Dear Reviewer,

I would like to thank you for your valuable remarks on the MS as well. Please find my responses (plain font) to the comments (italic font) and suggested changes in response to the comments below. New or altered text is indicated by a red font, whereas old text elements are indicated by a blue font.

Response to Reviewer #2

1. Throughout the manuscript (See line 70 for example), the authors have claimed he is not aware of any study that used copulas to consider seasonal dependence in a mixed distribution approach. Ganguli and Reddy (2014) developed copula-based ensemble drought prediction models with up to 3 months' lead time considering the seasonality of SPI at a 6-month accumulation window. Two variants of drought forecast models were proposed: a single ensemble drought prediction model without seasonal partition and separate models for each of the four seasons in a year, combining them to constitute a yearly forecast model. The analyses showed that the seasonal prediction model performs better as compared to the model without seasonal partition. In addition, the incorporation of a copula-based conditional framework helps to provide an estimation of uncertainty.

RE: Thanks for this citation. This study uses a copula-based model between predicted and observed SPI to provide an estimation of uncertainty of the predicted drought index. While it is an interesting application of Copula models in the field of drought indices, it appears to have a quite different objective as compared to the scope of this study. Here, we focus on the extreme value distribution, using methods of frequency analysis of extreme events, which is not the case for the cited study. Moreover, here we use Copulas to model dependencies between seasonal distributions, whereas the cited study uses copulas between marginal predicted and observed SPI to simulate uncertainty. We therefore think that our study indeed presents an innovative approach to consider seasonal dependence in a mixed distribution approach.

As said above, the study shows an interesting application of copula models and a citation will be added accordingly.

2. "Eqs. 2, 6-8 representations of random variables are not correct. In fact, for summer, the random variables are drawn from the population, S = {s1, s2, ….., sn}. Likewise, for winter, the random variables are drawn from the population, W= {w1, w2, ….., wn}. The probability of a low-flow event with magnitude during summer, s and winter, w seasons to be represented as $F_S(s)$ and $F_W(w)$, respectively."…

RE: Our formulation is based on Stedinger et al. (1993) Section 18.6.2, which formulated a specific solution of the cdf of the mixture distribution as a product $F_S(m)$ $F_W(m)$. We agree with Stedinger that this is a valid formulation for the special case when we want to estimate a low-flow event with a certain magnitude m (or q). For this purpose, we set w=q and s=q, to obtain a specific solution for the case w=s=q, which we present in our MS. This notation is not only consistent with Salinas, but also with the first part of the study, which was already accepted for publication in this form. I like to keep this formulation but will clarify this in the text.

… Further, typically subscript/sample space is shown using capital letters, whereas the argument/individual random variables are shown using small letters with the same notations. They cannot be different, for example, q as mentioned in the manuscript even after considering seasonal stratifications.

RE: As said before, our formulation is consistent with this basic formula notation, but presented for the special case of s=w=q. We thereby follow the notation of Stedinger (see above).

3. Line # 105, How is the copula parameter ϑ is, estimated? There is no direct relationship between Spearman's ρ and ϑ for the Gumbel-Hougaard copula. However, such a relationship exists for Kendall's τ and ϑ. Also, it would be good to see the gauge-wise performance of Gumbel-Hougaard copula for the summer and winter seasons either in the SI section or in the main manuscript to see how credible are the copula performance in modelling seasonal dependence.

RE: The copula parameter is estimated base using a maximum likelihood estimator. A sentence will be added to the revised text.

4. Eq. 9, same error for random variable representation, PQ(q) instead of Pm(q).

RE: To avoid confusion, we replace $p_m(q)$ by p, as the further indices are not needed here (same for Eq. 10 which is now Eq. 11).

5. *At first, Eq. 11 should appear, followed by Eq. 10. Again, representations of random variables should be corrected considering seasonal partition in line with Eq. 9 and others.*

RE: Thanks, the order of equations has been changed and the text modified accordingly. As stated above, we would like to keep the notation most similar to Stedinger's notation, in line with our earlier responses.

6. Line #155, there is a subtle difference between the two estimators.

The sentence will be reformulated accordingly in the revised MS.

7. Line # 168, "We note that this behavior…

RE: the text was changed accordingly

8. Line # 173, how seasonality and seasonality ratio are determined in this study? Also, please discuss the associated implications of each of these indices in brief.

RE: Text will be added accordingly.

9. Line # 190 and paragraph therein: However, the copula-based approach is expected to preserve seasonal dependence patterns apart from the fact that they consider the marginal distribution of any form. On the other hand, the mixed distribution assumes only one type of probability distribution.

RE: The mentioned paragraph (around line # 190) is about visual evidence. To keep the flow, I would not include theoretical properties of the estimators here.

10. Line 195, could you provide a list of gauges, WMO ID, their latitude/longitude, catchment area, and years of available records in SI as part of the reproducibility of the work?

