Reviewer 2

We want to thank Reviewer 2 for her/his constructive comments. Our answers are written in italic letters alongside to the reviewer's comments.

This study assesses projected evolutions of snow-related events in a small alpine region located in Switzerland, using a simulation chain composed of dynamical climate simulations, a stochastic precipitation generator, a snow model. This study provides interesting results about these possible future events, and the methodological choices seem reasonable, at least for the simulations, but there are two main aspects of the manuscript that need to be improved.

Thank you for your assessment. We agree that the method description needs to be expanded and that the manuscript would benefit from a more chronological sequence with a clearer separation of sections. We will also clearly describe the advantages of the chosen uncertainty partitioning method compared to other methods. For more details, please see our specific responses to your comments below.

1. Presentation of the methodology

Section 2 is difficult to follow for several reasons. The first reason is that the different subsections 2.2, 2.3, 2.4 do not follow a logical order. When the snow model is described, we do not know how its inputs (total precipitation, air temperature, etc.) are obtained, or their spatial resolution. Another example, factors of change are first introduced in Subsection 2.3 whereas they are obtained from climate model outputs in Subsection 2.4. I advise following the order of the simulation chain: 1/Climate models, 2/ Weather generator, 3/ Snow model.

We will reorder the method section in order following the input of Reviewer 1 and 2

Secondly, while I understand that all the details of the methodology cannot be provided, the current presentation lacks important information. In particular, from Table 1, it seems that the different precipitation products are used to fit different properties of the precipitation fields (i.e. monthly mean rainfall using optimal interpolated fields, mean areal rainfall using weather radar data). Does it mean that the variability of precipitation at a monthly scale (mean, variance, skewness, etc.) is reproduced using these optimal interpolated fields? What information is used to reproduce statistical properties at a finer resolution (hourly, daily)? For example, how the largest ("extreme") values at daily and sub-daily scales are reproduced? Since this is an important aspect of the study which focuses on intense rain-on-snow events, it needs to be clarified.

It is correct and useful that multiple sources representing different time scales are used to train the weather generator model. Indeed, the variables listed in Table 1 were used both for calibration and validation of the model (so, yes, for example, the spatial variability of precipitation is reproduced by using the optimal interpolated fields). We agree with the reviewer that additional details need to be provided on the model's ability to reproduce sub-daily climatic variables (such as precipitation, temperature, and radiation) and extremes, and we will include a new figure to illustrate this.

It was also unclear if there is any information of snow data at a daily scale. To my knowledge, weather radar data do not provide this kind of information. At a monthly scale, it is indicated at l. 110-111 that "optimal interpolation (OI) of snow depth sensor data and a gridded precipitation product, RhiresD) are used, but in Table 1, the line "Optimal interpolated fields" indicates that it is used to fit "Monthly mean rainfall", not snow, so that it is unclear if these OI fields provide total precipitation values or only rainfall. I am not sure where the product RhiresD appears in Table 1. What should be clarified is the list of the statistical properties (statistic, spatial and temporal resolution) of snow and rain that are fitted (and simulated) by the weather generator, and what source of information is used for each of these statistics.

We did not use daily snow data for training the weather generator. On a monthly level, we did so in the form of updated total precipitation to improve the standard Swiss precipitation product RhiresD, which suffers from a suboptimal station distribution at higher elevations as well as from undercatch of unshielded precipitation gauges. Using the optimal interpolation described in Magnusson et al. (2014), we assimilated daily snow depth measurements using RhiresD as the background field.

We will specify the use of snow information in more detail and also include more information about the interpolation of the precipitation used to train the weather generator.

At l. 131, it is indicated that factors of change are calculated, but no details are provided. For example, the factors of change are usually computed with respect to a reference period, but I could not find this information.

A control period of 30 years was used to compute the factors of change for mean temperature and precipitation change. We will add this information and explain the factors of change further.

