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Interactive comment on "How much can we save? Impact of different emission scenarios on future snow cover in the Alps" by Christoph Marty et al.

Anonymous Referee #1
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The manuscript by Marty et al. is concerned with the assessment of future snow cover changes over two mountainous regions of Switzerland. For this purpose a gridded version of the CH2011 Swiss Climate Scenarios is used to force a distributed model of the surface snowpack. The analysis includes an assessment of projection uncertainties arising from the assumption of different emission scenarios and from climate model uncertainty. In agreement with previous works the study finds an important decrease of future snow cover that considerably depends on elevation and, for the late 21st century, on the choice of emission scenario. In a general sense, the topic of the work fits very well into the journal's scope and adds a further piece of information to 21st Century climate change impacts in the European Alps. Qualitatively and quantitatively previous works are confirmed employing a new methodology that relies on gridded climate change information and the application of a spatially distributed snow pack model. As such, I consider the work as being relevant for the scientific community. For most parts, the methods are described appropriately, and the conclusions are well-based on the results obtained. There are only minor language issues. The paper however suffers from a number of inaccuracies in the description of the underlying datasets, from a partly questionable analysis of interannual snow cover variability and from a partly poor figure quality. Please see the listing below for further details. These issues should be improved before publication of the paper. For this purpose, only few new analyses are required and the basic structure of the paper does not have to be changed. I'd therefore suggest to return the manuscript to the authors for minor revisions. I hope my comments are considered constructive. Congratulations to the authors for this nice piece of work!
With kind regards.

MAJOR ISSUES
Reference to and description of the climate scenarios: On page 4 line 25 the climate scenarios are introduced as the ensemble mean of 20 GCM-RCM chains, and the term "ensemble mean" is later on frequently used. I believe this is not correct. To my knowledge the employed gridded scenarios provide three estimates for each season, each variable, each emission scenario and each grid cell: A median estimate, a lower estimate and an upper estimate. For most analyses in the present work the median estimate is used. This however is not the same as the ensemble mean signal as it originates from a probabilistic procedure that implicitly weights the underlying climate model chains. The ensemble mean field is basically only used for deriving the spatial anomalies to the regional estimates (see Zubler et al. 2014). Hence, the authors need to speak of the "median estimate" (and of the "upper" and "lower estimate" in Section 3.7). This concerns the entire manuscript.

Response:
We fully agree and replaced "ensemble mean" with "median estimate" throughout the manuscript.

Analysis of inter-annual variability (concerns several parts of the manuscript): In my opinion, the focus on uncertainty due to interannual variability in many analysis is not justified. This concerns, for instance, the analysis of the d-value in Section 3.3 or the variability ranges in Figure 3 or the entire Figure S2. As the authors correctly state, the employed delta change scenarios do NOT

account for changes in interannual variabililty, and the variability of the input series of temperature and precipitation will always reflect the variability of the control period. Hence, it is critical to explicitly analyse the range of signals obtained by comparing one future year to the mean state of the 13-year reference period as changes in interannual variability between the control and future period are completely neglected. My suggestion would be to rather include an assessment of climate model uncertainty (by considering always the lower, median and upper estimates of the climate scenarios).

Response:
We agree and therefore changed the following points:

