Comments from Referee #2

We thank the reviewer #2 for these comments on our manuscript. Please find below a detailed feedback to individual comments and questions.

Main comments:

RC2#1. The methodological approach proposed in the paper, based on simultaneous use of observations and model projections, is stimulating in particular when facing problems (like the one addressed in the paper) in which numerical simulation models are characterized by heavy tuning-parameterization (fudge factors) changing in time. However, in order to make the estimation process tractable in the specific application considered in the paper, many ad hoc assumptions have to be introduced and the results are admittedly (Section 5.2) problematic, raising doubts concerning the **applicability** of the adopted working hypotheses. This situation often occurs in operational statistical estimation: thorough a posteriori analysis is almost invariably required; the authors should critically re-examine their assumptions.

We thank the reviewer for this comment. We agree that some assumptions are particularly critical and Section 5.2 tries to discuss these limitations. Our ambition for this study is to present different possible nonstationary extreme value models that combine past observations and an ensemble of simulations from climate models. It follows a study by the same authors on past observations (Le Roux et al., 2020) which shows that nonstationary GEV models are adequate for the assessment of ground snow load extremes in the French Alps. We also refer to the comment RC1#1 which presents Goodness-of-Fit tests of the proposed models of this study. It is also important to note that the set of the proposed models is larger that most nonstationary GEV models of the literature. These additional ad-hoc assumptions can actually be viewed as a more general framework. For example, the GEV parameters of nonstationary models often follow linear trends, which is a special case of the piecewise linear functions, i.e. when L=1.

Among the wide range of possible nonstationary GEV models, we have performed many tests in order to select a limited set of applicable models. Some models were overparameterized and could not be fitted (i.e. the parameters could not be identified). This might be a point that could be added to Section 5.2 in order to comfort the proposed approach. On the contrary, it must also be pointed out that modeling the evolution of the GEV parameters using nonlinear functions is particularly challenging, which explains that it is rarely done (see Table 1), whereas monotonic evolutions seem overly simplistic in many applications, especially over a long time period (e.g. more than 150 years). As a consequence, we do not wish to re-examine this particular assumption as it both represents an interesting challenge (how to cope with these nonlinear evolutions) and a necessary framework (for some extreme variables that could reach a “peak” in the future).

We also applied the methodology to other variables (results not shown) and obtained satisfying results in the sense that their interpretations were in agreement with what can be expected from a physical point of view. We are thus confident that the general approach (i.e. the two-step approach presented in Section 3.5 and the use of adjustment coefficients) is promising and can be considered as a solid basis for various applications, although we acknowledge that the current approach has some limitations.

RC2#2. The paper is neat and clean, but here and there not easily legible as it is very concise (“dense”): since the paper proposes issues of potential interest for a wide audience in which “non experienced” readers could find elements of interest I suggest a more “friendly” communication approach; but I leave to the authors deciding whether being concise is more important than being readily accessible for a wider audience.

It is important for us to reach a wide audience of practitioners and we agree that the current manuscript could be improved from this perspective. For example, a simple sketch illustrating the concept of the “adjustment coefficients” could be added in Section 3.2:

