MS Title : Use of streamflow indices to identify the catchment drivers of hydrograph
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Responses to Reviewer Comments

We sincerely thank Editor Elena Toth, Dr. Wouter Knoben, and the anonymous reviewer for reviewing the manuscript and offering valuable critical comments to improve the manuscript. Their suggestions have significantly improved the quality of our contribution to this revised manuscript. We provide here our responses to their comments.

The line numbers mentioned in "Response" correspond to the "Clean version" of the revised manuscript.

Reviewer 1:

I thank the authors for taking into account my comments to their original manuscript. I think the manuscript significantly improved in the method section. However, I still have some comments on the results and discussion section. Please find my comments below.

1. A clear 'take-home message' of the paper is, in my opinion, missing. What is the message that the authors want to convey with this analysis? Perhaps that streamflow indices of falling/rising limbs are governed by similar/ very different processes in different regions across US? I suggest highlighting the message of the paper in the abstract and in section 5. If the focal point of the paper is on time asymmetry of the streamflow indices, I would also mention it in the title.

Response: We are grateful to you for your insightful recommendations and comments. The primary goal of this study is to bring out the distinction between signatures directly linked with rising limbs and falling limbs and their utility in uncovering processes associated with the hydrograph's steeper ascending and gradual descending limbs. A set of streamflow indices at the catchment scale for 671 basins in the Contiguous United States (CONUS) is also introduced here. These streamflow indices complement the catchment attributes provided earlier (Addor et al., 2017) for the CAMELS data set. We have highlighted this concept in the abstract as well as in Section 5 (Concluding remarks).

2. The abstract is unbalanced. The first half of the abstract is dedicated to an introductory explanation of streamflow indices and time irreversibility, while the current work is described only starting from line 15. I suggest shortening the first introductory part (lines 8-15) and adding a couple of sentences about the actual results and findings of the analysis.

Response: Thank you for this valuable suggestion. We recognize that the abstract was unbalanced due to a lack of discussion of the conclusions and findings. We have now modified the abstract (lines 8-24) in the revised manuscript and shortened the introductory lines as suggested.

3. Line 141: How are events/hydrographs with multiple peaks treated? Do you identify multiple (short) rising and falling limbs or you exclude multiple peaks?

Response: We apologize for the lack of clarity in this aspect. We did not use rainfall data to identify the events and thereby limbs of the hydrograph in this investigation. The identification of rising limbs and falling limbs is illustrated through an example as follows: Suppose there are 10 days and corresponding streamflow states as follows:

Days	1	2	3	4	5	6	7	8	9	10
States	1	1	0	0	0	0	1	1	1	1

State 1- represents a wet day, and state 0, a dry day. In the above example, a rising limb (first two days) is followed by a falling limb (next four days), and then it is further followed by a rising limb of four days. If there is no increase or decrease in the flow with respect to the previous day, it is reckoned as part of the recession limb. We, therefore able to identify multiple peaks of the hydrograph.

4. Line 222: check figure number

Response: Thanks for pointing out this mistake. We have corrected the figure number in the revised manuscript (line 238).

5. Line 227: this sentence is unclear

Response: We apologize for the lack of clarity in the text. Jehn et al. (2020) summarize the characteristics of each catchment cluster in terms of climate, hydrology, and location. The sentence is now modified (lines 247-251).

6. Line 230: how are streamflow indices transformed?

Response: We apologize for the lack of clarity in the content. The word "transformed" was wrongly used by us. The sentence is now modified in the revised version (see lines 246-252). We aim to explain that Jehn et al. (2020) summarize each catchment cluster's climate, hydrology and location characteristics. Since the ten clusters encompass catchments with a diverse variety of hydrological characteristics and spatial patterns across the United States, straightforward interpretations of the observations to explain the hydrologic behavior in each cluster are easily facilitated. As a result, we used this idea to categorize streamflow indices into groups that indicate different hydrological behavior, making it much easier to interpret hydrological processes.

7. Line 234-235: it's unclear whether you are describing the work of Jehn et al. (2020) or your work.

Response: Thank you for pointing this out. We have added Jehn et al. (2020) reference to this description and restructured the corresponding paragraph (lines 246-252).

8. Line 229-232: these two sentences look like a repetition to me. Please consider deleting one.

Response: Thank you for the suggestion. We have deleted lines 227 and 232 and restructured the corresponding paragraph (lines 244-252).

9. Section 4.1: in this section, the authors describe, cluster by cluster, the values of the streamflow indices and of the corresponding catchment descriptors. While maps and boxplots of streamflow indices are shown in fig. 5,6,7,8, the spatial patterns of catchment descriptors are not shown here. This makes the section hard to follow. I suggest producing some additional visuals, showing streamflow indices and catchment descriptors together, for each of the clusters (e.g. as boxplots).

