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Comment on hess-2022-358

Anonymous Referee #1

Referee comment on "A mixed distribution approach for low-flow frequency analysis – Part 2: Modeling dependency using a copula-based estimator" by Gregor Laaha, Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-358-RC1>, 2022

The work „A mixed distribution approach for low-flow frequency analysis – Part 2: Modeling dependency using a copula-based estimator“ by Gregor Laaha proposes a seasonal mixture distribution for low flows by explicitly taking account the dependency between the seasons using a copula approach. The applicability and main differences to common approaches are demonstrated for a large European data set and attributed by regression models using catchments and seasonality indices as explaining variables.

The manuscript is well written and logically structured. The research topic is highly relevant, as it extends the existing seasonal mixing approaches for floods to the low-flow case and takes into account the important aspect of dependence between winter and summer low flows. Especially this extension makes it relevant to hydrological sciences, as it is a novel approach considering the special characteristics of low flows.

I have a few comments that might help the readership to better follow the stream of thoughts and provide some background information. With these minor changes, the manuscript should be accepted for HESS.

Comments:

- I have concerns when using the term “accurate” when comparing different statistical models (e.g. in the abstract and the last lines of the conclusion). I can understand the idea of comparing the models by estimating the differences in the low flow quantiles. As always for statistical models in hydrological statistics, it is almost impossible to compare the results to the “true” value, as we simply cannot observe this. We are limited to the observations we have and can only compare the results of the models. Hence, the term “more accurate” for one model compared to another is not suitable,

even if one says that statistically the new model is more reasonable (to which I absolutely agree). The only way to justify something like accuracy would be simulations, as in this case the truth is known. I therefore recommend to weaken the statements in the manuscript and omit terms like "more accurate". Instead, the use of the term "gain" seems to be a very good idea to me and should be done throughout the whole manuscript. Similarly, the terms "error" (Figure 3) as well as under- and overestimation (e.g. ll. 275-276) seem to be misplaced here. I would also highly appreciate a simulation study that clearly demonstrates the benefits of the copula mixture model, but I can understand that this would increase the length of the manuscript too much.

- The author states that the Gumbel-Hougaard copula is used "as we are modeling extreme values". There exist many copula models for this purpose and I cannot see why only this should be suitable, especially for an application to whole Europe. I suggest to use a goodness-of-fit criterion such as AIC/BIC to select the best-fitting copula from a sample of possible models. It is not a priori clear to me why there should always be the same dependence structure for all catchments.
- After the model description, the manuscript goes right into the details of the results. I am missing at least a short description of the data. As one major aspect here is the dependence between winter and summer low flows, it is of high interest how these events have been defined. What is done in a case when a summer low flow reaches into winter or there is no clear distinction between summer and winter? How is winter and summer defined and are the same seasonal thresholds used for whole Europe? These are aspects which make the application of seasonal mixture models to low flows so much harder compared to floods. Moreover, they motivate this whole study.
- The indices under consideration should be explained. For example, it is not clear what SR or the circular seasonality index r are. In Table 1 it is stated that SR is the "ratio between mean summer and mean winter low-flow." If this is correct, why is it an indicator of seasonality? If there are few extreme low flows in summer and otherwise many large low flows while in winter there are many small low flows, the indicator would be highly impacted by the extreme low flows in summer. Wouldn't it be more reasonable to use the proportion of summer (or winter) low flows in the annual maximum series to investigate which has a greater impact on the AM? The index r only appears in Table 1 but is not mentioned in the text.

Technical remarks:

- l. 19: use "than" instead of "as"
- l. 58: What does "annual drought duration of volume" mean?
- l. 62: I guess a "not" is missing here.
- l. 149: (and throughout the text) I believe it should be "Elbersee gauge" instead of "gauge Elbersee"
- l. 158: "of" instead of "or"?
- l. 163: "that the mixed probability estimator"
- l. 246: should be "in Table 2".
- l. 308: "has" instead of "have"
- Figure 1: The mixed Copula model should be added to the legend. Currently, it is only mentioned in the caption.
- Figure 3: remove "as" from the first line in the caption