

# ***Interactive comment on “Accounting for Non-stationarity in Extreme Snow Loads: a Comparison with Building Standards in the French Alps” by Erwan Le Roux et al.***

**Anonymous Referee #1**

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The paper presents a comparison of models for annual maxima of the ground snow loads (GSL) for each massif and each altitude interval of 300 m. The simplest models are the Gumbel and GEV with fixed parameters, then come non-stationary GEV models with either the location or location and scale parameters that may vary linearly with the year. The GLS data is provided by feeding the snow pack model Crocus with Safran meteorological reanalyses. According to the authors, only analyses with a proxy for GLS (such as snow depth) were carried out previously. The statistical models for extreme values applied in this work are relatively simplistic. This is justified by the authors by the short duration of the reanalysis data. The quantity of interest is the 50 year return level as it is used by the French regulation. A thorough comparison of the

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estimates provided by the analyses in the paper with the French standard is conducted.

I missed some validation or references to validation of the GLS data. As mentioned by the authors, Safran has a number of biases. Crocus might be based on assumptions which are not alway fulfilled and so the end product, GLS, might also suffer from a number of shortcomings.

In addition, there is no validation of the GEV models, just the final selection among the models in Table 2. These are based on AIC and likelihood ratios. So the best model is selected. But do they fit well ? What if none of the models were really adequate (even the best one among them) ? Maybe some qq-plots analyses should be included.

Given the amount of literature, I found it a bit disappointing that no attempt was made to rely on models that make use of more data, not only annual maxima as mentioned in the discussion. For instance, the tail index is taken to be constant in view of the difficulty to estimate it. There are many ways around this, one of which is the so-called regional analysis.

The authors argue that the number of years of the GSL reanalysis is too short to attempt to use anything else than linear relationships in the non-stationary models. Nevertheless, they recognize that other extreme value approaches, such as peaks-over-threshold, can be apply to exploit more data (more than a maxima per year). This seems a bit contradictory. If the authors could show that the GEV models with linear non-stationarities fit well the data without too much uncertainty in the estimates, then it would alleviate this issue.

Although the paper is generally well written, I think it can be improved on a number of aspects. - I found that the abstract was not conveying too well the main analyses and conclusions of the paper. - Section 2 : Line 57 : "we rely on GSL reanalysis", please, specify that is is a reanalysis of GSL. - P.3 What is the spatial resolution of Safran ? Does GSL has the same spatial resolution ? -P.7 L.134 : somewhere in the likelihood ratio, the model M\_0 should appear. Check the expression, I think there is a mistake.



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P.11 last sentence : "... often above effective return levels " effective in what ways ? not sure what it means here -P.13 L.245-250 : "... start the non-stationarity after the most likely year ", what is meant by most likely year ? -P.14 L. 260 "We did not rely on this choice because ..." This sentence should be rephrase, it is not clear, in my opinion. -P.14 L.265-270 : "annual maxima of GSL ARE " ARE instead of is, in several places - P.14 L.265-270 : "THE main reason is that " use THE - P.14 L.265-270 but in other places as well : ON average not in average - P.15 the trends OF the 50-year return levels not "w.r.t"

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