RE: We are using here the dataset of Laaha et al. (2017) for demonstration, a selection of stations (mainly based on the former FRIEND archive EWA which is now embedded in the GRDC data base) which was gathered for the purpose to explore regional drought patterns across Europe. As such we do not have full station information available, mainly coordinates and flow series. Making this data set publicly available is beyond the scope of this study and I thus feel unable to provide such additional information.

11. How the low flow is estimated in this study? Whether the constant/variable threshold approach is implemented to detect low flow signal.

RE: The comment corresponds with comment 3 of Reviewer 1. Low flows are defined by the annual / seasonal minima series (see beginning of Section 2.1). There is no threshold selection involved. The definition of the low-flow index will be made clearer right from the beginning of the paper.

12. Line 215: How relevant would be seasonal exceedance probability estimation since the concept of return period revolves around the sampling of annual and partial duration series?

RE: The AMS method of frequency analysis can readily be applied for seasonal calculations, when the annual extreme are sampled from a specific season, i.e. after seasonal stratification of the complete time series. The seasonal distribution is then a characterization of the occurrence probabilities of discharges in the respective season only. It is indicative of the severity of, e.g., a summer event relative to all other summer events, which is supplemental information to the annual probability of the event. Here we do not focus on the merits of knowing the individual seasonal distributions, but on the advantages when these are recombined to an annual estimate.

13. Eq. 13, the relative absolute deviation will not show any over/underestimation effect. Therefore, the absolute unit would be more beneficial.

RE: The absolute deviation was found useful in the first part of the study when the focus was on the magnitude of deviation rather than on the sign of the deviation. In this second paper all relative deviations are negative so both indices are equivalent. For the sake of comparability with the companion paper I would like to keep both indices here as this has no consequence for the results of the paper.

14. Fig. 3 caption; does the uncertainty of the annual probability estimator considers all catchments across the pan-EU scale?

In principle yes, in the given range of the graphs, while a few catchments with higher values (>600 %) were excluded for the sake of visual clarity. This is now added to the figure caption: Single outliers > 600 \% are discarded.

15. Line # 288: the large differences between two estimators at low return period is consistent with the differences in quantile estimates between the annual maxima/minima and partial duration *approach. In fact, the difference in return period estimation in Annual maxima/minima vs. partial duration series is generalized using a simple exponential relationship. Can the author derive such kind of generalized formula for the given EU catchments?*

The focus of this paper is the AMS approach only. The differences between the two estimators are stemming from the fact that the annual series is not homogeneous, which violates the validity of a distribution fitted to the data. The fitted distribution can be disturbed in various ways. I therefore cannot see an analogy to the difference (and exponential relationship) between AMS and PDS approaches.

16. For Figs. 2, 4-5, and 7-8, please use a continuous color bar at the bottom of the figures and show the color discretization.

There are pros and cons between continuous and discrete colour bars. The continuous can be said to show the original information more directly, whereas the discrete color classification has the advantage that it allows the reader to read-off the value more easily. I find the discrete color better suited and would like to keep it for the study.

17. Table 3. The minimum quantile is always zero, indicating water level is always less than the stage recorder during the low flow period; therefore, instead of furnishing information on the Min quantile, the 1st (25th percentile), median, and 3rd quantiles, including the interquartile range, would suffice. In fact, the IQR would show the catchment-wise variability in low flows for each estimator.

RE: We updated the table accordingly:

18. Line # 322, VIF is not defined earlier.

RE: The abbreviation VIF is defined in Section 3.2, where I added a short characterization:

The VIF provides an index that measures how much the variance of an estimated regression coefficient is increased because of collinearity. The adjusted R^2 is a penalized measure of model performance, so that a greater difference between adjusted and unadjusted R^2 will be interpreted as an indication of overfitting.

19. Line # 344: This suggests sensitivity towards BFI, which is, in turn, the function of catchment soil types and the availability of water bodies nearby.

Yes. More general, it is an indication of discharge share from stored sources (such as groundwater, lakes, soil water, or snow storage). This was added to the text.

20. Line 353: Laaha, 2022?
Thanks, corrected.

21. Line 386: the uncertainty could also stem from estimated copula parameters and the uncertainty due to marginal distribution. Therefore, sample lengths have a profound impact on multivariate *distribution. A list of available sample lengths is to be presented. For credible assessment of multivariate risk, sample lengths need to be at least more than 25 or larger.*

RE: We fully agree. It is common knowledge that the sample length is an important factor of the accuracy of frequency models, and often a minimum sample length of 25 or 30 years has been recommended. Here we have 673 series with full record length of 35 years and further 80 series with a record of at least 30 years and 29 records of at least 25 years. The information will be added to the text.

As can be seen, all stations have a common 25-year observation period, thereby satisfying both minimum record length criteria. As most of stations are based on the same sample length, these sources of uncertainty can be assumed to factor out.

