Earth Syst. Dynam. Discuss., referee comment RC3
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Comment on esd-2021-79

Tamas Bodai (Referee)

Referee comment on "A non-stationary extreme-value approach for climate projection ensembles: application to snow loads in the French Alps" by Erwan Le Roux et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-79-RC3, 2021

The paper under review "A non-stationary extreme value approach for climate projection ensembles: application to snow loads in the French Alps" estimates 50-year return levels of snow load via fitting annual maxima historical and projection data by a non-stationary GEV distribution, with the global mean surface temperature as the co-variate.

My feeling is that this paper is not providing a good solution to a real problem. They consider several GCM-RCM model pairs ('model' in the following). In the Introduction the authors point out that previous studies evaluated extreme value statistics (EVS) for individual models and in some cases they took then the ensemble mean of return levels. In this regard the authors are concerned that the estimates for separate models are not so reliable because of data scarcity. However, they seem to do this themselves. They introduce the concept of "adjustment coefficients", which is really just a difference of an estimate of the GEV parameter for a particular model (or subset of the data) from the estimate upon lumping all the data. I think we really don't need a name for this, beside the problem that they do what they criticised. On the other hand, lumping all the data together, in order to have seemingly more robust estimates, is also problematic. As i pointed out in some recent publications of mine, a model ensemble (or multi-model ensemble, MME, as it's most often called) does not represent an objective probability distribution. As such, fitting a GEV to MME data is flawed methodology. It has no meaningful probabilistic interpretation. On the contrary, doing this for (converged) initial condition ensembles is fine.

It is actually a profound scientific challenge how to use MME data in a meaningful way. I don't mean to discourage anyone from trying, though, and hope that real progress can be made.

The authors promised a constraining of the estimates/projections using observational data. Emergent constraints is now a popular concept, but it appears to me that the authors did nothing like that. They simply threw the observational data in the mix. However, the information from it is diluted by the large amount of model data.

Obs data is rather used for bias correction. I'm not sure this was done. Or, if it was done, then it seems to have even less use to throw the obs data into the mix for doing EVS.

I share my detailed comments on the manuscript with the authors in an annotated pdf. Hopefully it is useful one way or another. I'm sorry that i cannot be more positive this time. If i misunderstand something, i'm happy to learn from the author's response.

Please note that i always do peer-review non-anonymously, and i never make recommendation for or against publication. I leave this wholly with the editor. If i submit a recommendation, it is only to bypass the rigidity of the electronic submission system of the journal and therefore please consider it void.

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Please also note the supplement to this comment:
https://esd.copernicus.org/preprints/esd-2021-79/esd-2021-79-RC3-supplement.pdf