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The Cryosphere Discussions

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.

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).

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).

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.

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.

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

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

page 3, line 22: "(atmospheric)": the 3D aspect probably not only concerns the atmo-

spheric part but also the (sub-)surface part of SNOWPACK (vertical layers in the soil and the snowpack).

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

page 3, lines 29-31: Unclear.

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.

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

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

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.

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.

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?

page 8, line 32: "winter months".

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

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 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”.

page 9, line 16: “who demonstrate”.

page 9, line 19: “who used”.

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

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

page 11, line 21: “from the ensemble”.

page 12, line 27: “between the individual models”.

page 12, line 30: “of these projected changes”.

page 13, line 31: “Rhine”.

caption of Figure 3: “chapter 3.3” instead of “chapter 0”.

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.

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.

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

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.

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