Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-307-RC2, 2021

Review of hess-2021-307

Anonymous Referee #2

Referee comment on "Use of streamflow indices to identify the catchment drivers of hydrographs" by Jeenu Mathai and Pradeep P. Mujumdar, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-307-RC2, 2021

The manuscript "Streamflow indices to identify catchment drivers of hydrograph" by Mathai and Mujumdar estimates six streamflow indices for 621 stations in the U.S. and investigates their correlation with 15 catchment attributes, taken from the CAMELS data set in a spatial context. This study aims at identifying the drivers of streamflow indices, by distinguishing indices related to the rising and falling limbs of the hydrographs, i.e., implicitly related to different processes. The idea of the study is potentially very interesting however, in my opinion, the analyses, results and discussion presented should be further expanded and developed in order to be considered suitable for publication. The manuscript is overall well written, but I have some specific comments/suggestions regarding the organisation of some sections/figures. Please find my specific comments below.

Major comments:

- Methods: the method section is partly unclear (lines 85-93) and further explanations are needed. How are the diurnal increments of streamflow obtained (line 85)? How is the Weibull distribution fitted to the data (line 86)? How are the recession coefficient b1 and b2 obtained (the description of these two indices is missing)? How are the correlation coefficients, presented in the result section, calculated? Please clarify these aspects in the method section and provide references.
- In the manuscript too much space is given, in my opinion, to the presentation of the CAMEL database and the related catchment attributes or clustering of stations. Three figures (Figure 3, Figure 4 and Figure S1) and two tables (Table 2 and Table S1) of the manuscript are directly taken from other publications (in some cases the figures are copy-pasted, and the source is cited, and in other cases the data is simply re-plotted compared to the original publication and the source is cited). I would suggest significantly reducing the room (both text and figures/tables) allocated to these "non-original" parts of the manuscript by, e.g. combining, condensing and reworking (or removing) the above cited figures and tables.

- The correlation analysis between the streamflow indices and the catchment attributes (pages 16-19, section 6.2 and 6.3), which should represent the main focus of the paper, is carried out in a bit too simplistic and superficial way. The only results presented in section 6.2 are two correlation matrices (where the authors do not specify how the correlation coefficients are calculated) and the analysis in the 'climate index space' (section 6.3, figure 9) merely consists in plotting the streamflow indices as a function of two (arbitrarily chosen) climate indices. Further and more rigorous analyses should be added (e.g. are the correlations significant?) in order to properly investigate the processes represented by the time-irreversibility-based indices and to support the authors' statements.
- The discussion of the results is poor, and section 6 is limited to the mere description of the figures/tables. A discussion or interpretation of the processes behind the obtained results would be advisable.

Specific comments:

- Lines 71-74 about the novelty of the study would better fit at the end of the introduction section.
- Table 1: there is an apparent change in terminology (i.e. "rising limb" and "ascension limb" are used in the description of different indices). Please use consistent terminology throughout the manuscript.
- Figure 2: 3 rising limbs are taken into account in the RLD denominator, but only 2 are considered in the RLD numerator. Why is that? How do the authors take into account rising/falling limbs that fall only partly into the analysed period? Please specify it also in the text of the method section.
- Figure 2: please align the labels of the time intervals to the centre of corresponding segments.
- Section 3 (contributions of the study) and Section 4 (motivation of the study) would be better placed at the end of the introduction. The description of the Camel dataset and corresponding catchment attributes (currently in Section 4) would be better placed in the Data section 5.
- Line 105: "to identify the key drivers of streamflow hydrographs" or "to identify the key drivers of streamflow indices"?
- Line 110: what do the author mean by "attribute class"? It's not clear to me.
- Line 180: "the landscape of each catchment is described using multiple attributes […]" I believe that the attributes of Table 2 are representative of broader catchment features than landscape (e.g. climate)
- Line 186-191: line numbers are mistakenly written into the last column of the table
- Line 192: how are "high precipitation days" defined?
- Section 6.1: several statements in this section are not justified by the results and figures presented by the authors. This occurs e.g., in lines 219-220 "these clusters … respectively", lines 221-222 "as these clusters … rapid snowmelt", lines 226-229 "These catchments ... forest.", lines 321-232 "This is because … regions", lines 238-239 "This is due to… regions", lines 240-241 "dominant with … snowmelt" and line 246.Please support your statements with additional figures/results.
- Captions of Figures and tables: the captions of the manuscript often contain the description of the results presented in the figure/table (Figure 4, 5, 6, 7, 8, 9 and Table 3, 4). This is not needed. Please place the description of the figure/table in the main text.
- I would suggest combining Figure 5/6 and 7/8 into one figure with multiple panels (i.e., one figure with the current Figure 5 and 6, and another figure with the current figure 7

and 8). This is because the message conveyed by figures 5 and 6 and by figures 7 and 8 is similar and complementary (they present spatial patterns and spatial clusters) and it would be easier for the reader to have comparable results in a more compact form.

- Table 3 and Table 4: Reading the tables would be easier if rows and columns were inverted (i.e. transposed table). I would also suggest merging table 3 and 4 for a more rapid and direct comparison.
- Section 6.2: the streamflow indices are here referred to as "Flow descriptors". Please use consistent terminology throughout the manuscript.
- Section 6.3: Why only 2 climate attributes (aridity and snow fraction) are considered?
- Section 6.3: The clusters G1, G2, G3 are arbitrarily chosen by re-grouping 10 pre-existing clusters. The reasons of this choice are not fully clear to me and this double clustering creates some confusion in this section. Perhaps the authors could use a clustering algorithm or better justify this choice.
- The title of section 6.3 is unclear