2. Uncertainty assessment

The uncertainty assessment really puzzled me. There is a large number of publications on uncertainty partitioning for climate model simulations (Déqué et al., 2007; Hawkins and Sutton, 2009; Northrop and Chandler, 2014; and many others). These papers all apply an Analysis of Variance (ANOVA) method which provides a clear and rigorous framework in order to obtain a total variance and its components. The different contributions logically sum to one. I do not really understand the approach proposed in Fatichi et al. (2016) which is based on the evaluation of percentile ranges. At l. 154, it is indicated that the 5-95th percentiles obtained from the ten climate models actually refer to the minimum and the maximum, which seems to be a major flaw of the method. Low and high percentiles cannot be obtained from a very limited number of climate simulations (even if you emulate these simulations) and the evaluation of the dispersion (variance) is the best that you can obtain. Secondly, I cannot understand how we can interpret the different contributions if they do not sum to one (l. 167). Fractional uncertainty, as a percentage (e.g. Fig. 3 in Hawkins and Sutton, 2009) provides a direct assessment of the most important contributors to the uncertainty.

We follow the uncertainty partition method described by Fatichi et al. (2016), which is significantly different from other partition methods (such as the one described by Hawkins and Sutton that the reviewer mentioned). In our case, the reason to use the first method and not the second is that it is more suitable for the situation where you have the complete internal climate variability - model uncertainty - scenario uncertainty chain (as obtained from the AWE-GEN-2d model). We do agree that the method that we used is not described sufficiently (as the first reviewer noted as well) and we will revise the method section to explain the method and how we use quantile ranges. Using Fatichi et al.'s method, the fractional uncertainty contribution cannot be summed to one, but to interpret the different contributions, they do not need to be summed to one. Due to the reviewer comment, we see the need to explain the method and its differences from other uncertainty partitioning methods in more detail. We will therefore extend that section of the manuscript.

At l. 164-165, it is indicated that “weights [are used] to avoid overweighting days with only low climate change signal uncertainty”. I do not see the problem of having a low climate change signal uncertainty, and why it becomes a problem using your approach.

We have chosen to weight the annual mean of daily fractional uncertainties with daily total uncertainty. This is, in our opinion, meaningful because for days with a low total uncertainty the distribution between the uncertainty sources is of minor importance. These days may, however, substantially influence this distribution in a non-weighted annual mean. We will clarify this procedure in more detail in the revised manuscript.

For all these reasons, I strongly recommend using a standard ANOVA approach for the uncertainty assessment.

3. Minor comments:

All figure captions: Usually “(a)”, “(b)”, etc. are placed before the description of the respective subpanels.

We will change this as suggested.

Figure 2: The labels of the y-axis are ILWR and ISWR for panels (c) and (d) whereas in the caption, it is inverted.

Many thanks for finding this error. We will change this as suggested.

260: “by definition, only determined by natural variability”: I am not sure what you mean by “definition”. There is also an important part of model uncertainty for the current climate periods. This kind of uncertainty is usually removed mechanically using factors of change (as you did probably). A clarification would be appreciated here.

We will clarify in the revised manuscript that current climate periods are only affected by natural climate variability in our model setup.

14 – l. 293: I guess “Figure 7” is missing.

Many thanks for finding this error. We will change this as suggested.

17: Figure 9 is not presented and described.

Many thanks for finding this error. We will change this as suggested.

References

Déqué, M., D. P. Rowell, D. Lüthi, F. Giorgi, J. H. Christensen, B. Rockel, D. Jacob, E. Kjellström, M. de Castro, and B. van den Hurk. 2007. “An Intercomparison of Regional Climate Simulations for Europe: Assessing Uncertainties in Model Projections.” *Climatic Change* 81 (1): 53–70. https://doi.org/10.1007/s10584-006-9228-x.

Hawkins, E., and R. Sutton. 2009. “The Potential to Narrow Uncertainty in Regional Climate Predictions.” *Bulletin of the American Meteorological Society* 90 (8): 1095–1107. https://doi.org/10.1175/2009BAMS2607.1.