Reply to Reviewer 1

We would like to thank William Farmer for the time dedicated to our paper and for his comments that will contribute to improve the paper.

In the following, reviewer's comments are in Italic (R1), Authors' comments are in normal text (AC). Moreover , authors' changes in manuscript based on comments of all referees are summarized at the end of this document.

R1: This work could benefit from a clear statement of hypotheses. In my opinion, the main hypothesis is that the daily flow duration curves at an ungauged location can be simulated with knowledge of the precipitation record at both the ungauged site and some index site. This hypothesis relies on a further assumption that the cumulative distributions of streamflow and precipitation correlate in some way. In the revisions I am proposing, I think the authors should clearly set out to quantifiably address these hypotheses. A figure may improve the understanding of the methodology.

AC: We thank the reviewer for the suggestion, We thank the reviewer for the suggestion, we will clearly state the hypothesis of the work (see also comments below) and a figure will be added to give a clearer explanation of the procedure.

R1: As a previous commenter noted, the approach is difficult to understand and may be greatly simplified. The authors create cumulative distribution functions (CDFs) for streamflow and API at both an index site and a target site over some reference period. They then create a CDF of streamflow and API at the index site for some target period and a CDF for API at the target site for this same target period. The method then only uses the CDFs of (1) API at the index site in the target period, (2) API at the index site in the reference period, and (3) streamflow at the target site in the reference period.

AC: We thank the reviewer for the comment, we will change Sect. 3 to make it clearer and shorter and we will remove redundant parts.

R1: In addition to improving readability, revising the methods section might also address the concerns raised by the previous commenter. Namely, that it seems the approach could be greatly simplified through interpolation along the relevant relationships without the need for intermediate exceedance probabilities. This would be accomplished by the following: (1) Create the CDF of API at the index site in the target period, (2) Plot the API of the index site in the reference period against the streamflow at the target site during the reference period, (3) interpolate each API from the target period, in order, along the curve created in (2) to produce the CDF of streamflow at the target site in the target period. While this approach is achievable algorithmically, and identical to the one proposed, it raises several concerns about the implicit assumptions. "Are API and streamflow ranked independently? (See step 3 of page 10.) If so, the implicit assumption is that the exceedance probability of the API on a given day is equivalent to the exceedance probability of the streamflow on that same day at the same site. This is a pretty sizeable assumption. As a start, it would be good

to see if the temporal sequence of API exceedance probabilities is highly correlated with the temporal sequence of streamflow exceedance probabilities at a single site over the same period."

AC: We thank the reviewer for the comment, the methodology section will be largely modified and a clear statement of the hypothesis will be provided. Specifically, it will be shown that the temporal sequence of API exceedance probabilities is highly correlated with the temporal sequence of streamflow exceedance probabilities at a single site over the same period. For instance, in the following a table showing this correlation is reported for Blanco River (USA) for four different periods of time.

Period	correlation
1948-1968	0.978
1968-1988	0.995
1948-1963	0.998
1948-1958	0.970

R1: The second assumption arises when we move from the CDF of API at the index site in the reference period to the CDF of streamflow at the target site in the reference period. This movement introduces a second implicit assumption: namely, that the exceedance probability of the API on a given day at the index site is equivalent to the exceedance probability of streamflow on that same day at the target site. Put another way, if you accept the assumption in the previous paragraph, this step assumes that the temporal sequencing of API is identical at both sites. Again, this needs to be demonstrated: Is the temporal sequence of API exceedance probabilities at the index site highly correlated with the temporal sequence of streamflow exceedance probabilities at the target site in the reference period? It may be argued that the temporal sequencing is irrelevant. This is not the case. By assuming the same exceedance probabilities in step 7 of page 10, we are assuming a perfect correlation and, therefore, assuming a temporal correspondence.

AC: We thank the reviewer for the comment, in the revised manuscript we will show that the temporal sequence of API exceedance probabilities at the index site is highly correlated with the temporal sequence of streamflow exceedance probabilities at the target site in the reference period. As an example, in the following a table reports the correlation between Blanco (index site) and three target sites (specified below) for four different reference periods.