- *We rewrote the corresponding paragraph and added a few sentences: "The Delta change method applies changes in temperature and precipitation, which depend only on time period and emission scenario but are otherwise constant. Therefore changes in future climate variability, which may be present in the original RCM model predictions, are neglected. According to climate model projections there are no clear signs how future temperature and precipitation variability will evolve in winter in midlatitudes (Deser et al., 2012), although a recent study demonstrates a slight decrease of winter temperature variability (Holmes et al., 2016). The analyzed inter-annual variability in this study is therefore first of all determined by the inter-annual variability of the underlying temperature and precipitation conditions in the reference period. For the future scenario periods the shown inter-annual snow variability is additionally influenced by the non-linear dependence of snow on temperature, which changes the variability dependent on the size of the ΔT values."*
- *The variability information in Figure 3 was replaced as suggested with lower and upper estimates at least for one region and on emission scenario. The figure caption was changed accordingly: "Decrease of annual mean snow depth (%) relative to the reference period (1999–2012) for the Aare region and the Grisons region for the three different emission scenarios and time periods based on the median estimate change of temperature and precipitation (bars). The lowest and highest estimates (Table 3) are shown for the Aare regions and A2 scenario only (dots)."*
- *The figure caption of Figure S2 was changed to: "Distribution of the annual relative decreases of the snow depth for A2, A1B and RCP3PD and the three different future time periods (2020-49, 2045-74, 2070-99) for Aare (left) and Grisons (right) based on the inter-annual variability of the reference period."*
- *An assessment of the climate model uncertainty has now been included in Figure 6 by showing not only the median, but also the lower and upper estimates of the climate scenarios. The figure caption now reads: "Total volume of snow (Jan-Mar) in the Aare region for the today (solid line) and the end of the century (dotted line). The shaded area for the reference period indicates half of the standard deviation (for readability) of the inter-annual variability. The shaded area of the 2085 scenario period indicates the range between the lowest and highest estimate based on the A2 emission scenario (Table 3)."*

We kept Figure S2 as a supplement since believe it provides a valuable information how the future inter-annual variability can evolve due to non-linear dependence of snow on temperature.

MINOR ISSUES
Reference period: The reference period of the presented work is 1999-2012, while the reference period for the CH2011 delta change scenarios is 1980-2009, hence there's on overlap of 11 years only. This inconsistency should at least be mentioned (if not accounted for explicitly by scaling the CH2011 deltas according to difference of the mean observed climate for 1980-2009 versus 1999-2012).

Response:
We agree and emphasized this fact by adding the following sentence in the Data and Methods chapter:
"Please note, that the reference period of these delta values (1980-2009) has an overlap of 11 years only with the reference period of the meteorological input data (1999-2012). However, a comparison of the winter temperatures for example revealed a difference of only 0.06 K between two reference periods."

Figure quality: The quality of many figures (e.g., Figs. 4, 5, 6, 7, 8, S1, S2, S4, S5, S6) is very poor (both in the PDF and when printed) and should be improved.

Response:
We apologize for the bad quality and included improved figures in the revised version.

Aare vs Grisons: The manuscript frequently switches between showing results for the Aare region only, for Grisons only or for both. This is rather confusing, and there does not seem to be a clear motivation for this. I'd suggest to harmonize the presentation in this aspect or to better motivate the choice of one of the regions for a specific analysis.

Response:
We forgot to mention this in the original manuscript. We therefore included the following sentence at the beginning of the Results chapter: "We often show results for both Alpine regions, but sometimes we focus on the Aare region only since the results are quite similar and its area below 500 m is larger and more homogeneous than the corresponding elevation zone in the Grisons region." Moreover, the results for the Grisons region were also added to Figure 2.

page 2, line 14: The model employed by Marke et al. is named AMUNDSEN (nor AMSUNDSON).

Corrected

page 3, line 4: "The precipitation in its northern part".

Corrected

page 3, line 22: "(atmospheric)": the 3D aspect probably not only concerns the atmospheric part but also the (sub-)surface part of SNOWPACK (vertical layers in the soil and the snowpack).

Yes, there is an optional module in ALPINE3D, which also considers the soil. However, this module was turned off in our simulations. In order to emphasize this fact we rephrased the corresponding sentence to: "It consists of a snow cover and runoff module SNOWPACK and optional modules like vegetation, soil and snow transport (Lehning et al., 2006)."

page 3, line 26: The reference Bossard et al. is missing in the list of references.

Corrected

page 3, lines 29-31: Unclear.

We agree and rephrased the sentence to: "Since this study focusses on snow on ground but not snow on glaciers, the few pixels with glacier surfaces were removed in the post-processing in order to reduce the uncertainty of our results."

page 4, lines 11-12:
The gridded climate scenarios employed here are formerly not part of the CH2011 scenarios but are an extension to them. Also they are not the only scenario product provided by the CH2011 initiative (as suggested by the second sentence). Please reformulate.