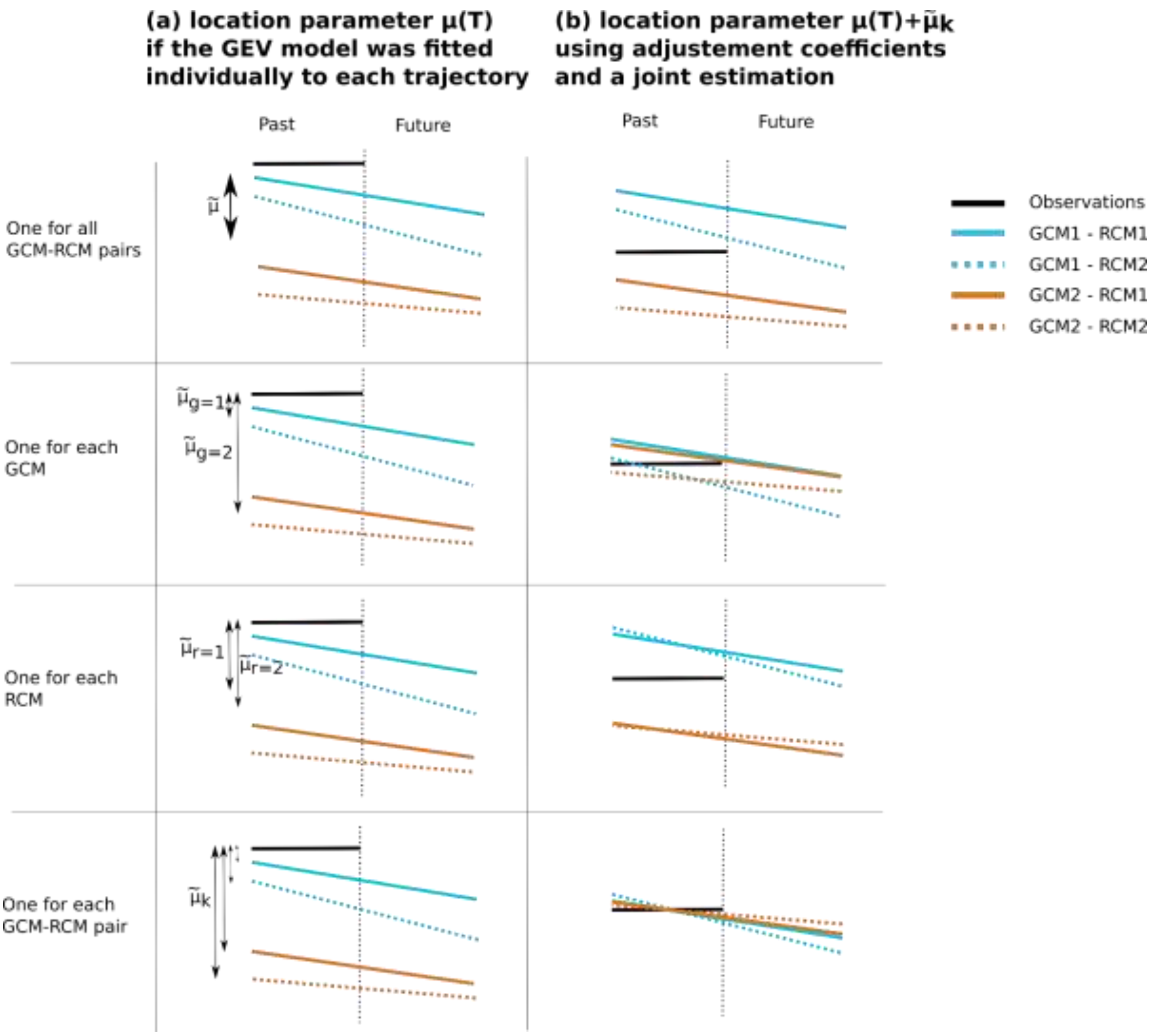

Figure 3: Illustration of the evolution of the location parameter μ(T) for the different options of adjustement coefficients with a fictive ensemble composed of 4 different GCM/RCM pairs with 2 different GCMs and 2 different RCMs. (a) location parameter μ(T) if the GEV model was fitted individually to each trajectory. (b) location parameter μ(T)+μ_k using adjustement coefficients and a joint estimation.

Specific comments:

RC2#3. Many acronyms and “technical slang” words appear in the paper: a glossary may help.

Thank you for this comment. We define these acronyms when they first appear in the text. If the reviewer thinks that some acronyms or technical terms are not properly defined, we would be pleased to explain them in more detail.

RC2#4. **Line 3** “chain of MME”: define in text or in glossary.

MME refers to “multi-model ensembles” as defined in the text at l. 28 and is a standard acronym when a set of simulations obtained from different climate models is used. It is a very common acronym and it does not seem necessary to define it in a glossary in our opinion. The term “chain” will be replaced by “climate simulation”. Climate simulations are often referred to as “chains” because it is obtained as the result of a simulation chain: a RCP scenario provides the greenhouse gas concentration trajectories, then a General Circulation Model (GCM) provides large scale climate simulations for these scenarios, then a Regional Climate Model provides climate simulations at a fine spatial resolution forced by the GCM outputs.

