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Hydrology and Earth System Sciences Discussions

Interactive comment on "Observed drought and wetness trends in Europe: an update" *by* I. Bordi et al.

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Reviewer's assertion

It is my opinion that a shift from anonymous to eponymous (signed) reviewing would help the scientific community to be more cooperative, democratic, equitable, ethical, productive and responsible. Therefore, it is my choice (and my aesthetic attitude) to sign my reviews.

Reviewer's report

1. I enjoyed reading this paper and I think it is a useful contribution to the literature. I like the way the authors ridicule (though in a formal and austere manner) linear trends in hydrometeorological variables, whose quest has indeed become very trendy. The paper is nice and well written and the mathematical part, although I did not check carefully, seems sound and convincing. In my opinion, the paper could be published in HESS even as is. However, I have a few concerns and I think there is significant potential for improvement. To this aim, below I list several recommendations for some changes and additional analyses, which I make in a constructive manner.

2. My major concern is about how well the data represent reality. The dataset aims to be monthly precipitation over Europe on a regular grid 1.9°×1.9°, in longitude and latitude, from 1949 to 2009. However, it is not measured precipitation "but derived completely from the [weather prediction] model 6-h forecast". The authors say that "we may feel enough confidence on the data quality". However, we may need some more information to feel that confident. It is my opinion (cf. Koutsoyiannis et al., 2008), that "the climatological community focuses on theories and models, whereas the hydrological community has greater trust in data"; here data means observations. I wish to commend the authors for addressing their paper to the hydrological community (HESS); at the same time, I hope that they can tolerate some incredulity in terms of the data. There is a crucial question that the authors should discuss: Are these data outputs of the same forecast model using consistent input data? Or do they originate from different models, older models for older periods and newer models for more recent periods, and/or with different input data? I hope the answer for the former question is positive; otherwise some consistence tests are necessary and perhaps some adjustments to make the older data consistent with the newer ones, etc.

3. To increase our confidence to the model data, I propose a simple additional analysis.

The authors include trend analyses at four specific grid points in Greece, Scandinavia, northern England and northern Germany. I think it would be very useful and convincing if they repeated the analyses for the same points using observed data. I think it should be easy to find and analyse four natural observed time series. I can help the authors to find a time series close to the grid point in Greece (it seems to be located between the cities of Lamia and Volos). It would be very interesting to see whether or not the model data are consistent with the observations, continually or at specific periods (I hope they are).

4. In my view, the main findings of the paper are that: (a) the drought and wetness indices are not static but change through time, and (b) the changes do not form monotonic trends (as they would in a simplistic climate-change thinking of global warming supporters) but appear as irregular fluctuations in time–and in space. These findings are apparent in all graphs and suggest a perception of climate consistent with the Hurst-Kolmogorov (HK) dynamics. Apparently, the authors are not aware about this as they do not include any reference to Hurst (1951) who discovered this behaviour in geophysics, to Kolmogorov (1940) who (studying turbulence) proposed for first time the mathematical frame for this behaviour, or to recent works that have linked this behaviour to climatic trends (Koutsoyiannis, 2003; Koutsoyiannis et al., 2009), essentially showing that Nature is "naturally trendy" (Cohn and Lins, 2005). The HK framework underlines the high uncertainty of complex hydroclimatic (as well as geophysical, technological, socio-economical) processes and facilitates understanding and mathematical (stochastic) description of processes in a more consistent manner than deterministic descriptions and classical statistical descriptions (Koutsoyiannis, 2006). Moreover, this framework corrects the, usually overstated, significance of statistical tests of trends (Koutsoyiannis, 2003; Cohn and Lins, 2005, Koutsoyiannis and Montanari, 2007; Hamed, 2008; Khaliq et al., 2009) and resolve the paradox of “regional inconsistency” or “spatial non-uniformity” (also influencing the present paper), where neighbouring locations may have significant (according to classical statistics), yet opposite trends (Hamed, 2008).

5. In an HK perspective, what is observed in this data, i.e. the absence monotonic (linear in particular) trends and the long-term fluctuations, seems absolutely regular and expected. The authors use several terms to describe this behaviour such as "nonlinearity", "nonlinear fittings", "multiyear periodicities", and "long-term periodic behaviour". "Nonlinearity" is a term that is typically used to characterize the deterministic dynamics of a system and not to describe a fitting of line to data. "Periodicity" is used to describe a deterministic control that implies a cyclic repetition with a constant period; this is not the case here. I would suggest replacing these terms with "multi-scale fluctuation", which seems more consistent with what we observe ("long-term fluctuation" would be fine too, but I think there is fluctuation also on the short term).

6. Given the relevance of the scope of this paper with the HK dynamics, I think that an expansion to include a testing of the studied time series for HK behaviour, including estimation of Hurst coefficients, would be beneficial for the completeness of the analysis and for a better understanding. In essence, such a study would show how (by which law) the variability changes with time scale, using a full range of time scales, instead of those used now (3 and 24 months).

7. The Conclusions section includes an interesting statement, that "further analyses are needed . . . to understand the physical causes leading to the observed change in precipitation ... About the last question, it should be of interest to investigate if an increase of the baroclinic activity at midlatitudes occurred in recent years leading to a change of the tropopause height". I wonder, if such physical causes are eventually found (e.g. the increase of baroclinic activity), would not a new question arise, What caused these causes? The causes are useful if they can be used for prediction of future events, but I doubt if in this case such causes can provide useful deterministic predictions.

8. I fully agree with the last sentence of the paper, i.e., "These results should be taken into account in drought risk assessment and in planning proactive measures to limit the negative impacts of drought and wetness in Europe”. I would add that, given the

difficulty in predicting deterministically the evolution of droughts (has anyone predicted in the 1990s that the increasing trend of droughts would reverse after 2000?), the only feasible way to take this behaviour into account and plan proactively is to use HK stochastic dynamics for future projections. That is why I insist that my suggestion in point 6 above is essential for the study of droughts in Europe (and not only).

9. Minor comments:

- Mention of "statistical significance" of trends seems not necessary in the context of the paper. It is reminded that the statistical significance is substantially affected by the presence of HK dynamics (see references in point 4) and most papers in the literature that neglect this are mistaken.

- The symbol "R $-$ square" is not a proper symbol–the dash could be taken as a minus sign. I would suggest replacing it with R^2.

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