22. Line 388: authors have pointed influence of catchment area, BFI, and to some extent, climate; however, terrain attributes, soil types, and land use/land cover do also have a profound impact on drought seasonality, persistence and recovery pattern.

RE: Yes, all these characteristics contribute to low flow generation, and BFI and seasonality are good indicators of their combined effect on catchment storage and release properties of the catchment. A sentence was added accordingly:

Storage and catchment size only have a minor effect on the performance gain of the mixed copula estimator and appear to play a subordinate role. This should also be the case for other catchment characteristics, such as soil properties, vegetation, and terrain, that tend to have more influence on surface processes and fast components of the water balance than on long-term storage, and thus less influence on the redistribution of water over time.

23. Line 400: The pronounced differences in quantiles, mainly at low return periods, are consistent with annual vs. partial duration series, wherein the differences tend to diminish for high return period events. A generalization of this would add value.

RE: As stated above (Comment 15), I cannot see any analogy to the difference between AMS and PDS approaches. The errors of the conventional annual estimator here stem from a different source (process heterogeneity of an annual extremes series).

24. This study is useful at a regional level focusing only EU zone. A discussion on how this study would add value for other parts of the globe, for example, in monsoon-dominated regions (South Asia and Africa) where marked seasonality is pronounced or in areas with relatively stable climate with subtle climatic variability (for example, sub-tropics) would be beneficial for audiences across the globe.

RE: We think the added value is mainly for regimes where both summer low-flows (due to precipitation deficit) and winter low-flows (due to freezing) occur, as only there the low flow processes are so fundamentally different that process-heterogeneity of annual extremes series gets thus prominent. This restricts the added value to cold and temperate climate with a cold season. We added clarified this issue as follows:

In the subsequent step, the performance gain was evaluated based on a larger pan-European data set. The patterns match well with the findings from the exemplary catchments. There is generally little difference between the mixed distribution approaches for severe low-flow events. However, for mild low-flow events the differences are large. For the T=2 year event, the gains of the mixed copula approach are most pronounced in the lowlands north of the Alps which is subject to cold climate. The gains are much smaller in the west, which is subject to temperate climate. Altogether, the patterns suggest a large gain of the mixed copula estimator over the annual probability estimator, making the method highly relevant for Europe as a whole. Similar effects can be expected around the world in cold and temperate climates, where both summer low-flows (due to precipitation deficit) and winter low-flows (due to freezing) occur. The method, however, should be less relevant for other (seasonal or aseasonal) climates without a frost season, as generating processes are not so fundamentally different for the low-flow events there.

25. Last but not least, the study assesses the performance of mixed distribution vs. copula-based dependency framework for modelling low flows accounting seasonality. This message is not well reflected in the title. The title could be tuned in that direction, for example: A bivariate approach for low-flow frequency analysis considering seasonality Part 2: Comparative assessment of Mixed Probability vs Copula-based Dependence Framework.

RE: Thanks for the suggestion! The title "A mixed distribution approach for low-flow frequency analysis" was chosen on purpose, to reflect that the approach is closely related to established methods for floods, which are here transferred to low flows. These approaches were often termed "mixtures" (Stedinger et al., 1993) "mixture distribution" (Fischer and Schumann, 2021; Szulczewski and Jakubowski, 2018) or "mixed probability distribution" (Fischer et al. 2016) approaches. The main aim of both papers is to propose a mixed distribution approach for low-flow frequency analysis. Here, we extend the estimator incorporate dependency of seasonal events using a copula-based estimator. I still find the title well suited, but this may also be a matter of taste. However, as the first part is already accepted, I feel unable to modify the title in the suggested form. However, I find the suggested part 2 (Comparative assessment of Mixed Probability vs Copula-based Dependence Framework) well suited and will use it in the title.

26. A Minor comment:

Line 41: Ganguli and Reddy (2012) presented a bivariate risk assessment framework for meteorological droughts in Saurashtra and Kutch regions of Gujarat state in India. Based on the tail dependence measure, the Gumbel-Hougaard copula emerged as the best model for modeling joint dependency between drought severity and duration. The comparative assessment of traditional bivariate distributions, such as bivariate log-normal and bivariate logistic models relative to copulas suggested that the extreme value family of the Gumbel-Hougaard copula was better suited for the area.

RE: Thanks, the citation will be added as well.

References:

Szulczewski, W. and Jakubowski, W.: The Application of Mixture Distribution for the Estimation of Extreme Floods in Controlled Catchment Basins, Water Resour Manage, 32, 3519–3534, https://doi.org/10.1007/s11269-018-2005-6, 2018.

Fischer, S., Schumann, A., and Schulte, M.: Characterisation of seasonal flood types according to timescales in mixed probability distributions, Journal of Hydrology, 539, 38–56, https://doi.org/10.1016/j.jhydrol.2016.05.005, 2016.