Author's response

I thank the Editor and the two reviewers for their valuable feedback.

In my author's response I have considered all comments of the reviewers and the Editor comments. Please find my detailed responses below.

Response to reviewer comments

Reviewer #1

This work presents an innovative formulation for the low-flow frequency analysis which accounts for the seasonal behavior of the low-flow that characterizes some regions.

The manuscript is very well written and organized and the research is of significant scientific interest. To my opinion, the work can be published after some minor revisions that could make it clearer.

Please, read in the following my suggestions.

1. The title suggests that a part 2 exists. If so, please refers to it in the manuscript. What does it deal with?
2. I suggest to include a figure that synthesizes the seasonal characteristics of the analyzed dataset, e.g. empirical cumulative distribution of the observed low-flow values, mean monthly series, so to appreciate the effectiveness of the mixed proposed approach.
3. Sometimes variables are not explicitly defined. Please, make sure of that. For example, at line 129, define clearly the utilized circular seasonality index (r) and the seasonality ratio (SR). I see the reference to Laaha and Blooschl (2006b), but I suggest to make the manuscript self-structured.
4. The values of the relative frequency in Figure 3 are very low; the discussion cannot be appreciated very much. Maybe I would add the cumulative relative frequency or the CDF
5. Comments to table 2 at lines L241-245 are a bit confusing. What does it mean 32 to 34 cases? I don't have the total number of gauges to verify the 10%. Please, refer also to the syntax used in the header of the table, i.e. 1st and 3rd
6. Please, make clear in the text among which variables the correlations are computed (section 4.2.1)
7. Please, note that Figure 4 is neither recalled nor discussed in the text. Add comments. Define the Relative error. Which return period does it refer to?
8. Figure 6: be consistent with the shown variables (relative error not defined, performance gain not defined)

TECHNICAL CORRECTIONS

Please see in the following technical observations:

- L70: "Extreme events"
- Figure 1: make consistent the red colors (line and circles)
- Figure 2: please add the total number of the analyzed gauges, even in the text.
- Table 1: please add the symbol rd_T of the relative deviation.

- I would swap Table 1 and Table 2, since this last is discussed earlier.

RESPONSE

I thank the reviewer for the very positive and constructive feedback. Please find my responses below.

1. The title suggests that a part 2 exists. If so, please refers to it in the manuscript. What does it deal with?

Thanks, now mentioned in the abstract and in the introduction (L 60):

In a two-part series we aim to fill this research gap. In this first part we propose a mixed distribution approach for low-flows to perform frequency analysis in catchments with a mixed summer/winter regime. The method is based on an independence assumption which is explored in the second part of the series \citep{Laaha_copula_2022}, where we address possible seasonal dependency using a copula-based estimator.

2. I suggest to include a figure that synthesizes the seasonal characteristics of the analyzed dataset, e.g. empirical cumulative distribution of the observed low-flow values, mean monthly series, so to appreciate the effectiveness of the mixed proposed approach.

The seasonal characteristics are characterized in Figure 5 and a detailed assessment is given in Laaha and Blöschl 2006ab. I am therefore reluctant to add "more of the same" information to this paper. However, I see your point and suggest adding an additional Figure with regime plots (monthly mean and monthly low flow regime) of the four example gauges of Fig. 1. This new (Fig. 3) is presented in Section 3.1 where text was added to the second paragraph:

Austria covers an area of 84 000 km^2 and is climatically, physiographically and hydrologically, highly diverse. This diversity leads to different low-flow regimes, as can be seen from the monthly hydrographs of the example gauges in Fig. \ref{figure:A1}. Catchments situated in the forelands and pre-alps have a rain-dominated regime whose variability is strongly modulated by sustained baseflow at the Weg and Ebensee gauges. Alpine catchments show a snow-dominated regime that is quite pronounced for the Schönenbach and St.Peter-Freienstein gauges. Overall, the low-flows in the eastern lowlands occur mainly in summer and are the result of a seasonal water balance deficit …

3. Sometimes variables are not explicitly defined. Please, make sure of that. For example, at line 129, define clearly the utilized circular seasonality index (r) and the seasonality ratio (SR). I see the reference to Laaha and Blooschl (2006b), but I suggest to make the manuscript self-structured.

