

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC3
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Comment on nhess-2022-174

Anonymous Referee #3

Referee comment on "Analyzing the informative value of alternative hazard indicators for monitoring drought hazard for human water supply and river ecosystems at the global scale" by Claudia Herbert and Petra Döll, Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-174-RC3>, 2022

This study proposes a new classification of streamflow drought hazard indicators (SDHI) to guide researchers or practitioners in selecting an appropriate indicator(s) for their objective(s). In order to demonstrate the importance of the proper selection of SDHIs in a drought assessment, the authors compared eleven SDHIs, including three new indicators, and quantified their similarities and discrepancies. Because human society and ecosystems adapt to a local streamflow regime, this study underscores their habituation implicitly assumed in each SDHI. This includes how one can define a normal condition and a condition under a lack of water. In this context, the results show that drought severity, i.e., accumulated drought magnitude, rather than mere magnitude at a time step, may need to be evaluated to investigate prolonged drought events. The dry season in the arid region, which is not suitable for a conventional threshold method, and the water deficit in comparison to water demand are also paid particular attention in this study. Classifying SDHIs and organizing their features, as a result, the authors stress that one needs to select an appropriate SDHI(s) in view of their assumption(s) and operational drought early warning systems need to cover a wide range of DHIs to support a widespread users' demand.

The paper provides several important insights regarding SDHIs. Because a better understanding of the application and interpretation of DHIs is a crucial challenge, the results are valuable for the drought research community and practitioners. However, I have some major concerns that the authors need to address during this discussion phase.

- **Structure of this manuscript**

I assume the authors have a certain intention, but I would say that the current structure of the manuscript is not so reasonable to me. Due to the structure, it was difficult for me to read the manuscript and grasp this study's point and novelty. Therefore, the author needs to reconsider the design of the storyline. The followings are my suggestions:

- I recommend the authors move Section 3.1 to Method (or partly, Introduction) because this section describes (i) debatable points, difficulties, or caveats in selecting/applying SDHIs, (ii) characteristics of each SDHI that serve as premises for subsequent analysis/discussion, and (iii) the definitions of key terms (e.g., magnitude/severity, conceptual/operational, etc.). Moreover, this section includes relatively general contents, some of which were mentioned in the Introduction, and refers to many preceding studies. Probably, the authors aim to present the classification as a new result, but I would say that it should be reasonable to present the classification as a new classification "method" in Chapter2 and demonstrate the needs and validity of the classification in the Result Chapter.
- In conjunction with the previous comment, I would recommend the authors modify the structure of Chapter4 and rename Chapters 3 and 4 as Results and Discussion. This is because the paragraphs in P. 37 and 38 in Chapter4 give a good summary of the results. Although the authors describe the essence of a comparison at the end of each comparison in Chapter 3, these key lines are fragmented in the Chapter. Thus, the summary and interpretation of the results fit the best right after Chapter3 (= at the beginning of Chapter4) with Table 3. I would title the summary section "systematic approach for selecting streamflow drought hazard indicators", and the Recommendation could be section 4.2.
- The position of the description in Model validation is not reasonable (section 3.4) #1; the 1st paragraph of the section has to be in section 2.1 because these are not results of this study and are justifications for the use of WaterGAP.
- The position of the description in Model validation is not reasonable (section 3.4) #2; the validation results (the 2nd paragraph of section 3.4) have to be presented before the model-based analyses (i.e., section 3.3).

- **Model validation**

I require the authors to present a more description on the model validation concerning stream drought reproducibility. Currently, no figure has been presented for section 3.4. Also, I expect an additional analysis to evaluate how well the simulated stream flow data reproduce hydrological drought events detected by the observation data. I would say that, at least, the comparison of observation- and simulation-based drought detections at the selected two gauge stations should be presented similarly to Figure 2.

- **The selected two gauge stations**

The basin names and their characteristics (lines 507-510, lines 516-517) should be described right after the first sentence of section 3.2 in order to explain the phrase “at two GRDC gauge stations with different streamflow regimes”.

- **March 2002**

- I am curious about why March 2002 was selected as an example in this manuscript. An additional short line is expected in this regard.
- The authors need to discuss the generality of the results in Section 3.3 for March 2002, although I assume that similarities and discrepancies among SDHIs are similar for other months and years too.

- **Other**

The writing often lacks sharpness. It includes many restated phrases (for example, 21 “i.e.” in total) and repetition (e.g., the authors repeatedly stress that severity is a cumulated value.). This manuscript can be more concise to deliver the authors’ messages to readers.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2022-174/nhess-2022-174-RC3-supplement.pdf>