

It seems the author have put a lot of effort in the revised version of the manuscript – this is very appreciated. I think particularly the methodology section was much improved, which enhances the comprehensibility and potential reproducibility of the novel approaches described in the manuscript. Most of my comments are of minor but there is one larger issue that should be addressed/resolved. The page and line number refer to manuscript version 2.

Major comment

I'm puzzled by the sometimes very different elevation gradients in projected (absolute) snow cover change, which result from raw RCM data, bias adjustment and downscaling. E.g. in Figure S8, you show that for RCP8.5, largest absolute reduction in snow cover fractions are expected for elevations ~1000 – 2000 m, depending on the RCM-GCM combination, season and processing method (raw or different bias adjustments). In contrast, Figure 6 (rightmost panels) shows that absolute reduction in snow cover fraction generally increase with elevation. Both figures show bias adjusted data (without downscaling) – so I guess one of them must be wrong.

Minor comments

L91: I'm not sure if really all biases are constant in time. I suggest to rephrase this sentence to: "these systematic biases seem to be predominantly constant across time, ..."

L98: "limitations of both approaches." → limitations of the high-resolution snow model are not discussed, I would thus rephrase it to "limitations of the presented method."

L161: I'm not sure if I understand this sentence correctly – with "future change estimates" you mean relative values, right?

L187: I would rephrase (or entirely remove) the sentence with "unavoidable" because you just propose in the following sentence how the imbalance could be resolved.

L196: "future meteorology" → "projected meteorological data"

L281: I'm confused by the term "non-strictly" because a "strictly monotonic relationship" would also guarantee a unique solution (as you show in the reply to referee 2). Maybe the term "monotonically non-decreasing function" would be less ambiguous in this context?

L294-L298: I was not able to fully understand what you mean by these lines.

L472: The ordering of the subsections 4.x is a bit odd. Normally, the evaluation/validation of the results is shown first before one discusses e.g. future projections (to underline the robustness of the results). This ordering is however almost reversed in your manuscript. You could consider reordering the subsections.

Figure 2: This figure is very helpful to understand the methodology. Could you increase the axis (tick) labels in panel (b) a bit – they are rather hard to read.

Figure S2: I'm not sure what you mean by "SCD summaries by elevation." Maybe you could rephrase that. Besides, I was quite confused by the x-axis labelling of panel (b) "pixels/grid cells". I first read it as "pixels per grid cells" but I think you mean "pixels or grid cells".