Thanks, added to the beginning of the section, which is now formulated in the following way:

Seasonality is characterized by two indices: the seasonality ratio (SR), where SR > 1 indicates a winter and SR < 1 a summer low flow regime, and the circular seasonality index (r), where a value of 0 indicates the weakest possible seasonality (events equally distributed over the year) and a value of 1 indicates the strongest possible seasonality (all events fall on the same day). For the definition of indices see \cite{laaha_seasonality_2006}.

4. The values of the relative frequency in Figure 3 are very low; the discussion cannot be appreciated very much. Maybe I would add the cumulative relative frequency or the CDF

Yes, added.

5. Comments to table 2 at lines L241-245 are a bit confusing. What does it mean 32 to 34 cases? I don't have the total number of gauges to verify the 10%. Please, refer also to the syntax used in the header of the table, i.e. 1st and 3rd

The number of catchments is mentioned in L. 178. This number is now also added to L. 241, and to the caption of Fig. 2, and reference to the syntax in the header is given.

6. Please, make clear in the text among which variables the correlations are computed (section 4.2.1)

Between the accuracy gain and three seasonality indices (see first sentence of this paragraph).

7. Please, note that Figure 4 is neither recalled nor discussed in the text. Add comments. Define the Relative error. Which return period does it refer to?

Thanks, will be included in the first sentence of the paragraph.

8. Figure 6: be consistent with the shown variables (relative error not defined, performance gain not defined)

The term error was defined as a synonym for the deviation when we place emphasis on the inferior model (L. 194). Analogously, the term gain was defined as the change in performance of the superior model compared to the inferior model (L. 192). However, to be consistent with a reviewer comment of the companion paper (part 2 in HESSD) the terminology has been slightly changed, so that the terms "deviation" (neutral formulation) and "gain" (relative merit of superior model) are consistently used throughout the text.

Technical corrections:

Many thanks, have been amended in the revised version of the MS.

Reviewer #2

This study develops a mixed distribution approach for low flow frequency analysis, particularly focused on regions that potentially have a distinct winter low flow period, due to freeze/snowfall/lack of moisture, and a summer flow flow period, due to high ET and low precipitation. The approach is built off similar mixed distribution statistics used for flood frequency analysis, but applies it in a unique way, due to the nuances of low flow. The paper first develops the concepts/statistical underpinning and then applies it to characteristic sites in Austria to show how the model responds when hydrologic drought is dominated by one season or a mix of both seasons.

Overall, I believe this is a useful and well conceived study. I list a few major comments that I believe should be addressed before this study could be published. With that said, I do believe this study has the novelty and value to ultimately be published in HESS.

I ultimately recommend a major revision.

Major issues:

1. Line 85 states that summer and winter events are independent of one another. This is a relatively strong assumption that underlies the method. I question this assumption. Given that baseflow has long persistence along with the climate drivers that create hydrologic drought, I imagine potential for strong temporal autocorrelation between summer and winter events.

Please test the temporal correlation of Summer and Winter, lagged in either direction.

To further eliminate the potential for temporal autocorrelation, I recommend checking the dates of each hydrologic drought and potentially including a buffer period. If November 1 is the division between Summer and Winter (see comment 3 below), please make sure there are no years where Summer drought occurs on Oct 25 and Winter drought occurs Nov 7, for example.

2. Overall issue - use of the word "gain" and, for example line 12 of the abstract "the error is reduced by ...". I am not convinced, if the underlying assumptions are not checked (above), that the new mixed distribution estimates are exactly correct. Therefore, this phrasing that the estimates are improved by XXX% or that error is reduced by XXXX% should be softened or modified. In the derivation of gain (Eqs 10-12), this is referred to as "change in return period" or "relative deviation". Those terms, e.g. Delta T, are more neutral, and in my opinion more accurate, than stating that error is reduced by XXXX. I would be more willing to accept the stronger wording only with centuries of simulated data.

