Comment on nhess-2021-73

Anonymous Referee #1

Referee comment on "Drought evolution characteristics of the Qinghai-Tibet Plateau over the last 100 years based on SPEI" by Shengzhen Wang et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-73-RC1, 2021

The paper of Wang et al. addresses drought trends in the Tibetan Plateau using the SPEI index and attempts to provide some indications about the main temporal and spatial trends in the years 1901-2018. Nevertheless, the manuscript is very far from achieving its objectives, because it has many drawbacks leading me to suggest its rejection. The main problems are:

- The paper does not provide any novelty either concerning methodology or results. It mainly looks like a technical report, but rather confused.

- The paper is very poorly organized, with several serious drawbacks in every Section (please refer to specific comments).

- Methodology explanation has many unclear aspects. In particular, the spatial scale of the problem is not declared. If the authors use a spatially distributed database, I don't understand why they talk about spatial interpolation. Furthermore, please note that a global SPEI database, based on CRU TS dataset v4.03, already exists: https://spei.csic.es/database.html

- There's practically no discussion. The authors never try to explain the reason for the results they get. Furthermore, results are formally inaccurate (e.g., the term "significant" is used with excessive ease).

Therefore, I strongly suggest that the authors deeply rethink both research aims and the structure of their paper and try to take more advantage of the only original part of their study, consisting of retrieving drought hazard data, which could be more fruitfully utilized for validating global datasets.

Please find below my specific comments.

- Abstract

It is quite confusing concerning results.

L13: "The drought on the Qinghai-Tibetan Plateau showed a clear gradual increase in aridity from southeast to northwest over the last hundred years" I guess the increase concerns the aridity trend

- Introduction

It is poor in many details. A more thorough discussion is needed concerning the choice of the drought index. Recent studies on climate trend over such an important and wide area must be described in much more detail. Even the choice of the climate dataset must be adequately justified, highlighting the (not few) drawbacks of the selected dataset and other datasets available. E.g., how does the selected dataset work on the vast study area? In what subregions are it more or less reliable? Is it more performing in the most recent years and less in the past? Why is it preferable to others? Still concerning the dataset, please consider that the last version of CRU TS is the 4.05 released on 17 March 2021. Even though I acknowledge that it has been released very recently, some discussion about the differences with the selected version (released in May 2019) is needed.

- Methods (not Method)

Many ready-to-use tools exist to calculate SPEI and to perform Mann-Kendall tests and wavelet analyses. If the authors used some of these tools, as I guess, they should explicitly state it.

- Section 2.1

Explanation of SPEI is not clear, though it is a quite well-known index. The time scale used for SPEI is never declared.

Sometimes present tense is used, other times past tense. This needs to be checked.

Equations: Equations numbers are all (0.0)

L59: lowercase gamma

L61: why interpolation? It's just a difference

L63: PET_i

L67: parameters b and c?

L73: what about W_0 and W_2? Are all the W_i terms calculated using the equation below?

- Section 2.2

The authors abruptly introduce the Mann-Kendall test without explaining why they need it

- Data and processing

3.2 Data preprocessing

Why the bilinear interpolation algorithm was used? Was the CRU spatial resolution modified? I can't find any statement related to this point throughout the text so far. What are the "points to be measured"?

Most important, no detailed information about the study area is provided (and, of course, no maps delimiting the study area).

- Results

Fig. 1 does not explain what the authors state in the text. Furthermore, it is not clear. Titles of vertical axes are not provided. The meaning of "Frequency of droughts (L)" per year is not clear at all.

Fig. 2: again, what do the authors mean with drought frequency? More generally, the reasons provided by authors to justify the use of SPEI compared to historical droughts (not clear how they were recorded) is weak and not objectively presented.

L156: so, I understand that the authors did not need to calculate potential evapotranspiration, but they used it directly from the CRU dataset.

Fig.3: not clear what is the difference between the SPEI line here and in Fig. 1. Furthermore, I guess that the overall linear trend is not significant.

LL157-159: here it looks like the authors start some reasoning about the overall time trend, then they abruptly shift to spatial heterogeneity.

Section 4.3: what do the authors mean by the term "significant" from the statistical point of view? This aspect is of the foremost importance to understand the main message. Not clear how the different time periods were subdivided.

L186: Aljinshan … Qaidam Basin … Ali Plateau… etc: these toponyms are unknown to most of the readers. As a consequence, this paragraph is almost incomprehensible. That's why a map of the study area is needed.

Section 4.4: the periodic analysis and Figs. 6 and 7 are poorly presented and discussed. It is neither recalled later.

- Discussion

Section 5.1: actually, that's not Discussion, but presenting further results concerning precipitation and temperature, which, if really needed, should have been presented before SPEI results (since SPEI depends on precipitation and temperature). As a matter of fact, in the Conclusions, the authors first recall precipitation and temperature results, then SPEI.

Fig. 8 is not well described: what are UB and UF? What are the units and what do the vertical axes mean?

L236: "Zhang and colleagues (Zhang Wangxiong et al., 2019)" must be rewritten Zhang et al. (2009).

As before, when the authors explain spatial changes referring to toponyms, the text is incomprehensible.

Section 5.2: that's not Discussion, it is coming back to Results and presenting them in another (and possibly not consistent) way.

- Conclusions (not Conclusion and Discussion)

LL266-267: that's not true, the main topic of the paper is the SPEI index, not precipitation and temperature that are presented only in Section 5.1.

LL277-281 and LL297-306: two long sentences are repeated twice. This is a sign of insufficient carefulness in the final drafting of the manuscript.