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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Nonstationarities in the occurrence rates of flood events in Portuguese watersheds" *by* A. T. Silva et al.

**Anonymous Referee #2**

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General Comments

The authors present an exploratory analysis on the variability of flood occurrence rates in ten Portuguese watersheds in order to examine the principle of stationary that nowadays rely on Portuguese hydrologic modelling. At this aim, the authors used a theoretically-sounded statistical basis. The authors have concluded that the mathematical formulation used in Portugal for the flood frequency modelling must be revised. This is an scope of the maximum interest. The improvement in the understanding and subsequent mitigation of such hazardous episodes is an issue of the paramount interest for the hydrometeorological modelling community as well as to better assess the effects of climate change in flood and flash-floods frequencies. The work is theoretically

sounded, the methods use suitable statistical methods and these are well referenced. However, I have some important concerns that I feel that they must be clarified before the acceptance of the present manuscript

1. A major concern is the quality of the stream- and rain-gauge data that the authors have used. The authors must be aware of the problems with dealing with such long series daily data. It is usual to find some important gaps in the daily time series. And I have not found either an explanation of how these data have been checked and revised in order to avoid inconsistencies. I would strongly recommend to the authors to clarify this issue, since all the later statistical analysis is based on these data.

2. I have also missed an introductory paragraph dealing with the problematic of floods and flash-floods at least over the Iberian Peninsula. Such episodes produce every year many personal and material losses. Therefore, some relevant information about these events must be included. Such this found in Doswell 1994 and 1996; Chappell, 1986 as well as some known references studying these events in different locations over the Iberian Peninsula (Llasat et al., 2003, Benito et al. 2003, Amengual et al. 2007, etc.)

3. A second major concern that arises is that the manuscript is clearly untidy. The authors mix paragraphs that in my opinion should go in the introduction, or in the data section or in a methodology section or in the discussion section and put them altogether. In order to facilitate the understanding and readability of the present manuscript, I must advise an in-depth revision of this issue. At this moment, the manuscript does not reach an international standard quality.

Specific Comments

4. The authors must be aware that they have available a limited dataset. That is, ten flow-gauges and only four rain-gauges. So, I would appreciate some comments about how limits your work. I mean, you should avoid to use statements such as: '....an exploratory analysis on the variability of the streamflow regime in mainland Portugal...'. You must be aware of the limited number of datasets that you have for Portugal when



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you are drawing your results and conclusions. Furthermore, these catchments are mainly located in the northern part of the country. So, it is difficult to convince me to generalize your results for the whole country. I must recommend to be aware of these limitations

5. I have missed some description of the physical and climatological characteristics of the watersheds under study. I guess that these must be important. So, I would recommend a short descriptive section addressing this issue. I would also recommend highlighting the physical and climatic differences among northern and southern basins.

6. I have found some parts of the current manuscript too concise. This fact somehow prevents an adequate understanding of the methods used by the authors for the wide scientific community interested in HESS. For example, statements as: '. . ..Furthermore, tests were carried out to determine whether or not the thresholds were adequate: the mean number of over-threshold events in a year and the mean exceedance above threshold, tests no. 1 and 2, respectively, reviewed by Lang et al. (1999).The application of such tests did not invalidate the adopted threshold selection criterion. . ..' are hardly to fully understand if they are not more developed. I would appreciate additional efforts to clarify and avoid confusing statements like this. I believe that the authors must effort in order to make more accessible the comprehension of its work

7. The mathematical notation k/(k+1) can produce some kind of misunderstanding. I would recommend changing it. I also believe that it is more correct to use the term inter-annual variability rather than multi-year variability

8. I would recommend that more than a visual analysis of Fig. 3., the authors calculate the trends. It would be enough to make a simple linear adjustment to properly discuss this issue.

9. Another very confusing statement: '. . ..An additional evaluation of whether there was any significant increase or decrease 25 in the intensity of those processes focused on



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the number of extreme events per hydrologic year, a discrete variable denoted by _k for the hydrologic year k/(k +1)...'

10. First paragraph of section 3.1 should be placed in the introduction section

11. Section 3.2: I would recommend putting all the mathematical expressions not as a part of the text but as independent equations in order to improve the readability.

12. Section 3.4. It is fundamental to separate the database and the methods from the results. Currently, this is one of the major lacks of the present manuscript

13. Section 4: First paragraph must go to the introduction.

14. '... in the 1960s corresponds to a prolonged NAO negative phase in the same years. However, in the 1930s, where there are peaks in ËĘ_(t) in graphs S8 and S9 of Fig. 6, there is no similar negative phase in the winter NAO indices...' . It is not strange. You must remember that precipitation is strongly modulated by regional and local factors.

15. Conclusions: I would strongly recommend to the authors to highlight somewhere the strong limitations that you have when dealing with the datasets. You only have available data from ten stream-gauges and four rain-gauges. So, you have to put your conclusions in the correct framework when making general statements. Climatologically speaking the present database is neither sufficient nor representative to draw generalizations. Furthermore, It is my opinions that it is not correct to include references in the conclusions unless for pointing out future lines of work.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8609, 2011.