3. Line 36 - You state that many studies have suggested defining summer from about April or May to November, and winter as remaining. But, you never state how you are defining the two seasons for the analysis in Sections 2.4 and 3. Is this the division? Is it April or May? Please provide the exact day.

4. Please provide how many years of data are used for each river (n = ?).

5. Line 242 - Please explain how the deviation can be exactly zero. I find this highly unlikely given the fitting of two distributions and then comparisons of the extreme tails of each

distribution. If this occurs because the Mixed Model defaults to the single season, explain this. Although, I doubt it would be exactly the same to 1 or 2 decimal places.

This shows up in Figure 4 as well and is confusing.

Minor issues:

Line 14 - misspelling "broade"

Line 70 - misspelling "extreme events"

Line 130 - I appreciate the link to Laaha and Bloschl 2006, but the first time you introduce SR, please insert the sentence from the caption of Fig 5 "SR > 1 indicates ..., SR < 1 indicates." Perhaps add in "Seasonality ratio ranges from XXX to YYYY".

Line 216 - spelling of Pearson

Line 270 - "Despite there is a large". Please revise for grammar

Line 285 - It looks like there are some words missing "as the have an "

Line 301 - misspelling "one"

RESPONSE

We thank the reviewer for the positive and constructive feedback. Please find my responses below.

1. Line 85 states that summer and winter events are independent of one another. This is a relatively strong assumption that underlies the method…

RE: I fully agree with this statement that the methods proposed in the paper rely on the independence assumption and there will be certainly catchments where the assumption is not strictly fulfilled. This is similar to the iid assumption of other estimators that is often not strictly fulfilled in practice, yet useful for model development.

I have therefore structured the research in a two-part paper series where the first part develops methods assuming there that dependency will have no significant effect. The second part extends the scope will explore seasonal dependency in detail. It examines the value of an extended estimator that accounts for the seasonal correlation of low-flow events and assesses its value in a hydrological context. This companion paper (hess-2022-358 A mixed distribution approach for low-flow frequency analysis – Part 2: Modeling dependency using a copula-based estimator) was submitted to HESS on 13 Oct, and is currently under review.

In the paper at hand, the effect of possible seasonal correlation has only been discussed at the end of the paper and I see from your comment that it should come earlier, where the reader starts to question the assumptions of this paper. We will therefore add at Line 90:

It should be noted that the assumption of strict seasonal independence will only be met in part of the catchments, while there will be cases where some dependency of seasonal minima exists. This is explored in detail in the second part of this two-paper series \citep{Laaha_copula_2022}, where we *examine the value of an extended estimator that accounts for the seasonal correlation of low-flow events.*

In response to the reviewer's comment, and the editor's comment to make the paper self-contained, I further tested the temporal correlation of Summer and Winter events as suggested by the reviewer. However, I see less added value in performing an analysis of the data for each hydrologic drought and possible buffer period. Such analysis would be equivalent to an inter-event criterium in the threshold level approach, which however is not performed in the methods of this paper. We here do not distinguish drought periods so we cannot analyse if they are interrupted. This would only give one (of many further) possible indications of dependency but will not provide a straightforward method to eliminate the potential for temporal autocorrelation as suggested by the reviewer comment. Seasonal correlation may not only be evidence of the occurrence of rain-to-snow season droughts (i.e. summer-to-winter droughts), but also a more general indication that the watershed has not fully recovered from the summer event when the winter event occurs. As seasonal correlation is a more general measure of dependence then some (threshold-dependent) inter-event time criterion, it is consistently used in the MS.

