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Hydrology and Earth System Sciences Discussions

Interactive comment on "Are droughts occurrence and severity aggravating? A study on SPI drought class transitions using loglinear models and ANOVA-like inference" *by* E. E. Moreira et al.

Anonymous Referee #1

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1 General comments

This paper deals with the question of the evolution of meteorological drought occurrence and severity in Portugal over the last century. Long precipitation time series from 10 stations are used to compute SPI12 and describe meteorological droughts. The comparison of SPI class transitions between different subperiods is here used to detect stable trends or oscillations over the whole observation period.

This paper addresses a quite relevant question, right in the scope of HESS. Further-

more, the authors use an innovative approach which is quite appealing for quantifying low frequency variations of meteorological droughts. There are however methodological issues (prior definition of subperiods and prior removal of time series with trends) as well as various presentation issues (non relevant literature review and non readable table results) that would prevent it to be published in Hydrology and Earth System Sciences.

I therefore recommend major revisions and hope that the authors will submit a revised manuscript that would address the issues detailed in the following section.

2 Specific comments

The following paragraphs list my major methodological comments:

- P11284, L13-14: the authors rejected time series showing a trend in annual precipitation, so this will considerably reduce the possibility of finding trends in drought class transitions (especially persistence). *This possibly leads to a significant bias in the results of this study.*
- P11285, L14-15: The authors write here that "sub-periods were defined according to this perceived dynamics in order to gain accuracy when comparing them". *This is in my opinion a major flaw in this study.* The prior choice of periods (based on the precise characteristics that are looked for) prevent any robust conclusion to be drawn from the subsequent analysis.
- For all the "Results" section: results are presented in non-readable tables that did not help me in my appreciation of the paper. According to the submission records, this was expected by the authors but they presumably and unfortunately did not took the time to make their results accessible to the reader.

Other specific comments are detailed below:

- P11280, L7 to P11282, L28: This very long literature review is far from being relevant for two main reasons:

  1. the authors picked up studies of different types of droughts: agricultural (Szép et al., 2005 in Hungary; Richter and Semenov, 2005 in the UK; Moonen et al., 2002 in Italy) or hydrological (Raziei et al., 2009a in Iran, Katz et al., 2002, floods (!) in the USA) whereas the present work focuses on SPI drought, i.e., meteorological droughts based on precipitation deficits only. The possible trends on soil moisture/streamflow droughts for example may come from trends in temperature (hence evapotranspiration) and not from trends in precipitation.
  2. the authors also pick up studies of various locations around the globe, trying to find consistent patterns of trends. The evolution of droughts is however largely dependent on the site studied, and I cannot see how studies in Mexico (Seager et al., 2009a), Iran (Raziei et al., 2008) or India (Raje and Mujumdar, 2010) may inform the present work in terms of trend findings. Note however that teleconnections may explain some possible consistency, but this is a research topic on its own. Results from closer studies should therefore be preferred, and the authors presumably did not carefully search the literature for the Iberian peninsula (89 responses to the sole query "trend AND drought AND Spain" in the WoS for example).

  What would be interesting to find in this literature review is: (1) a summary of previous findings in terms of meteorological (but also other types of) drought trends over Portugal or neighbouring areas (Spain, Morocco) and a state-of-the-art of methods to assess such trends (which would show the novelty of the proposed approach).

- The authors do not seem to have a clear view of the literature, as illustrated by (1) various expressions like "it is common in our time the idea (sic)" or "it is often said" and (2) lengthy quotes (up to 12 lines long!) of specific studies in lieu of summary of research findings justified by citations.

- P11282, L24-25: The authors also very quickly mention the future of droughts by quoting 2 climate change impact studies (Droogers et al., 2007; Olesen et al., 2007) on agricultural/hydrological droughts. The driving forces for changes in such droughts are however very different from meteorological droughts studied in the present work. These citations therefore do not seem relevant, and I do not encourage the authors to start discussing about the future of droughts in the present paper as they are interested in observed past changes.

- The authors do not mention the reference period for standardization and computation of the SPI12. If the whole observation period has been used here (as I presume it has been), this introduces a potentially significant bias between the different stations which have different record lengths. The reference period should definitely be clearly defined and the SPI computation possibly adjusted accordingly.

- P11284, L9-10: the authors mention that "the annual precipitation data sets used in SPI computation…". What kind of input time series has been actually used here? Monthly time series or annual time series?

3 Technical corrections

- P11279, L27 to P11280, L6: The choice of the 12-month time scale is not clearly justified in the text. Statements from the authors are generic findings, but local specificities linked to geology (aquifers or not) or size of the catchment may heavily influence the relevance of the 12-month time scale for surface water drought. Please either provide some justifications (e.g., correlations with low-flows) or remove this specific statement.

- There are a lot of grammar and typos all over the text. The authors should make it carefully reviewed by a native English speaker.
- P11280, L5: What are these “former studies”?
- P11281, L5: “(Raje and Mujumdar, 2010)”
- P11283, L22: “Suhaillaa et al. (2011) Âż is not present in the reference list
- P11284, L6: Table 2 is referenced before Table 1 (L11284, L19). Please switch table numbers and orders.
- P11287, L13: I don’t understand the change in index.
- P11289, L10: please define “the F test”
- P11289, L22: please define “the Scheffé multiple comparison method”
- P11290, L24-26: What should be understood here by “global climate change”?
- P11291, L9: The authors mention the “drought prone Alentejo” area, while they have written above (P11278, L21) that “drought is a normal recurrent feature of climate, which occurs in all climatic zones.” (on which I definitely agree). These statements are clearly in contradiction. Could the authors reformulate or specify their views on this?
- P11291, L26 to P11292, L1: This paragraph has already been written (much or less in the same form) above (P11284, L5-7 and P11284, L26 to P11285, L3).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 11277, 2011.