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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Identifying a Transition Climate Zone in an Arid River Basin using a Hydrological Drought Index" *by* Libo Zhang et al.

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Responses to review comments

(Note: Revision is attached as supplement)

In the manuscript entitled "Identifying a Transition Climate Zone in an Arid River Basin using a Hydrological Drought Index", Zhang et. al., analyzed meteorological and hydrological drought indices to identify transitional climate zone in a Heihe River Basin (HRB) in the arid northwestern China. The authors used simulations from a Regional Integrated Environmental Model System (RIEMS 2.0). Based on their analyses, the authors found the hydrologic based drought index being more suitable for the characterizing the transitional climate zone in the study basin, compared to meteorological

based drought index. While the study might fall within the scope of this Journal, there are several limitations, in my opinion the work performed here is not adequate for the publication in this Journal. Major issues with this study is detailed below.

Response: Thanks for reviewing our manuscript and providing many constructive and valuable comments and suggestions. We agree with the major concerns and confusions raised by the reviewer. We think that they are caused by our misuse of the drought index terms rather than by using inappropriate indices. In fact, but we actually used correct indicators of aridity indices for this climate classification study. For this reason, we think we are able to address the comments 1-3 and 7 and by using aridity indices to replace drought indices and providing clarification. As suggested, SPI analysis and a brief description of model parameterization are provided. They should address the comments 4-6. Some writings of contexts or the way to present them are changed to address the comments 6-9. Figures are improved to address the comments 10-11.

Please see below for detailed responses.

1. I find the narration as authors put up that drought indices are used for climate classification a bit strange. The aridity index as used by the authors as drought index is also strange as this is generally used as climate characteristics (starting from the Budyko's classical work). Also what I have hard time to understand is that the authors are using the full range of aridity index – which considers both wet and dry phase – in their analyses. So where is the perspective of droughts here? In order to consider droughts the authors must focus of one end of the drought indices (drier parts) and not the entire range. The similar is the case with the author's analyses for the hydrological drought index. So, if I did not get it wrong – the whole perspective of the author's analyses revolving around the drought indices are misleading

Response: It is a very valuable and helpful comment. This study is to classify climate regimes in the Heihe River watershed of the arid northwest-

ern China. As pointed by the reviewer and also indicated in other literatures (https://en.wikipedia.org/wiki/Climate_classificationin), aridity indices are one of the indicators used in climate classification. One of the two indices used in this study, the Budyko-type Aridity Index (AI), is apparently an aridity index. The difference between potential (PET) and actual evaporation (AET) is often used to measure aridity (https://www.springer.com/us/book/9783642291036, P 21-39). Thus, the other index used in this study, the Evaporative Stress Index (ESI), is also an aridity index, though it appears as a ratio of AET to PET instead of a difference between PET and AET. However, we misused terms by calling the two aridity indices as drought indices and therefore caused the confusions. In the revision, the terms are changed from meteorological and hydrological drought indices to meteorological and hydrological aridity indices throughout the paper.

As clarified above, this study is to classify climate regimes rather than analyze droughts. Besides the arid lower Heihe River reach, this region also includes the humid upper reach in the mountain area and a transition zone in between the arid and humid regions. Thus, the full range of aridity indices was used with lower and higher values indicating arid and humid climates, respectively.

2. The authors must provide argument and reasoning for the choice of selected drought indices. Why do not authors choose conventional and commonly used drought index – Standardized precipitation index (SPI) or the Standardized Precipitation- Evapotranspiration Index (SPEI) for meteorological droughts; and runoff based drought index for hydrological droughts.

Response: As indicated above, this study actually used aridity indices rather than drought indices for climate classification. Considering many similarities between aridity and drought indices, we describe and compare with some popular meteorological and hydrological drought indices in the Introduction section. As suggested, SPI is analyzed in the revision (see the response to comment 4).

3. Related to the above one, why are the drought indices analyzed at annual time scale and not monthly or moving average estimates as commonly used in drought studies?