Response: We thank you for this advice. We have now added the boxplots of the attributes of the clusters (Jehn et al., 2020) in the Supplement (Figure S3) to describe the regional patterns in CONUS.

10. Section 4.1: the description of the spatial patterns is repeated, in every paragraph, using geographical regions and clusters (also having a spatial meaning). In my opinion this makes this section unnecessarily long and hard to follow. Using regions or clusters in the description would suffice.

Response: Thank you for raising this concern. The idea of representing streamflow indices as spatial maps with respect to geographical regions aligns with spatial maps introduced by the CAMELS dataset. One of our contributions to this study itself is generation of the streamflow indices maps.
Secondly, the purpose of using clusters is to make a comparison with the existing catchment classification literature. We believe that we can thus link the present work to the previous studies carried out in the CONUS region.

11. Line 245-246: this sentence (related to high forest proportion) seems to me in disagreement with the fact that you observe high rising limb density. The same for low aridity.

Response: Thank you for this correction. We have now removed the lines 245-246. Please refer lines 253-275 in the revised manuscript. The interpretation for higher and lower rising limb density is provided below:

The Appalachian Mountains (Cluster 10), Southeastern and Central Plains (Cluster 1), and all Southernmost states of the US (Cluster 9) witness high rising limb densities (Fig. 6.a).

It is noted that Cluster 1 is characterized by dense vegetation cover and low elevation resulting in smaller annual snowfall. Cluster 10 catchments have a higher mean elevation than most other clusters, experiencing low aridity and high forest cover. However, Cluster 9 encompasses all of the United States' southern states, with lower precipitation seasonality and higher forest cover and green vegetation. Furthermore, all of the catchments in Cluster 9 are very near the sea, with a low snow component and high evapotranspiration.

It is further noticed that the rising limb density shows a negative correlation (Table 2) with the area ($r = -0.30$), elevation ($r = -0.20$), fraction of precipitation falling as snow ($r = -0.33$), and depth to bedrock ($r = -0.32$).

Northwestern Forested Mountains (Clusters 3, 4), located in the mountains of the western US, experience low values of rising limb density. The catchments of Cluster 3 have the largest snow storage in the dataset. Cluster 4 is found in the western United States' mountains, where there is a significant fraction of snow, same as Cluster 3. So, low values of rising limb density are observed due to a negative correlation with the fraction of precipitation falling as snow with rising limb density ($r = -0.33$).

The study's findings indicate that rising limb density is mainly governed by elevation and fraction of precipitation falling as snow in the CONUS region.

12. Lines 248-251: these two sentences look like a repetition to me. Please consider deleting one.

Response: Thank you for this suggestion. We have removed the repetitive sentence in Section 4. Please refer lines 270-275 in the revised manuscript.

13. Line 252-253: this sentence seems to contradict the previous sentence. One contradiction concerns the season: the cluster is dominated by summer events, but the authors write that snow on the ground may be the cause of low values of raising limb density, which would imply a different event seasonality. Is it plausible that ground is covered by snow in summer in this region? Also 'quick snowmelt' and 'long-lag time' contradict each other.

Response: Thank you for pointing out the mistake. We have now removed the lines 248-253 (older version) and rephrased them as in the lines 270-275 in the revised manuscript. The interpretation for lower rising limb density is provided below:

Northwestern Forested Mountains (Clusters 3, 4), located in the mountains of the western US, experience low values of rising limb density. The catchments of Cluster 3 have the largest snow storage in the dataset. Further, Cluster 4 in the Western United States' mountains is characterized by a significant fraction of snow, like Cluster 3. So, low values of rising limb density are observed due to a negative correlation with the fraction of precipitation falling as snow (r = -0.33).

14. Line 257: this sentence is unclear. The shape parameter reflects the flashiness of the increasing limb. The fact that low (average) discharge values are observed does not tell anything about the shape parameter.

Response: Thank you for this comment. We would like to clarify that lines 254-267 describe the spatial variation of rising limb scale parameters, not the rising limb shape parameters. In this section, we want to highlight that the higher discharges can create higher values of rising scale parameters as the rising limb scale parameter regulates the magnitude of the rising limb.

15. Line 283-284: why does forest cover correlates positively with falling limb density? What is the hydrological concept behind?

Response: Thank you for raising this question. We are not stating that forest cover positively correlates with the falling limb density.

