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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Uncertainty analysis of the rate of change of quantile due to global warming using uncertainty analysis of non-stationary frequency model of peak-over-threshold series" *by* Okjeong Lee et al.

Anonymous Referee #1

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The manuscript is overall well-written but needs to be checked for English thoroughly to increase readability. The reference list is appropriate and includes recent works on the topic. However, some improvements in the structure of the manuscript are needed, since some descriptions of the methodology are found in the discussion section. About the content of the manuscript, the analysis rely on just 2 stations in Korea, and only considers one covariate (daily dew point temperature), consequently the whole reads like a technical report rather than a scientific contribution to HESS. The interesting aspect is the model evaluation that is not based on model fit but on the uncertainties

on rainfall quantiles using a Bayesian framework. Overall the "reference DPT" concept used in the paper is interesting but not well defined and deserves a better explanation in the method section and discussion since it has practical interest for non-stationary analyses.

I would recommend adding more stations (obviously there are enough stations in Korea for such an analysis = https://doi.org/10.1002/joc.2068) and more importantly compare different covariates commonly used in non-stationary frequency analysis of extreme rainfall in the context of Korea (see my comment below about the lack of description on how the covariate is selected and used). These two recommendations would increase the representativeness of the results but also provide regional insights for Korea. At the current state of the manuscript, the reader cannot know if these results are only valid for these two stations and with this covariate.

Some specific comments:

My first comment is about the title, quite long and not very informative of the main scientific results of the paper. Something like this title might be better: Uncertainty of non-stationary frequency analysis applied to extreme rainfall in Korea.

Abstract, line 28, this sentence it not clear, maybe too general : "However, since the parameters of the estimated probability distribution contain a lot of uncertainty"

Abstract, line 40-42, this whole section below is quite trivial. Of course when a wrong covariate is selected in the POT model there is a stronger uncertainty. . . I don't see a major finding here.

Overall the abstract needs a major upgrading to better present the main findings of the study

Page 3, line 74 : change "in many documents" by "by many authors"

Page 3, lines 99-102: these sentences are not very clear.

Page 4, line 111, Why the authors are considering dew point temperature as a covariate for daily rainfall extremes for their stations in Korea ? Do previous works justify this choice ? No justification is given.

Page 5: line 130, “Daily rainfall depth of 0.1 mm or more was applied to the analysis,” is not very clear. Does this mean you consider daily rainfall lower to 0.1 mm =0 ? Is this related to rain gauges uncertainties ? How the results can be impacted by this choice ?

Page 7, line 170: I guess you mean here instead the “scale” parameter

There is no indication on how the covariate is included in the model; is it the Dew point temperature the same day of the extreme rainfall event? at the starting day of a rainfall event or its peak ? On the opposite, is it computed for the week, or the months before the event ? No information is provided here.

Page 12, line 327, “However, under the condition that DPT is not given in advance” not clear to me. Do you mean you draw randomly DPT values instead of the values corresponding to the days with extreme rainfall?

Page 13: the whole page/paragraph is quite long and not very clear to me, it could be shortened to the main findings.

Page 14, line 366, this equation should be in the Method section

The beginning of section 4.1 is obviously not a discussion and should be in the results section

Page 14, lines 386-390, this information should be in the method section, we should not discover in the discussion section how the covariate was implemented in the model. . . see my comment above.

The issue of setting a reference covariate to a given return level is an interesting aspect. I believe it is necessary to first analyze the response of extreme rainfall to different

values taken by the covariate, and as mentioned here it is difficult to identify a single value of the covariate related to a high risk of extreme rainfall. Yet this aspect needs more discussion, I don’t see an added value of randomly selecting a covariate from a pre-defined distribution (of the covariate).

Page 16, line 423; is this not totally expected?

Conclusions, lines 498-501, these sentences are elements of context and should not appear here but rather in the introduction.

Table 2: units?

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