

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

Anonymous Referee #1

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Response to the comments of Silva T. et al

General comment

In general I feel satisfied with the responses provided by the author. To my knowledge the two main drawbacks of the paper laid on the disordered structure of the manuscript and on the lack of explanation of data origin, quality, selection criteria etc. The authors will try to re-structure the paper in a new version of the manuscript, and have provided justifications about the data (which is precisely what they need to incorporate in the revised manuscript). Nonetheless I still disagree in some of their comments.

In their overall appreciation, the authors state that “Accordingly, we do not agree with

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the recommendation of rejecting the manuscript on the basis of presumptions on data quality and/or data homogeneity, and the structure of the paper, without a critique or a single comment on the methodology and models or on the results derived from the analysis.” To me, either of the two reasons alone is enough to doubt about the suitability of the paper to be published in a scientific journal. The role of the referee (and that is what I was trying to do) is to indicate the weaknesses of the work to be improved or corrected and thus make the paper more suitable (if deserved) for publication. Of course I’m not going to criticize the good things of the paper (statistics and models), but the authors must understand that without explanations about the data, the results are subject of suspicion. Example: if I don’t know the criteria established for selecting data series, how can I know that you only took the series that suited your preliminary hypothesis and yielded the “wanted” results?

The authors feel to have been misunderstood by the referee, but I don’t see it is a question of misunderstandings, but of lack of information and explanations. I thus suggest that all the explanations given by the author in the response to my revision should be integrated in the manuscript.

In detail

Author comment: “The data we used are acquired by the Portuguese Water Institute, INAG, which is the official entity responsible for the installation, exploitation, maintenance and data storage and processing of most of the surface, underground and coastal water (quantity and quality) monitoring networks. The data are made available via a web portal of the INAG The Portuguese national SNIRH database (Sistema Nacional de Informações sobre Recursos Hídricos, www.snirh.pt), which, prior to their disclosure, performs data consistency analysis on a regular basis and has very high standards as data quality is concerned. Also, it is the main source of hydrological and hydrometeorological data used by researchers and practitioners in the field of water resources engineering and science in Portugal.”

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Reviewer comment: Please include this information in data section

AC: "As for data homogeneity, which rigorously means every element of each of our samples is extracted from the same population with an assumed PDF with the same set of parameters, we must clarify that, in our analysis, we are not claiming such an attribute"

RC: I was actually referring to the existence of in-homogeneities in the data series, such as an abrupt change due to: changes in the monitoring instrumental, re-location of the gauge station, temporal inactivity, change in the calculation formulae... The existence of such would invalidate any statistical procedure; in this case, the number of POT events would be completely biased after/before the in-homogeneity. Because of it different tests have been developed in order to detect and correct them (e.g. SNHT by Alexandersson 1986). I now believe that the series analyzed have been tested against in-homogeneities by the SNIRH, but prior to your explanation, should I assume it?

AC: "In addition, we must clarify that our samples do not have to have the same sizes nor the analysis has to be performed only for a single common period of data in order to validate our conclusions; note that we are not performing a regional analysis or similar procedure where we need a common statistic or a common growth curve."

"The samples should have and they do have some overlapping periods of data to allow the conclusions on high and low values of $\lambda(t)$, through different time windows. Data have been scaled by average values, taken in different time spans, to see how their respective occurrence rates vary with time and not to compare them among different samples or series, as implied by the reviewer's comments."

RC: I am satisfied with this explanation, but this is precisely what the reader of the paper demand. First, it must be clarified the reason for selecting those series and no others; second it shall be stressed or discussed whether the analyses/results are independent of the time period or not, and why.

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AC: "Before made official via the SNIRH, the data are object of quality control. Regarding the samples we used, a few missing daily values were found in the daily flow series and were filled according to procedures that were developed and validate for Portugal (see references below) and in such a way not to have an effect in the paper's results. We remind that in our analysis we are mainly concerned with the times of occurrences of hydrological extremes and not with the values of such extremes."

RC: Please include this explanation and references in data section.

AC: "We do not understand why the reader should doubt the representativeness of the data. Upon finding a nonstationary behavior of $\lambda(t)$ in one watershed, a question is automatically raised: "is such behavior due to site-specific interventions on the territory (e.g.: urban development, crop type, forest fires), or is it due to climate?". Our strategy to answer this question was to apply the methods to several watersheds in different parts of the country and also to rainfall gauging stations located nearby, and assert if those time series exhibited a similar behavior. We believe our results show that the nonstationarities are not site-specific. We previously justified why we are not able of including data from the central region."

RC: The reason for doubting about the representativeness is quite easy to understand: If I don't know the criteria for selecting the series, how do I know that you only analyzed series that fitted your sought results? I think the greatest part of the problem raise from the lack of an explanation about the criteria for selecting the series. If these are the only series available, please stressed it in the paper; if there are more series available, please specify the reasons for not analyzing them. If you selected the series randomly, please clarify it. Please note that I do not intend to doubt of your honesty, but as a reader I need an explanation about the criteria for selecting some series and no others.

AC: We have a long experience regarding the study of the stream flow regime in Portugal. According to our knowledge of the territory the response of the watersheds we selected to extreme events is nearly pristine.

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RC: This information must be included in the manuscript. The authors must have a wide knowledge about the selected watersheds, but no clue is given in the text (or figures) about their supposed non-altered regime. Are there reservoirs upstream the studied gauges? If there aren't, please specify it.

AC: "We do not understand what the reviewer means by 'representing it in the same figure'. Our intention was not to quantify the influence of the NAO and we are not claiming that such influence is homogeneous in space. Given our purpose, we believe that the number of case studies is sufficient to support our conclusions. Furthermore, as we have stated in the paper, the relationship between the NAO and the λ_k is not particularly strong: it is merely indicative of an influence. We believe that such relationship could not be rigorously quantified, at least using our approach, regardless of the number of case studies in the south of Portugal."

RC: The aim of your study is exploring nonstationarity on floods occurrence. Identifying the causes for such nonstationarity is not amongst the objectives of the work, but then NAO is brought up as a cause for nonstationarity? Why NAO and not Escandinavian Pattern, or Eastern Atlantic Pattern, or Eastern Atlantic /Occidental Russian Pattern? Why not for example, non-climatic factors such as the changes in forest cover in the watersheds (which have been demonstrated to exert a control over floods)? What I mean, is that either the paper deals with the NAO or with the causes for nonstationarity, thus a serious analysis (or at least discussion) of the relationship between NAO and floods is performed, or not. If not, the section of the NAO should be removed as it is not the objective of the work, and it does not provide novel results.

To summarize, my overall recommendation is to include all information provided about data origin and quality, putting special effort in explaining the criteria for data selection. I will be pleased to revise the new manuscript

Alexandersson, H. 1986. A Homogeneity test applied to precipitation data *Journal of Climatology* 6:661-675.

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

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