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Interactive comment on "Multi-components ensembles of future meteorological and natural snow conditions in the Northern French Alps" *by* Deborah Verfaillie et al.

Anonymous Referee #1

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The work by Verfaillie et al. presents a comprehensive analysis of past and future snow conditions at a mid-elevation mountain range in the French Alps. The regional SAFRAN re-analysis and bias-adjusted RCM experiments covering three greenhouse gas emission scenarios are used to drive the Crocus snowpack model, and model simulations are compared against observations at a single measurement site. The snowpack model is employed in a multiphysics ensemble approach which allows for an assessment of the contribution of snowpack modelling uncertainty to the overall projection uncertainty. Results for a range of snow indicators are presented. Concerning the overall future degradation of the snowpack they largely confirm previous works, but also provide a number of new and useful insights that are at least valid for this specific

case.

Overall, I consider the paper as a relevant and interesting piece of work. The methods and data used are comprehensively described and are well introduced (except for the downscaling and bias-adjustment method ADAMONT, which is however explained in detail in a previous paper). The methodological approach is sound and valid. The introduction and the discussion properly refer to existing works in this field, and the conclusions are well based on the results obtained. There are no language issues, and the topic clearly fits into the journal's scope. As such, I could generally recommend a publication of the work. However, a few minor and one major issues remain, and I'd suggest to ask the authors for a revision of their work in these respects before final publication. Minor issues are listed at the end of this review.

The remaining problem with the existing manuscript is its rather technical touch and the wealth of information that is presented in terms of data sets, emission scenarios, scenario periods, methods and especially snow indicators. The comprehensiveness of the work is impressive, but the reader very easily gets lost in this large amount of information that is presented in the text, in the tables and in the figures. These information might be very useful for local stakeholders operating in this very region and being affected by snow conditions, but for a truly useful contribution to the scientific community the results and their presentation need to be much better streamlined in my opinion. The generally most interesting part of the work is probably the entire methodological approach and the possibilities that arise from it. The very detailed results for a representative elevation of 1500 m in the Chartreuse mountain range are more of a case study, and the details of their presentation should receive less emphasis. One option might be to remove parts of the analysis entirely from the manuscript and place it in an additional, accompanying publication (e.g. the multiphysics ensemble analysis which is only briefly described in the results and which has much more potential to be analyzed in more detail). Another option is to move some of the material from the main manuscript to the supplement. This could for instance concern several of the snow indicators (like onset and meltout date), that are in any case only briefly discussed. In combination with such a streamlining, I'd suggest to put a little more emphasis on the actual processes that are responsible for the identified future changes in the snow indicators. Little is so far said about that. The Crocus model output surely provides an opportunity to do so (e.g., separate analysis of snow accumulation and snow melt amounts). In this respect, the relation of the snow indictaor changes to the GLOBAL temperature change is not very helpful and the authors should think about putting the LOCAL temperature change into focus (though I completely understand the choice of the global scale given political climate targets). Such a shift of the focus away from details of the case study and towards a more methodological and process-oriented analysis would be very worthwhile in my opinion. Apart from this and as said before, I consider the manuscript as being of high quality and of general relevance for the readership of the journal.

With kind regards.

Minor issues ===== Spatial scale of the Crocus application: What remains somehow unclear is the spatial setup of the Crocus model. I assume the authors use a single-site setup, driven by the outcomes of the SAFRAN reanalysis and of the ADAMONT downscaling method for a representative 1500 m elevation range in the Chartreuse massif. Is that the case? If so, this needs to be clearly said and described in some more detail. It would imply that the results shown are only valid for that specific elevation range in this massif. What about other elevations then? Is it possible to come up with some speculation here as well? Snow projections will surely strongly depend on the elevation considered, and some placement of the results into a broader spatial context would be helpful.

Page 1 Line 2: “investigates” instead of “introduces” is probabaly the better choice.

Page 1 Line 9: “reduction in mean interannual snow conditions” is rather unclear.

Page 2 Line 30: “they” instead of “there”.

Page 3 Line 22: The term “currently” is probably wrong. At this point, more GCM-RCM chains are available from EURO-CORDEX. The authors just either specify their date of access of the data base or justify their selection of all available model chains.

Page 4 Lines 8-15: Please check: Is SAFRAN really ONLY available over mountain ranges? To my knowledge, entire France is covered.

Page 8 Lines 5-21: This method description is rather confusing and very hard to follow. Please streamline. Figure 1: The STEDx should represent some duration of exceedance and hence need to be represented by some horizontal range in this graph. The representation by single vertical arrows is probably wrong, please check.

Page 9, line 1: Temperature changes are surely not computed in a relative manner, please check.

Page 19 Lines 6-10: I assume this is simply an effect of random internal variability at decadal scale, could that be (simulations out-of-phase with reality)? Please clarify.

Page 23 Line 31: Isn't it rather random variations (instead of systematic variations)?

Page 25 Lines 14-16: Is this really the case? Why should a matching of quantile distributions reduce interannual variations? please check and better explain.

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