Sites	1948-1968	1968-1988	1948-1963	1948-1958
Tangipahoa	0.996	0.997	0.994	0.997
Choctwhatchee	1	1	0.999	0.999
Bogue	0.990	0.992	0.995	1

R1: The third assumption, which was alluded to earlier, is that the CDF of API is identical across sites for both the index site and the target site in the same period. This is what allows the authors to use the CDF of the API of the index site in the target period for step 6 on page 10. It may be that this is what the authors meant by "the assumption of large scale precipitation" (line 16, page 9); if so, please clarify. Regardless, this assumption needs to be validated through correlation or a KS test.

AC: We thank the reviewer for the comment, in the revised manuscript we will show that the CDF of API is identical across sites for both the index site and the target site in the same period through correlation. In the table below an example of correlation between Blanco (USA) and three other sites is reported for four different time periods.

Sites	1948-1968	1968-1988	1948-1963	1948-1958
Tangipahoa	0.98	0.99	0.97	0.99
Choctwhatchee	0.98	0.98	0.98	0.98
Bogue	0.99	0.99	0.98	0.99

The distributions of API have the same type of distribution – Weibull is accepted for all of them. On the other hand the distribution parameters may differ from site to site and from time period to time period. For instance, the Weibull is the best fitting distribution of the API at Blanco for the periods 1948-1968, 1968-1988, 1948-1963 and 1948-1958. The same applies to the API of three sites for the same periods as it is shown from the table below.

Sites	1948-1968	1968-1988	1948-1963	1948-1958
Tangipahoa	Weibull	Weibull	Weibull	Weibull
Choctwhatchee	Weibull	Weibull	Weibull	Weibull
Bogue	Weibull	Weibull	Weibull	Weibull

R1: Without some quantifiable validation of these assumptions, the proposed method is tenuous at best and left vulnerable to criticism. With that in mind, and the comments of the previous commenter, I'd like to propose that exploring these assumptions might result in modifications of the methodology that might move away from the case of simple interpolation. Is the relationship between API and streamflow constant across periods or sites? Should API and streamflow be ranked independently or with some sort of dependence? Should the API of the index site in the target period be used to map to a different site in a different period (i.e., the target site in the reference period)? Exploring these questions, and validating the underlying assumptions, will produce a more robust approach."

AC: Thank you for this comment. We assume that the relationship between API and FDC is the same for the same site regardless the time period. This assumption has to be justified, and we'll add a paragraph on this issue to the revised manuscript.

R1: "In addition to their main hypotheses proposing this methodology, the authors assert that the FDC is a product of the basin and the weather. This is surely intuitive, but the evidence provided could be greatly strengthened. The authors use KS tests, but is unclear how they were applied. It would be informative to clearly communicate if the CDF of streamflow from one period and the CDF of streamflow from another period could be considered significantly different. The authors have done this, but the presentation is not clear. The extension would be to ask if the API can be correlated with any differences across time. (As an aside: Was there any discussion of selecting stationary sites? How would nonstationary behavior play a role here?)"

AC: The FDC seems unfortunately to be significantly different from one time period to the other. The same applies to the API. In our opinion this is not caused by non stationarity of the time series but

more to some long memory effects. This of course has to be checked more in detail and a corresponding discussion will be added to the paper.

R1: This, in my opinion, raises another concern: The authors seem to be attempting to simultaneously address two very different problems. The first problem considers a target site that has a streamflow record overlapping with an index site, but the desired period has no overlap (the ungauged area is the same site, different period). In this case, the use of APIs within site, without an index site, would be most ideal. The second problem considers a site without any streamflow information; this situation necessitates the use of an index. Of course, when there are gaps in the API record as well, this transforms into four unique problems. Regardless, if we believe the underlying assumption that the CDF of streamflow is a product of basin and weather, then the solutions to these problems must be quite different. The first asks if knowledge of new weather can produce the CDF of streamflow, while the second alters both variables and asks if the CDF relationship can be transferred across weather and basin. Line 8 of page 3 implies that both problems are considered, but the remainder of the paper seems only to address the partially gauged site. I would advise addition of the second problem or, at least, a discussion of implication for the second problem (completely ungauged).

