We thank Davide Bavera for his encouraging and constructive comments. We address his issues (in italics) below. Changes in the manuscript are given in blue, answers to the reviewer are given in black.

1) *I would better explain and describe the selection procedure of the test sites where the method has been applied and possibly I would add a few more case studies in order to give more generality to the paper outcomes*

Unfortunately, we could not select from a variety of data sets since large, coherent snow depth data sets in a high resolution are rare and not always publicly accessible. We described our available sites and the measurement methods in section 2, in the original manuscript.
It is therefore also not possible to add more case studies since we do not have access to other highly-resolved snow depth data sets at peak of winter or during the depletion period covering large regions comparable to the size of the three we used (larger or equal to $30km^2$). Having coherent regions of highly-resolved snow depth data that cover elevation differences larger than 1500m and being large enough to randomly sample in differently sized domains (see Fig. 1) is quite unique.

2) *Some of the conclusion appear too strong and too general considering previous observation and moreover that it has been applied only for peak of winter for only a small number of snow conditions*

Thanks for pointing that out. We revised the Discussion and Conclusion section (see also our response to the first reviewer). We now also clearly state that our three data sets were all gathered at peak of winter in two different geographic areas. However, we do not fully understand what is meant by ‘a small number of snow conditions’. Our parameterization was tested in two alpine regions with different topographic characteristics. However, since we lack large-scale measurements from other environments, such as prairies, tundra and forested regions, our parameterization is restricted to alpine regions. Once more highly-resolved, large coherent snow depth data sets become available for different regions the application of mean snow depth as a climate indicator can be verified. In the last paragraph of the Discussion and Conclusion section we now discuss this issue.

3) *I would better highlight the limit of the paper outcome related to its data (site selection, time, variability of snow conditions)*

In the revised Discussion and Conclusion, we hopefully now better point out the limitations in terms of the data sets which we had available. See also our answer to the previous issue.

4) *Provide a more clear interpretation from the physical processes point of view of the paper outcomes in order to better understand the meaning and the relevance of the results deeply and precisely described in section 4*

We revised the Discussion and Conclusion section regarding the physical processes discussion.

Specific comments:

1) *Please better clarify how you individuated peak of winter timing*

We here define the peak of winter as the point in time with the highest snow depth during each individual winter. Thus, a measurement had to be conducted as closely as possible at this point/period in time. However, obviously it is very difficult to choose a measurement date which actual falls precisely at this point in time since the actual course of the winter is not known in advance. Please also keep in mind that high-resolution snow depth observations from aerial photography and from ALS require cloudless sky conditions. For more details about the data acquisitions see Section 2 in our manuscript, where also the articles describing the measurements in detail are cited.

2) *At line 19 p. 9792 describe under which hypothesis you assume spatially homogeneous melt*

The assumption of homogeneous melt rates might be questionable, even though it was previously found that homogeneous melt rates reasonably depict a depleting snow cover in mountainous terrain; see the study of Egli and Jonas (2009) which we cited in line 13 on page 9806. We used this assumption to derive snow cover depletion curves in order to verify a previously published SCA parameterization which was also derived assuming homogeneous melt rates (cf. Essery and Pomeroy (2004)).

3) *At line 24 p. 9797 subtraction should be the opposite. HS = winter-summer*

Thanks. Of course, HS was derived by subtracting the summer from the winter DSM. We corrected the typo.

4) *If possible please better motivate and describe the differences between the three study sites shown in table 1*

We feel we described the correlations with terrain parameters for all regions and each catchment separately quite sufficient in the original results section (on p.9803 lines 15-26) as well as in the original Discussion and Conclusion section (on p. 9809 lines 15 to 25).

5) *I appreciated the readability of figures and plots*

Thanks.