Response: Due to the limitation with PET calculation in the upper basin, where no values are available during much of the winter time when temperature is below 0oC, we did not analyze the aridity indices at monthly time scale. However, we compared averages over the analysis period for each of the spring, summer and fall seasons (Figure 8). Same as the annual values, noticeable differences are found in ESI but not in AI between the middle and upper basins for each of the three seasons.

4. To a certain degree I understand the choice of using the regional climate model in their analyses, but what I miss is the thorough comparison of the RIEMS model to observations – in the sense that it could provide meaning conclusion for drought analysis. The authors must demonstrate that the selected model is able to capture the observed behavior of meteorological and hydrological droughts (say SPI or standardized runoff index). Here I mean skill of the model for drought index and not the variable itself.

Response: As suggested, SPI is analyzed (Figure 5). The results show general agreement between the simulated and observed precipitation variability at basin reach scale. The results are described in the revision (Lines 257-263).

5. To my understanding the RIEMS model provide estimates of net radiation (Rn) through their numerical parameterization of the mass, momentum and energy conservations schemes – so why do not the authors use Rn variable instead of PET – which is just the proxy of available energy? Please elaborate.

Response: PET is used because it is an item in the formulas to calculate the meteorological and hydrological aridity indices AI (Line 151) and ESI (Line 163). It is true the Rn is produced by the RIEMS model. Rn is used in calculating PET with the Penman-Monteith method (Eq.1).

6. As stated by the Abstract and Discussion sections point towards “human distur-

bance" affecting the hydrological drought conditions. Do these statements are supported by their analyses or this is just the speculation? Do the selected model (RIEMS) considers the process affected by human disturbances (irrigation, farming, urbanization, grazing activities) and in which way those disturbances affect the hydrological processes? Please consider elaborating on the RIEMS parameterization and provide some modeling results in this direction.

Response: This study did not provide direct results on the effects of the land-surface processes and human disturbance on hydrological aridity conditions. Thus, the statements are just speculations. For this reason, this statement is removed from the abstract.

The RIEMS model used the Biosphere and Atmosphere Transfer Scheme (BATS) to simulate the land-surface hydrological processes. These processes depend on vegetation and soil properties, which change over time due to the human and natural disturbances. The vegetation and soil properties measured in the HRB in 2000 were used to replace the universal BATS specifications, but the disturbances were not included in the simulation that provided the data for this study. In the revision, we add discussion in section 4.2 about the land-surface parameterization used in the model and the ways in which the disturbances affect hydrological processes (Lines 477-486).

7. All you describe in Section 3.2 is hydro-climatic characteristics and not the spatial pattern of drought indices – as the section heading indicates and explained in text. See the point 1 of my comment.

Response: Following the change described in the response to the comment 1, the term in heading is changed from drought to aridity (Lines 150 and 282).

8. The starting paragraph in the Introduction section does not flow - instead of making a case (motivation) for the current study, it starts with describing the study catchment (Heihe river) and then jumping to other region (Colorado river in US). Please reformulate.

Response: As suggested, the study catchment is presented later in the Introduction section, following the descriptions of aridity indices and climate classification.

9. The results section starts with detailing supplement plots – I would have expected to see first the main plots and then the supporting plots and not other way around.

Response: These figures provide the climate background in the study region as well as simulation validation, which we think is useful to understand the aridity results and validate model performance. The concern with the comment may be partially due to the fact that we put these figures in the supplement. In the revision, we remove the supplement and present all figures in the main context. Some figures are combined to limit the number of figures.

10. Figure 1: Quality is too poor – I could not read any of the subplot's legend.

Response: The first and second subplots of this figure are removed and the remaining subplot is more readable.

11. Figures 2-7: What is the point of making these figures up to 43oN, when the whole study area ends at 42oN? Also please consider improving these figures and limit them to just show the study basin region.

Response: As suggested, the upper portion of these figures is removed.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-310/nhess-2017-310-AC2-supplement.pdf

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