These important points raised by the referee are now discussed in the last two paragraphs of the paper (Line 340 of revised MS), which were reformulated accordingly:

Although the distinction between summer and winter low-flow events is more straightforward, the assumption of their seasonal independence is a strong one that is unlikely to be met in a number of catchments. Baseflow has a long persistence along with the climate drivers that generate hydrological drought, giving potential for strong temporal autocorrelation. This may for example occur in large watersheds with a high storage capacity and, therefore, a particularly long time of recession and recovery at the end of the drought. To what extend the assumption of seasonal independence is fulfilled can be inferred from the correlation between summer and winter events. In the case of the Austrian study area, two third of the catchments show a highly significant seasonal correlation (46 \% at the $\alpha =0.01$ level) or significant seasonal correlation (22 \% at the $\alpha =0.05$ level) when using the Spearman coefficient as a reference. Such correlation may be evidence of the occurrence of rain-to-snow season droughts \citep{van_loon_hydrological_2015} or simply that the watershed has not fully recovered from the summer event when the winter event occurs. Both will have an effect on the mixture of distributions. We therefore see that the estimator could be further improved by taking into account the correlation structure of the events, which would be a valuable extension of the method for cases where seasonal correlations occur. Such an approach appears to be a natural extension of the mixed probability estimator and will be explored in the companion paper to this study \citep{Laaha_copula_2022}.

Apart from this limitation, the mixed distribution approach provides one consistent approach for summer, winter, and annual events that is more accurate than the traditional annual minima estimator. Because of all its beneficial properties, it should be used in the analysis of low-flow frequencies in climates with warm summer and cold winter seasons, under conditions where mixed seasonal low-flow regimes occur.

Finally, I need to note here that the correlation is explored in much detail in the second part of the study. Importantly, the results show that the correlation occurring in a number of catchments have only impact on the estimates of mild (e.g. 2-year) low flow events, which are usually of less interest. For severe events the difference is negligible what supports the validity of the estimates of this study (and supports the interpretation of "deviation" being a "gain" based on being a valid generalization of the common estimator).

2. Overall issue - use of the word "gain" and, for example line 12 of the abstract "the error is reduced by ...". I am not convinced, if the underlying assumptions are not checked (above), that the new mixed distribution estimates are exactly correct…

I see your point, but argue from the findings of the companion paper (where not only the assumptions, but also their impact on the estimates have been checked) that for the return periods we are usually interested in, the differences between the mixed and the extended mixed copula estimator are negligible so that the mixed distribution approach is indeed a valid and accurate estimator of the low-flow event.

To be also consistent with a reviewer comment of the companion paper (part 2 in HESSD) the terminology has been slightly changed, so that the terms "deviation" (neutral formulation) and "gain" (relative merit of superior model) are consistently used throughout the text.

Given that the terminology is also set out in Section 3.2.1, I wish to make this point clearer, by adding the following sentence (Line 212):

Note that the interpretation of the deviation as a gain depends on the (reasonable) assumption that the model is superior to the alternative model, and the terminology of the study should be interpreted as such.

3. Line 36 - You state that many studies have suggested defining summer from about April or May to November, and winter as remaining. But, you never state how you are defining the two seasons for the analysis in Sections 2.4 and 3. Is this the division? Is it April or May? Please provide the exact day.

Thanks – The period in the Austrian study starts in April and this will be added to the MS.

4. Please provide how many years of data are used for each river (n = ?).

I clarified in L. 187:

In this study we use the data set of \citet{laaha_three-pillar_2016} consisting of 329 Austrian stream gauges with measurements from the 1976 to 2010 period (Fig. \ref{figure:11}). The dataset includes 312 series with complete records and an additional 17 series with a record of at least 30 years.

5. Line 242 - Please explain how the deviation can be exactly zero. I find this highly unlikely given the fitting of two distributions and then comparisons of the extreme tails of each distribution. If this occurs because the Mixed Model defaults to the single season, explain this. Although, I doubt it would be exactly the same to 1 or 2 decimal places.

Yes, it occurs as you said because the Mixed Model defaults to the single season and this will be added as follows:

These refer to the strongly seasonal cases where the mixed model is based on a single season, so that the mixed and annual models coincide (Fig. \ref{figure:1}d)

Minor issues:

Many THANKS, they have been amended.