RC2#5. **Line 7** “with a robust quantification of uncertainties.”: this claim appears repeatedly in the paper; I found mathematical definition in Appendix A: Uncertainty estimation a technical quantification uncertainties, but not an analysis-discussion of the “robustness” of the estimation itself.

We agree that the term “robust” can be used only if there is a clear statistical interpretation. In our case, the quantification of uncertainties assesses in-sample variability using bootstrap methods and we do not provide results about the robustness of these uncertainties. The term “robust” will be removed in the revised version of the manuscript.

RC2#6. **Line 11** “is of major interest for the structural design of roofs”: not only (skying, avalanches, mobility, etc.); a few more words about applications could help.

We agree that snow loads may cause large-scale environmental damages (e.g. to forests) as well as damages to infrastructures (transportation networks, electricity networks) and accidents to people. This will be added to the abstract.

RC2#7. **Line 24** “EVT makes it possible to robustly estimate return levels”: see Line 7 comment.

As the term “robust” is not supported by the results, we propose to remove it from the manuscript.

RC2#8. **Line 29** “estimated separately on each chain of the MME”: see Line 3 comment.

“Chain” will be replaced by “climate simulation”.

RC2#9. **Line 32** “30-year time slices”: perhaps it is worth mentioning that 30 years is the traditional (WMO) “time scale” of “climatological” analysis.

Thanks for this comment, this will be added.

RC2#10. **Line 52** “robustly quantify uncertainties”: see above lines 7 and 24 comments.

Thank you, see responses above.

RC2#11. **Line 63** “adjustment coefficients”: a few more words could help.

Yes, we agree, see our response to the comment RC2#2.

RC2#12. **Line 80** “snow load” see Line 11 comment.

See our response to the comment RC2#6.

RC2#13. **Line 92** “Quantile mapping method ADAMONT”: a few words about it?

ADAMONT is a bias-correction and downscaling method which aims at adjusting daily climate projections from a regional climate model against a regional reanalysis of hourly meteorological conditions using quantile mapping. These explanations will be added to the manuscript.

RC2#14. **Line 204** “For a detailed analysis of the mean logarithmic scores of each parameterization for each massif, see Supplement, Part C.”: what is Supplement, Part C? Where is it?

Thank you for pointing out this mistake. This supplement was part of a previous version of the manuscript. This sentence will be removed.

RC2#15. **Fig.4** This figure plays a central role in the paper: some graphical features are too faint.

We will try to adjust the colors of the lines/bands in order to improve these graphical features.

RC2#16. **Line 255** “Figure 2.3 of IPCC (2019)”: wouldn’t it be possible to insert this figure or its direct internet link in the text?

Thank you for this suggestion. The following permalink will be inserted in the revised manuscript:

https://www.ipcc.ch/srocc/chapter/chapter-2/2-1introduction/ipcc-srocc-ch_2_3/

RC2#17. **Line 273** “because it sometimes leads to prediction failure, i.e. the predictive distribution gives a null probability to some future annual maxima.”: this is not clear to me!

This situation happens when $\xi(T)<0$, which means that the predictive distribution has an upper bound, and when some future annual maxima lies above this upper bound. This explanation will be added to the text.

RC2#18. **Line 287** “The 90% uncertainty intervals of return levels (Fig. 4) account both for the sampling uncertainty (Appendix A) and the climate model uncertainty (distributions are fitted together from the past observations and all GCM-RCM pairs).”: not easy to distinguish in the figure (see comment to Fig.4 above).

We agree that this figure could be improved in order to highlight the differences between the different uncertainty bands. This figure will be modified.

References

Le Roux, E., G. Evin, N. Eckert, J. Blanchet, and S. Morin. « Non-Stationary Extreme Value Analysis of Ground Snow Loads in the French Alps: A Comparison with Building Standards ». *Natural Hazards and Earth System Sciences* 20, n° 11 (6 novembre 2020): 2961-77. https://doi.org/10.5194/nhess-20-2961-2020.