AC: We thank the reviewer for the comment, the methodology explained in the paper needs to be applied to partially gauged basins, we will eliminate the reference to totally ungauged sites in the paper.

R1: In 1996, Hughes and Smakhtin (<https://doi.org/10.1080/02626669609491555>), among others, provided a technique for hydrograph simulation using flow duration curves. While their focus was on hydrographs, the extensions to ungauged FDCs can be made quite clearly (i.e., they could be derived from simulated hydrographs). Smakhtin and Masse (2000: <https://doi.org/10.1002/(SICI)1099-1085(20000430)14:6%3C1083::AID-HYP998%3E3.0.CO;2-2>) then extended this method to use a precipitation index. While I believe that the methods presented here are different, the novelty of this new method must be strongly articulated.

AC: The two papers recalled by the reviewer are very interesting and we will recall them in the Introduction section, strongly stating which is the novelty behind our work. We will add the following sentences in the manuscript:

"Smakhtin and Masse [1] used the weather at a donor site to extend the daily hydrograph at a destination site through the monthly FDC of the destination site itself. The monthly FDC at the "destination" site is found using different methods such as (i) regionalization of FDCs based on available observed records from several adjacent gauges Smakhtin et al. [2] or (ii) conversion of FDCs calculated from monthly data into 1-day FDCs (Smakhtin, [3]). The procedure presented by Smakhtin and Masse [1] is an extension of a previous work proposed by Hughes and Smakhtin [4] to extend and/or filling in daily flow time series. The drawback of the procedures proposed by Hughes and Smakhtin [4] and Smakhtin and Masse [1] is the necessity of retrieving the monthly FDC of the target site with well-known literature methods before applying the methodology to extend the hydrograph. While the novelty of the approach we propose is the possibility to retrieve the FDCs at partially ungauged sites from weather recorded at a reference catchment. Thus, without the need of applying procedures such as the regionalization."

R1: "I strongly encourage the authors to revisit the style of the manuscript. At times, it feels a bit disjointed and it may be improved by enforcing a strict Introduction-Methods-Results-and-Discussion (IMRAD) format. For example, section 3.1 is ostensibly a methods sections but presents a series of results that I think are pivotal to the paper (line 19, page 8). Similarly, the paragraph on page 13 and section 4.1 present new methods of analysis that have not been presented earlier in the paper. While IMRAD is not a requirement, I do suggest thinking carefully about the best approach to presenting the narrative."

AC: We thank the reviewer for the suggestion, a restyle of the paper will be performed. The results reported in Sect.3.1. are pivotal to the development of the paper, they will be moved under a paragraph named "Preliminary analysis".

The presentation of the performance criteria will be moved into the Methodology

R1: "In my opinion, this work needs more presentation and discussion of quantified results.

AC: In the updated version of the paper we will further develop the discussion.

R1: "The results sections heavily rely on visualization. Even the presentation of metrics in section 4.1 is visual. While this is useful, we still need to see some discussion on the performance metrics. For example, the scale on NSE in Figure 11 makes all positive values appear as a single color. This presentation means we can't honestly see how the methods perform."

AC: We chose to present results in a visual style to better show them. Because of the large number of sites and the large number of time windows we have investigated, to a reader it would take too much time to go through a tabular presentation, which may be also boring, while plots have an immediate impact. However, we agree with the reviewer regarding the scale of the plots, that sometimes make difficult to understand the goodness of the results. We will solve this issue in the paper, moreover we will add a deeper discussion of the results.

R1: Page 1, line 17: When talking about general duration curves, more commonly known as cumulative distribution functions, it is better to say "exceedance frequency" rather than "exceedance time".

AC: We will replace the word as suggested.

R1: Page 2, line 1: Please provide the citation for the Weibull plotting position.

AC: The citation will be added (i.e., Weibull, W., 1939: A statistical theory of the strength of materials. Ing. Vetensk. Akad. Handl., 151, 1–45).

R1: Page 3, line 4: Please provide more discussion and literature of this important point.