We are trying to highlight that Clusters 4, 2, 8 over Northwestern Forested Mountains, Central Plains, Great Plains, and North American deserts characterize higher magnitudes of falling limb density, whereas Clusters 6, 7 over Marine West Coast Forests and Western Cordillera experience smaller falling limb densities (in lines 279-284 of older version).

Our interpretation is that the falling limb density positively correlates with the arid climate (r = 0.39) and is negatively correlated with the forest cover (for LAI maximum (r = -0.37) and green veg frac max (r = -0.40, Table 2). Mean elevation (r = 0.55) also strongly characterizes the nature of the falling limb density. Moreover, the fraction of precipitation falling as snow (r = 0.42) is also positively correlated with falling limb density.

We have now included this discussion (lines 309-316) in the revised manuscript).

16. Table 3 and table 4: The authors have rotated direction of the numbers in the table. What I actually wanted to suggest with my previous comments, was to transpose the entire table, i.e. having the catchment descriptors in the rows and the streamflow indices in the columns. I would also suggest merging the two tables.

Response: Thank you for the valuable remarks, and apologise for not addressing your comment correctly earlier. We have now modified the table by merging Tables 3 and 4. Please see Table 2 in the revised manuscript. We have also transposed the table with catchment descriptors in the rows and streamflow indices in the columns in the revised manuscript. We have provided correlation coefficient (r) values, and corresponding (*p*) values.

Reviewer 2:

1. I would strongly recommend to integrate Section 4.2 into Section 4.1, to directly connect the cluster descriptions from Jehn et al. (2020) with evidence from the CAMELS data itself. For example, the sentence above could read:
"A high frequency of high precipitation, on the other hand, can result in more rising limbs and higher rising limb densities (r = 0.08; see Table 2)."
It is then up to the authors to determine if the correlations visible in the data are strong enough to warrant keeping the hypothesis about catchment conditions and hydrograph behavior included in the main text.

Response: We sincerely thank you for all your critical comments which sharpened our thought process and for allowing us to revise our manuscript.

We have integrated Section 4.1 and Section 4.2 and rephrased the discussion section (lines 221-388), and show correlations, along with the *p*-values to justify the hypothesis with catchment conditions and hydrograph behavior.

We have also merged Tables 3 and 4 by transposing the table with catchment descriptors in the rows and streamflow indices in the columns, as suggested by Reviewer 1. Please see Table 2 in the revised manuscript.

2. Section 4.1. divides the catchments in this study into high and low categories on each of the 6 indices. This seems related to the color scheme used in Figures 5 and 7 (each sub figure has 6 categories). It is unclear to me how the color coding in this figures was determined, as the color scales are not evenly distributed across the 3 low and the 3 high categories:

- Figure 5a
o Range spanned by the 3 low categories: 0.25
o Range spanned by the 3 high categories: 0.34
- Figure 5b
o Range spanned by the 3 low categories: 0.76
o Range spanned by the 3 high categories: 3.92
- Figure 5c
o Range spanned by the 3 low categories: 0.23
o Range spanned by the 3 high categories: 0.87
- Figure 7a

o Range spanned by the 3 low categories: 0.13
o Range spanned by the 3 high categories: 0.30
- Figure 7b
o Range spanned by the 3 low categories: 0.32
o Range spanned by the 3 high categories: 0.95
- Figure 7c
o Range spanned by the 3 low categories: 0.17
o Range spanned by the 3 high categories: 0.22

Can the authors clarify why these color classes are defined as they are?

Response: Thank you for raising this issue. We used the Natural Breaks classification method to plot the spatial maps (in Arc GIS software), and thus the color classes are defined as shown.

When we plot the entire streamflow indices in equal intervals, we observe that there are no values in some intervals. If there is only one value in the last highest interval, we cannot interpret and connect it with the existing catchment clusters.

We can choose several different classification methods to organize the data for the thematic mapping. With the Natural Breaks classification method, data values that cluster are placed into a single class. Class breaks occur where there is a gap between clusters. We can use this method if the data is unevenly distributed; many features have the same or similar values, and there are gaps between groups of values.

3. Section 4.3 does not connect to the main text

Section 4.3 provides brief descriptions of the correlation patterns visible in Figure 9, but does not connect these to the rest of the paper.

Response: Thank you for your comment. We have created a paragraph immediately below Section 4 to show how different sub-sections in the section are connected. The influence of streamflow indices fluctuates by climatic zones, as climate attributes influence the catchment streamflow dynamics (Addor et al., 2018; Berghuijs et al., 2014; Jehn et al., 2020; Knoben et al., 2018; Stein et al., 2021). Since the catchments are distributed in varied climatic zones (Jehn et al., 2020; Knoben et al., 2018; Stein et al., 2021), the CAMELS data is ideal for addressing this question. With this motivation, the effect of climate attributes on streamflow indices associated with rising and falling limbs is investigated here. All subsections under Section 4 are thus inter-related.

