The authors provided significant improvements to the manuscript by addressing most of the comments. The manuscript is also better introduced and contextualized. I still have a few minor comments and some technical corrections, described hereafter. It could be useful to carefully revise the English writing or make it proofread it by a native speaker.

Comments about the point-to-point response

(Citations of the response in red, my answers in black)

"l. 79: Why not also including the melt season?

The global objective of this work is to provide a realistic first guess of the snowpack structure in the context of SAR remote sensing signal inversion algorithm development. At relevant frequencies (Ku-band, X-band, C-band), the snowpack becomes opaque to microwaves when wet. This is why the study focuses on the accumulation period."

It would be worth mentioning in the text.

"Figure 2: I assume a typo ("VWS" for "VW")

Wind speed is referred to as VW everywhere in the SNOWPACK / MeteoIO /Alpine3D documentation. It stands for Velocity of Wind, as described in the official SMET format specification (https://meteoio.slf.ch/doc-release/SMET_specifications.pdf). This acronym was used everywhere in the manuscript out of homogeneity with the official specification."

The VWS acronym has not been corrected to VW ("VWS parametrized").

"l. 263: The observed altitudinal temperature gradient is the reflect of the lapse rate chosen for TA downscaling (l. 137). There is no proof here it is realistic.

This line has been rephrased as : First, the lapse-rate applied for TA downscaling and spatialization respects the general rule of thumb that TA should get colder with elevation."

The new formulation is also not very satisfactory. The fact that temperature decreases with elevation is not a result, but simply the direct consequence of the chosen lapse-rate.

"l. 270: What is the reason for simulating the snowpack in forested areas (in a remote sensing perspective), if the forest snow processes, which have a strong impact on the snowpack, are not represented?

We agree with the reviewer that there is limited interest in simulating the snowpack in forested areas in a remote sensing perspective. The difficulty of accurately modelling both the snowpack and radiative transfer under trees and snow makes for a particularly challenging problem. However, we have chosen to tackle the entire elevation range within our study area out of completeness, in order to assess how the subgridding framework is performing on the entire domain of the simulation."

This point remains quite unclear to me. As far as I understand, authors consider somehow "virtual open terrain" below treeline to cover a full elevation range. It should be more explicitly stated in the manuscript, together with motivations for doing so.

"l. 271-272: "The wind erosion effect on the snowpack is also well represented, as dominant winds are blowing from the South / South-West. As a result, the south aspect profiles show more defragmented grains (dark green) on the surface". I am not sure I understand this cause-consequence. As far as I understood, wind-induced snow transport is represented by a precipitation multiplier. Consequently, associated effects of snowdrift on snow microstructure are not represented. Or am I missing something? Please clarify.

The word "erosion" here has not been used appropriately by the authors and is certainly the cause of the misunderstanding. Lines 271 - 274 in the original manuscript have been modified as such: The wind effect on the snowpack is also well represented in the simulations. Indeed, dominant winds are blowing from the South / South-West, and as a result southern slopes are affected by stronger winds (fig. 7). In the SNOWPACK model, grain type is a function of dendricity and sphericity, two parameters governed by the temperature gradient within the snowpack. As the surface temperature is altered by surface winds, precipitation particles (lime green) on the south aspects tend to metamorphose faster into decomposing and fragmented precipitation particles (dark green) than in the northern aspects, especially in the alpine."

The grain type could be affected by many other parameters. In the present state, this conclusion is not sufficiently backed.

Comments about the tracked change manuscript

(Line numbers referring to the tracked change manuscript)

Abstract

l.3: "SWE retrieval". After deletion of the two previous sentences, we don't know anymore we're talking about satellites. Just mention quickly it's retrieval algorithm for remote sensing.

l. 7-9: "Automatic Weather Stations (AWS) are too sparse, and high-resolution Numerical Weather Predictions systems have a maximal resolution of 2.5 km × 2.5 km, which is too coarse to capture snow spatial variability in a complex topography." This statement is only applying to your study area which is not mentioned at this point.

l. 14-16: "profiles generated with Automatic Weather Stations data" is a bit unclear, please be more specific if it's simulated profiles driven by AWS data, or measurements. Similarly, rather say simulated profiles driven by raw HRDPS data.

l.19: SAR is not defined. On the contrary, you don't need to add the acronyms "(AWS)" and "(OGS)" there if you don't reuse them in the abstract.

1. Introduction

l. 54: you mention the HRDPS model but we don't know yet you're working on Canada. You could mention for example: "HRDPS, performing numerical forecasts at 2.5 km resolution over Canada (…)" or something like that.

l.57: the acronym NWP has not been explained at this point.

l. 57-94: The addition of a more complete literature review in the introduction is appreciated. However, the writing of this new paragraph could sometimes be clearer and more concise. There are many details which are not so necessary about some studies in the literature review (e.g. the part about the CHM model). The literature review should highlight the specificities of these studies regarding the focus of the present study and potential differences of methods. For example, how CHM snowpack simulations were compared to LIDAR data is not relevant in the context of a paragraph focusing on the atmospheric downscaling.

L. 80: missing reference.

2. Study area

l. 126: I count 8 AWS in the Park on Figure 1. Also please mention you only use six of them in your study area, as in Table 1.

3. The Numerical Weather Predictions downscaling processing chain design

l.148 : for clarity, perhaps you can add: "These parametrizations are described hereafter."

l. 167: rather say you downscale ILWR through a correction using the lapse rate highlighted by Marty et al. (2002). They did not literally introduce a correction.

l.195, and every other occurrence: precipitation (no s)

l. 200: repetition of "affecting"

l. 211: "whole"

l. 254: "tends"

4. Results

l.348 and 350: "AWS-SNOWPACK" should be used instead of "Station-SNOWPACK" I assume?

Figure 10 is not so easy to read: in particular, what are the x axis legends? Pixels "names"? Maybe the authors could think of a more "reader-friendly" figure conveying the same message.

5. Discussion

l.433: "whole"

l.433: "idealized" is probably not the best choice of word for a real site.

l. 431-435: more specifically, what parameterization of SNOWPACK are you talking about, and how could it vary with elevation?

l. 444: “HRDPS-SNOWPACK simulations”

l. 444-445: repetition

l. 454-455: unit error? The SWE bias is more likely in mm than cm.

l. 465 and 467: once again, is it really cm when talking about SWE (usually expressed in mm or kg/m^2)?

6. Conclusion

I would not repeat the research questions, but simply their answers with 1/2/3.

l. 512: “atmospheric variables”