

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

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### **General comments**

We appreciate the reviewer’s comments on the structure of our manuscript and will try to incorporate all, or at least most, of his/her suggestions in a revised version of the paper. As for our ‘insufficient efforts’ to assess the quality and homogeneity of the data series we used, we strongly disagree with the reviewer’s comments. 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,

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the Portuguese national SNIRH database (Sistema Nacional de Informações sobre Recursos Hídricos, [www.snirh.pt](http://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. 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. Please note that we are working with POT events and also that, in the general case, the annual probability CDF  $F$  of a variable  $X$  depends on both the CDF of the exceedances over a high threshold, and on the time-dependent Poisson rate,  $\lambda(t)$ , which can show a multi-year variability, thus creating nonstationarities. Our concern is entirely related to the parameter  $\lambda$  (the relevant data are the times of the events and not the events themselves) which we have found to be variable in time as opposed to a fixed value. Of course the CDF of the exceedances may or may not vary with time too, but this is not the focus of our manuscript nor does it affect our conclusions. In both cases (CDFs of  $X$  or the exceedances), however, we are not assuming invariant parameters or distributions. As the focus of our work, we modeled how  $\lambda$  varies with time and this has been done for each of the samples we used, showing that nonstationarities are present in the POT data as different time windows are considered. 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. Obviously the samples must have some overlapping periods to allow the conclusions on high and low values of  $\lambda(t)$  be drawn, particularly if the  $\lambda$  values are indexed in time, as in fact they are. Therefore, we think we have been misunderstood by the reviewer and hope these notes can help clarifying the misunderstandings. We feel that the reviewer raised more suspicions on the data than comments on the aim of the paper.

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Accordingly, we do not agree with 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.

## Specific comments

### A) Structure of paper

Reviewer's comments: "In my opinion this constitutes one of the main drawbacks of the paper. The structure is somewhat of a "chaos", and rather un-intuitive, leading to confusion to the reader. . . . The paper should be re-structured in a more intuitive way."

Authors' comments: We appreciate the reviewer's efforts to propose a new structure for our manuscript. As we mentioned earlier, we will incorporate all, or at least most, of the reviewer's suggestions in a revised version of the paper in order to make it more intuitive.

### B) Data Quality

RC: "I have serious concerns about the quality of the data used and its suitability for the analyses. The authors use robust statistical approaches to characterize variability and nonstationarity of flood and intense precipitation events, however, if the sample data does not accomplish quality criteria, all the assumptions and results obtained could lead to erroneous interpretations. I next summarize the main concerns about data:

1) Whilst the authors mention the source of the NAO index (climate research unit), no mention in the manuscript is done about the source of the streamflow and precipitation data. What institution/s provided such data? Is it open access data? Please clarify it in the text."

AC: Data used in our work are acquired by the Portuguese Water Institute and made

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available by the SNIRH ([www.snirh.pt](http://www.snirh.pt)), as previously mentioned. This remark will be added to the revised version. Although we think it is important to include the origin of the data, and we will include it in the revised draft, we do not understand why in a scientific paper draft, the omission of the data origin is a matter that should automatically raise serious concerns leading to the rejection of the manuscript.

RC: “2) There is no mention about the quality of data. The reader does not know anything about the homogeneity of the series. The presence of gaps is rather usual in precipitation and streamflow data, especially on daily series, and no information is given about it. Are all the series used free of data gaps? If there were gaps, were they filled/ what method was used to fill data?”

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.

References:

Portela, M.M., Quintela, A.C., 2002. Assessment of the streamflow characteristics under unavailability of discharge data: the mean annual flow depth over the watershed as a regionalization parameter. In: The Portuguese case, *2002 EGS Conference*, European Geophysical Society, Nice, France.

PORTELA, M.M.; QUINTELA, A.C., 2002, “Evaluation of the water resources in Portuguese watersheds without streamflow data”, *Conferencia Internacional de organismos de Cuenca (International Conference of Basin Organizations)*, Madrid, Spain.

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PORTELA, M. M.; QUINTELA, A.C., 2005, “Regionalization of hydrologic information: establishment of flow series at ungauged watersheds”, *Water Resources Management III* (Carvoeiro, Portugal), pp. 11-20, Wessex Institute, WITPress, ISBN 1-84564-007-1, Southampton, Boston.

PORTELA; M.M.; QUINTELA, A.C., 2006, “Estimação em Portugal Continental de escoamentos e de capacidades úteis de albufeiras de regularização na ausência de informação”, *Recursos Hídricos*, Vol 27(2), pp. 7-18, Associação Portuguesa dos Recursos Hídricos (APRH), ISSN 0870-1741, Lisbon (in Portuguese).