AC: The sentence refers to the results of the paper anticipating them for the readers. A deeper discussion is provided in Sect. 3.1. We will made it clearer in the text: "Results show that the FDC is not a property of a specific basin, because different time windows (such as decades) relate to different FDCs."

R1: Page 3, line 7: It is not clear what the "distribution of the FDC" is. The FDC is a distribution, so it is confusing to talking about the distribution of a distribution.

AC: We rephrased the sentence as "It is not possible to develop relations between basin parameters and FDC characteristics to yield synthesized FDCs where flow data are not available, as done for instance by Quimpo et al. [5]."

R1: Page 4, line 6: Florida, Louisiana and Texas are certainly not the East coast. I would suggest the Gulf Coast.

AC: We will rephrase the sentence

R1: Page 5, line 7: misspelling of database

AC: We will carefully check the English spelling throughout the paper.

R1: Page 5, line 2 (?): This is an example of inconsistent citation style. Bloeschl should be in parenthesis.

AC: We will carefully check the citation style throughout the paper.

R1: Page 8, line 5: Please provide citation to KS test.

AC: We will provide the citation (i.e. Massey, F. J. The Kolmogorov-Smirnov Test for Goodness of Fit. Journal of the American Statistical Association. Vol. 46, No. 253, 1951, pp. 68–78.).

R1: Page 9, line 10: What lead to this choice for alpha? (Also, note that the same symbol is used earlier in this section for significance: page 8, line 10.) Please provide citation or summary of initial exploration.

AC: We will provide different symbols for the two. When α tends to zero, API keeps tracks of the precipitation occurred in the few previous days and it represents the short memory of the basin. When α tends to 1, API represents the long memory of the basin as it includes the effect of precipitation occurred many days before. To capture this behavior, in this study α is chosen equal to 0.85. Moreover, this is in agreement with a previous study [6] which investigated the same case study area (i.e. Neckar catchment). This will be specified in the paper.

R1: Page 9, line 18: I strongly suggest referring to the "reference site" as an "index" or "donor". The reference connotation implies lack of human influence that might be confusing. The same could be said of the reference period

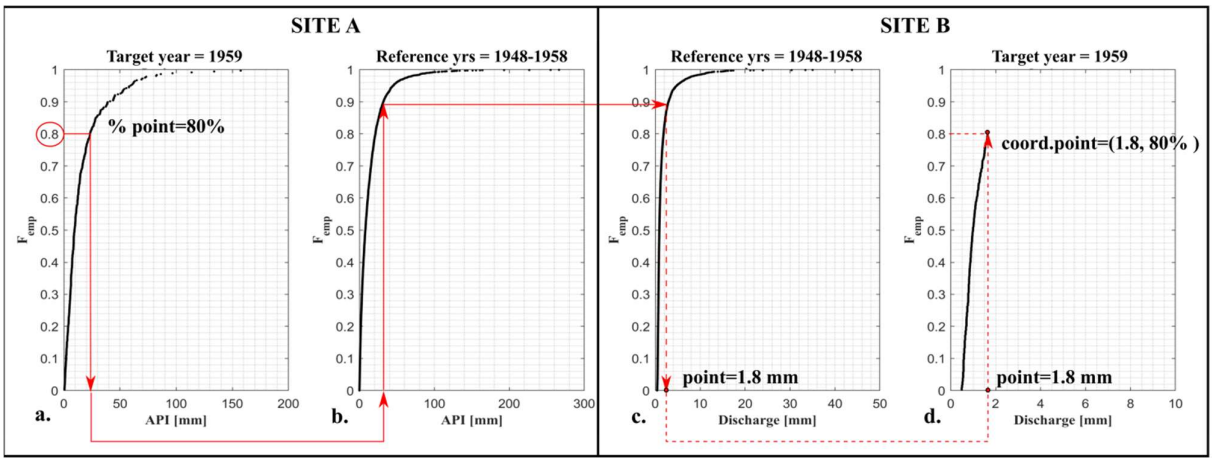
AC: We will use "donor" site instead of "reference" one.

R1: Page 10, line 17: The series of Nr and Nt are both being indexed with i, which leads to confusion.

