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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Do climate-informed extreme value statistics improve the estimation of flood probabilities in Europe?" *by* Eva Steirou et al.

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This paper presents a European data-based analysis of the correlation between a number of atmospheric indices and flood exceedance probabilities at the sub-annual timescale. The novelty of the paper is related to the extent of the study region, i.e., all of Europe. The outcomes are interesting because of the coherent spatial patterns of the identified correlations in climatically different parts of Europe. The paper is well written, properly concise and clear. I believe it can become a very valuable entry for HESS. However, as always, some improvements are possible. My main comments/criticisms/suggestions are the following:

- Title: I do not think that it is possible to easily answer this question, it never is when dealing with extreme value statistics in the real world. Actually, while I consider very interesting the analysis of the correlation between the atmospheric indices and the parameters of flood exceedance probabilities, I am less convinced about the accuracy of flood frequency estimation provided here. The reason is that, in engineering hydrology, I think nobody would fit locally a GEV distribution using a likelihood-based method with no information on its shape parameter. Regional analysis is normally used to improve quantile estimation for high return periods (say 100-years) which is not performed here. I would agree that the paper provides an indication that there is potential for improving flood frequency estimation by including atmospheric dynamics in our models, but I guess there is much more to do to actually improve the existing regional models in use. Maybe this is what the Authors meant but, to me, the title is a bit misleading. In my opinion, a title that focuses on the identified correlations between atmospheric indices and local floods would be better.

- It is always strange, to me, to see studies that use Bayesian inference without using prior information, especially when some very useful prior information is out there. For example, for the GEV shape parameter of the stationary model (but also for the non-stationary one) I recommend using (at least) the "geophysical" prior in Martins and Stedinger (2000). They demonstrate that without a prior on the shape parameter of the GEV, maximum-likelihood estimates (and therefore presumably also Bayesian estimates) for hydrological samples are much less accurate than those obtained with other methods (e.g., L-moments). The "geophysical" prior in Martins and Stedinger (2000) provides "common sense" limits, but even better constraints could be obtained through regional analysis, of course.

- The motivation for assuming that only the location parameter varies in time (through its relationship to the covariates) should be discussed more in detail. Considering the proposed model, with the scale and shape parameters fixed, implies that the variance of the flood series does not change over time (e.g., if the mean annual flood peak increases of 5 m3/s, also the 100-yr flood increases of 5 m3/s, and so all other quantiles). Is this a reasonable assumption? For example, Serago and Vogel (2018) strongly criticize it and propose to use models with the coefficient of variation of the flood series constant in time, since that is consistent with observations in many studies (see the cited literature there). Using a model where CV is constant would be as parsimonious as the one used here and, according to Serago and Vogel (2018), more justified. I suspect that using this other assumption would not invalidate the spatial patterns that are shown in Figures 1 to 4, but would result in very different values in Figures 5 to 7.

- One issue I would also suggest to discuss is the uncertainty in the covariates. The model used here assumes that the covariates are exactly known. If the uncertainty in their knowledge would also be included, would the flood quantile estimates still be more precise than for the classical GEV model?

Additional detailed comments:

Line 20: I would expect that the improvement of estimation of flood probabilities is conditional on how well the covariates can be predicted.

Line 71: I am a little confused by the positive-negative anomalies vs. Northern-southern Europe because the sentence terminates with “during its positive state”. Maybe a rephrasing could help.

Line 107: The motivation of using Bayesian inference because of the quantification of uncertainty sounds a bit weak. The quantification of uncertainties is possible also with other methods than Bayesian, which is instead usually selected when subjective preferences or prior information is available (at least by us... statisticians have more profound reasons).

Line 150: The climate covariates are assumed exactly known in the method. Would it be possible to account for the fact that they are stochastic variables as well? I do not ask to change the method but maybe some discussion could be dedicated to this issue

(see main comments).

Line 158: The motivation for assuming that only the location parameter vary in time, i.e., the brevity of records, is not very convincing. The Authors should discuss it more (see main comments).

