., Running, S. W., and White, M. A. (1997). Generating surfaces of daily meteorological variables over large regions of complex terrain. *J. Hydrol.* 190, 214–251. doi: 10.1016/S0022-1694(96)03128-9