AC: Since the reference series of both basins A and B have the same size, we will index the reference series (Nr) with *i*. The same applies to target series of both A and B, therefore we indexed the target series (Nt) with *j*. Moreover, we will consistently revise the Methodology section.

R1: Page 10, line 19: So, API_Ati is equal to API_Arj?

AC: Yes, it is. We will add the following Figure to better clarify the methodology and also we will re-organize and strongly improve the methodology section.



R1: Page 11, line 4: What is a supporting variable? This is not described as such earlier, so it surprises the reader.

AC: The supporting variable is the one used to retrieve the streamflow values, in this example is the API. We will better introduce it to be clearer.

R1: Page 11, line 17: "good agreement" is very subjective. Please provide thorough, quantifiable analysis. For example, a lot of the curves in figure 6 look rather poor for highs and lows (top row, second box from the left).

AC: At lines 17 and 18, the sentence is an introduction to the extensive explanation presented in the following lines of the manuscript. In the following, we comment each Figure, explaining weaknesses and strengths of each one. Thus, specifically, we comment the effect of choosing a small or large time period as reference period. Regarding Figure 6, we explicitly say that "The higher agreement in shape than in value is more evident when a small data set is considered as target period. For instance it results when the target period is 1 year, Figure 6. It is interesting to note that, even if for some target periods the simulated FDC does not perfectly match the observed one, the two FDCs have the same shape.

R1: Figure 5: Why was the box for ref:68-88 and tar: 88-98 not included? The caption needs to do a better job of describing the different panels.

AC: The missing panel would not add more information to the paper, therefore because of the lack of space the panel was not included. We will better describe the panels in the caption.

R1: Page 12, line 9: Spelling of FDCs

AC: We will carefully check the spelling throughout the paper.

R1: Page 13: The methods for this paragraph were very unclear to me. Could a figure or a revision help?

AC: In this paragraph we compute the moving average to show that the between-year variability of the discharge of a specific percentile can be high. Therefore, this suggests that percentiles cannot be considered an invariant characteristic of the basin and thus they cannot be estimated using geographic and morphologic characteristic of the basin only. We decided to show the moving averages of these percentiles as they are the most used ones.

R1: Page 14: Please provide the citation for NSE. Even better, a metric like KGE might be more appropriate

AC: The citation will be added. We used the NSE, despite the criticism it has received (e.g.,[7]) because of the familiarity most hydrologists and meteorologists have with it [8], facilitating the interpretation of the obtained values.

R1: Figure 10: What is the horizontal axis of this figure?

AC: Figure 10 will be replaced with a revised figure

Author's changes in manuscript

Some hints regarding authors' changes in the manuscript have been already given in the comments section. In the following, a summary of authors' changes in manuscript based on comments of all referees is given:

1. In the Introduction, we will clarify the novelty and the contribution of the paper.
2. The Methodology section will be reorganized and improved to provide a clearer description of the method. For the sake of clarity, a figure will be added.
3. We will provide a clear statement of the hypothesis.
4. The sections will be organized on the base of the Introduction-Methods-Results-and-Discussion (IMRAD) format.
5. For both case studies we will use the same performance criteria and results will be discussed in-depth.

References

[1] V.Y. Smakhtin, B. Masse, Continuous daily hydrograph simulation using duration curves of a precipitation index, Hydrol. Process. 14 (2000) 1083–1100. doi:10.1002/(SICI)1099-1085(20000430)14:6<1083::AID-HYP998>3.0.CO;2-2.

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[6] T. Sugimoto, Copula based Stochastic Analysis of Discharge Time Series, 2014.

[7] H. V. Gupta, H. Kling, K.K. Yilmaz, G.F. Martinez, Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling, J. Hydrol. 370 (2009) 80–91.

[8] D.N. Moriasi, J.G. Arnold, M.W. Van Liew, R.L. Bingner, R.D. Harmel, T.L. Veith, Model evaluation guidelines for systematic quantification of accuracy in watershed, Simulations, Trans. Am. Soc. Agric. Biol. Eng. 50 (2007) 885–900.