Line 174: Since Bayesian inference is done here, there is no reason why priors should not be used. For the GEV shape parameter of the stationary model I recommend to use (at least) the “geophysical” prior in Martins and Stedinger (2000) (see main comments).

Line 174: Which non-informative priors are used? Not all of them would result in the same inference. For repeatability, they should be stated.

Line 184: I would also look at the posterior distribution of the slope parameter and do the same as the Authors do here. I would just add a sentence to state that this is not a significance test (which has no meaning in Bayesian statistics).

Line 216: I worry that, for engineering purposes, the estimates of 100-yr floods through GEV without accounting for regional information is not to be recommended.

Line 221: Starting the sentence with “Since a Bayesian framework is used” is confusing because it sounds like saying that uncertainties cannot be quantified with other methods too.

Line 224 (and elsewhere): I would use the wording “posterior mode” instead of “maximum likelihood” because they may not be the same (it depends on the type of non-informative priors that are used). Bayesian posterior predictive distribution of flood peak quantiles or their posterior mean could have also been used. Is there a reason for choosing the posterior mode?

Line 258: Is there any (even speculative) reason for the contradicting patterns for Scandinavia?

Line 265: I also believe that the coherence in space is indicative of a real signal, however spatial correlation of the flood time-series could be a nuisance here, meaning that one sees the same dynamics in many sites because the same floods are occurring there (and therefore they should count as one site only). Since the spatial patterns are here over very large regions, the spatial correlation of the flood time-series cannot alone be responsible for it. However, I would suggest mentioning the problem.

Line 299: One curiosity. Since the proposed model has constant variance (and the dependence of small and large floods on the covariate is the same in terms of the difference in m3/s) I suspect the relative difference to be affected by catchment area (meaning by the average flow in the river). Is it the case? Of course, since the model is fitted independently to every site, the differences in fitted shape parameters will make this relationship noisier.

Section 3.2: I wonder how much the relative differences calculated here are due to the slope of the regression for the location parameter vs. the estimated shape parameters. Since no priors are used, I suspect that the posterior distributions of the shape parameter can be wide (spanning unreasonable values) and widely different between sites. Maybe a figure/table that also informs the reader about the obtained shape parameters would be useful.

Figure 7: Shouldn't be the classical GEV the same within each column? The credible bounds look different. Have I missed something?

Lines 335: The asymmetry of the credible bounds around the posterior mode is very well expected. If the posterior predictive distribution (or posterior mean) would have been used, that would have lied much more in the center of the bounds.

Line 352: I would add here a brief discussion on the predictability of the covariates since that is needed to make use of the model for prediction.

Line 357: I don't get the meaning of "...leads to highly varying flood quantile estimations for different probabilities of exceedance". Is the sentence referring to variations in time?

Or space? Or between models with different covariates? And, finally, the variability for "different probabilities of exceedance" of flood peaks at one site exists in terms of relative differences. In term of difference in m3/s, there is no variability at all, since in the models only the location parameter can vary. Maybe I just misunderstood. A rephrasing could help.

Line 358: Related to my previous comment on line 299: is it because the catchments in North-West Scandinavia and Britain are smaller than the others? Or is it because of unreasonably large shape parameters of the GEV?

Line 363: It is for me hard to see the decadal-scale variability in Figure 7. Maybe that could be shown in the figure.

Line 402: As an additional challenge (on top of the three that the Authors have listed) I would add the fact that now covariates are assumed perfectly known and should be instead treated as stochastic variables, I think.

References:

Martins, E. S., & Stedinger, J. R. (2000). Generalized maximumâĂŘlikelihood generalized extremeâĂŘvalue quantile estimators for hydrologic data. Water Resources Research, 36(3), 737-744.

Serago, J. M., & Vogel, R. M. (2018). Parsimonious nonstationary flood frequency analysis. Advances in Water Resources, 112, 1-16.

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