4. Recommend to change the orientation of Table 2 and 3 and merge them into a single table, where the attributes are listed in the y-direction (and therefore easy to read), while the indices are on the x-axis.

The two separate tables take up more space than needed and are also a bit awkward to read.

Response: We sincerely thank you for the valuable suggestion.

Please see our response to major comment # 16 of the reviewer #1 (reproduced here for convenience):

We have modified the table by merging Tables 3 and 4. Please see Table 2 in the revised manuscript. We have also transposed the table with catchment descriptors in the rows and streamflow indices in the columns in the revised manuscript. We have provided correlation coefficient (r) values, with corresponding (*p*) values. For *p*-value, a significance level of 0.05 is employed.

5. Correlation analysis of the CAMELS attributes themselves will show that the three vegetation attributes used in this table themselves have a very strong negative correlation with aridity (more arid, less vegetation), so there is somewhat limited additional information by including all 4. I'll repeat my suggestion to investigate the relations between CAMELS attributes and remove redundant attributes from the analysis in this paper.

Response: We agree that the correlation analysis of the CAMELS attributes will show that the three vegetation attributes used in this table themselves have a very strong negative correlation with aridity. Each attribute (e.g., climate vegetation, soil, geology) usually does not exist independently in space but is closely interwoven, resulting in various strongly correlated attributes in a catchment (Jehn et al., 2020; Stein et al., 2021). However, it would be beyond the scope of this paper to describe all probable relationships between attributes. Keeping this in mind, the main focus of this study was constrained to only identify the controlling attributes of streamflow indices.

6. Section 4.1 now contains hypotheses about how certain hydroclimatic conditions in catchments influence the rising and falling limbs of hydrographs. These are however still speculative – they could be true but there is no statistical evidence shown to give us an idea of how likely these statements are to be true.
I think the paper would be a lot stronger if the authors could add a section on next steps. Guiding questions for such a discussion may be: Given that we now have these hypotheses about the relationship between catchment attributes and hydrograph properties, how would we go about testing these hypotheses? Is additional data needed? Will this require specific modeling efforts?

Response: Thank you for the valuable remarks.

We have integrated Section 4.1 and Section 4.2 and rephrased the discussion section (lines 221-388), considering the data show correlations to justify the hypothesis with catchment conditions and hydrograph behavior.

We have modified the table by merging Tables 3 and 4. Please see Table 2 in the revised manuscript. We have also transposed the table with catchment descriptors in the rows and streamflow indices in the columns in the revised manuscript. We have provided correlation coefficient (r) values, and corresponding (*p*) values are provided in the brackets. For *p*-value, a significance level of 0.05 is employed.

We have also removed the lines 245-246 and 248-253 in the older version in which our previous interpretation about high/low values of rising limb density was incorrect and rephrased them as in the lines 256-275 in the revised manuscript.

For convenience, the interpretation for higher and lower rising limb density is provided below:

We can see that the Appalachian Mountains (Cluster 10), Southeastern and Central Plains (Cluster 1), and all Southernmost states of the US (Cluster 9) witness high rising limb densities (Fig. 6.a).

It is noted that Cluster 1 is characterized by dense vegetation cover and low elevation resulting in little annual snowfall. Cluster 10 catchments have a higher mean elevation than most other clusters, experiencing low aridity and high forest cover. However, Cluster 9 encompasses all of the United States' southern states, with lower precipitation seasonality and higher forest cover and green vegetation. Furthermore, all of the catchments in Cluster 9 are very near the sea, with a low snow component and high evapotranspiration.

It is further noticed that the rising limb density shows a negative correlation (Table 2) with the area (r = -0.30), elevation (r = -0.20) fraction of precipitation falling as snow (r = -0.33), and depth to bedrock (r = -0.32).

Northwestern Forested Mountains (Clusters 3, 4), located in the mountains of the western US, experience low values of rising limb density. The catchments of Cluster 3 have the largest snow storage in the dataset. Cluster 4 is found in the western United States' mountains, where there is a significant fraction of snow, same as Cluster 3. So, low values of rising limb density are observed due to a negative correlation with the fraction of precipitation falling as snow with rising limb density (r = -0.33).

The study's findings indicate that rising limb density is mainly governed by elevation and fraction of precipitation falling as snow in the CONUS region.

We have also added the boxplots of the attributes of the clusters (Jehn et al., 2020) in the Supplement (Figure S3) to better understand the regional patterns in CONUS, as in comment 9 of Reviewer#1.

References

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