We agree and reformulated the sentence to: "Projections of future climate are provided as an extension of the CH2011 climate change initiative. This initiative provides among others daily change values of temperature and precipitation for Switzerland on a 2 km grid (Zubler et al., 2014),…"

page 4, line 27: "considered for some analyses" instead of "calculated for some analysis".

Corrected

page 5, line 17: "for the months January and March for the Aare region".

We actually show the mean values for period between January and March. We therefore reformulated the sentence to: "… are shown for the mean values of the January to March period."

page 5, lines 17-18: This difference between Aare and Grisons are actually not shown, which should be indicated by adding "(not shown)" are something similar in order to avoid confusion.

We agree and changed the sentence accordingly: "Slightly higher temperature changes in Grisons than in the Aare region are projected, especially for the end of the century (not shown)".

page 6, lines 21-22: Can undercatch really be a reason. According to Section 2.2 undercatch of the gauges has been corrected for. It's probably more the uncertainty of such an undercatch correction that is important here.

We agree and changed the sentence to: "High RMSE values at high-alpine sites are also explained by the fact that the measured precipitation is often heavily affected by the uncertainty of the under-catch correction and…"

page 6, lines 26-27: It is not clear to which metric these ranges refer. Is it the mean bias of mean winter snow depth?

We agree and clarified the sentence to: "…the uncertainty in simulating the mean winter snow depth in the reference period at the point scale (between -15 and 26 % for the different stations) increases…"

page 8, line 32: "winter months".

corrected

page 9, line 9: "These results" and "who investigated".

corrected

page 9, lines 9-10: I'd doubt that it is really the temperature change anomaly that is responsible for the sensitivity of this elevation zone in terms of shortening of the snow season. It is probably the fact that this elevation range is closest to the 0 C limit and a future temperature will hence be more effective here in terms of snow day change. At higher reaches, many parts of the winter will still remain below the freezing level. At lower reaches the snow season is anyway too short to produce important reductions in the period length of continuous snow cover.

We agree and changed the sentence to: "*This is probably caused by the fact that this elevation zone is closest to the 0°C limit. At upper reaches, many parts of the winter will still remain below the freezing level. At lower reaches the snow season is anyway too short to produce important reductions in the period length of continuous snow cover."*

page 9, line 12: "4.5 months".

corrected

page 9, line 16: "who demonstrate".

corrected

page 9, line 19: "who used".

corrected

page 10, line 5: "section 0" -> please correct.

corrected

page 10, line 6: Where is this inter-model variability shown? This is not clear.

We agree, this is not shown. We therefore reformulated the sentence to: "Note that the, the inter-model variability, from which the median estimate is calculated, is much smaller than the inter-annual variability as shown in Schmucki et al. (2015b)."

page 11, line 21: "from the ensemble".

corrected

page 12, line 27: "between the individual models".

corrected

page 12, line 30: "of these projected changes".

corrected

page 13, line 31: "Rhine".

corrected

caption of Figure 3: "chapter 3.3" instead of "chapter 0".

The figure has been changed. A correction is therefore not anymore necessary.

Figure 6: In line with above comment on the analysis of interannual variability, I'd suggest to show the range of the three model uncertainty estimates for A1B. For the reference, it is OK to show the individual years.

We agree and implemented the suggested changes.

Table S1: This table could be shortened to provide only the mean value for each site. There does not seem to be any strong trend in the RMSE scores, and the temporal evolution is anyway not discussed.

This could be done, but we prefer the current version because the values for the individual years indeed provide the information that the error is not dependent on snow abundant or snow scarce years.

Caption of Figure S4: Please indicate that this is the Figure for the Aare Region (it is not mentioned in the caption).

corrected

Figure S5: I’d suggest to include the snow day threshold directly in the 4 panels. This would strongly facilitate the interpretation of the figure.

implemented