PULIDO-CALVO, I.; PORTELA, M.M., 2007, "Application of neural approaches to one-step daily flow forecasting in Portuguese watersheds", *Journal of Hydrology*, 332, 1 15. Elsevier.

RC: “3) The authors mention that the watersheds analyzed are “geographically spread over Mainland Portugal” (page 8612 , line 20). However, what one see in Fig. 1 is that most of the watersheds are located in northern Portugal, only 2 are located in the south of Portugal, and all central Portugal (and what its most important, the watersheds of the Tejo river) remain unstudied.”

AC: Historically, most of Portugal's stream gauging network were installed in the North (for planning and designing hydroelectric generation schemes) and in the South (idem for irrigation schemes) somehow neglecting the Central region which was not as adequate for either of the previous purposes as the other regions. So, it is quite difficult to find long record stream flow data in such region. Besides its huge watershed area (80600 km<sup>2</sup>, 31% located in Portugal, and 69% in Spain), the Tejo (Tagus) is highly regulated (including flood control) and therefore unsuitable for our purpose. Nevertheless we will change the sentence stressed by the reviewer in order to make it more correct.

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RC: “The reader, therefore, must doubt about the representativeness of the selected series when the authors state: “The similarities in the behaviour of  $\lambda(t)$  among different watersheds that are geographically spread over the Portuguese territory, and between rainfall and streamflow suggests that the observed trends are inherent to the natural multiyearvariation of the hydrological cycle, as opposed to potential anthropogenic influence on the catchments themselves”. Is it not possible to include a sample of watersheds from central Portugal?”

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: “4) Inhomogeneous period of study. This is probably the most critical drawback of this paper and the most important in terms of comparability and representativeness of results. Results yielded from the analyses cannot be comparable if the longitude of the series is not homogeneous, thus conclusions drawn are not reliable. Long-term mean of the series are used to calculate POT, if the longitude of series is not the same, how can the resulting POT series be comparable? They are also used to remove dimension and make séries comparable, but these average values correspond to different time periods.”

AC: We assume that ‘inhomogeneous period of study’ refers to different time spans among data samples and that ‘longitude of the series’ refers to their length. In fact, our samples do not have to have the same sizes nor the analysis was performed on the basis of a single common period of data because we are not looking for a common statistic among gauging stations, as commonly employed in regional analysis. We are examining how  $\lambda(t)$  varies with time and the values, of course, are indexed in  $t$ . 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: “Overall, my suggestion is that the authors repeat the analyses with a sample of series selected upon robust criteria, i.e.: common study period, quality of data, and (if possible) homogeneous spatial distribution. ?’is that possible?”

AC: We refer the reviewer to our previous comments. We performed our analysis on the basis of the long series of mean daily streamflows in unregulated Portuguese watersheds. Our study is exploratory and we do not intend to establish a nationwide rigorous study on how  $\lambda$  varies with time, at any given point in mainland Portugal. In our conclusions we recommend further research on this ample topic.

At this point, our conclusion is that we have been misinterpreted in many points of our manuscript and we hope to have clarified them.

## Minor issues

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RC: “Please change /correct the following

- Page 8615, line 8: “. . . Fig. 2b and c. . .” should be “Fig. 2c and d”, is that right?”

AC: It will be corrected in the revised version.

RC: “- Page 8617, lines 1-2: “. . . the year K in the horizontal refers to the hydrologic year . . .” I don’t think it is necessary to repeat this, as it was previously clarified in data section.

AC: The sentence will be deleted from the revised version.

RC: “- Page 8617, line 5: “from the late 1950s to the late 1960s”. Could you change in figures 3 and 4 the time scale of the X axis, with lapses every 10 years? i.e.: 1920, 1930, 1940 and so on.”

AC: The figures will be changed in the revised manuscript and we will include a tick for every ten years, analogously to Fig. 7.

RC: “- Page 8621, lines 9-10: How do the readers know that these rivers are not regulated? Any data/ information to support such assertion are needed. Maybe the Impoundment Ratio index (Batalla et al. 2004)”

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: “- Page 8621, lines 14-16: I suggest including a figure with a couple of examples of the streamflow and precipitation  $\lambda$  series plotted together in the same scale to see if they really exhibit similar trends.”

AC: We will include such figure in the revised version.

RC: “- Page 8623, lines 3-5. “Fig. 8 clearly shows that for every analyzed sample: (i) the majority of years without floods have positive NAO indices, and (ii) the years with the highest flood occurrence do not occur in positive NAO phases”. It is known that the control of NAO on precipitation/streamflows on the Iberian Peninsula is not homogeneous in space, being stronger its influence in the southwestern part, than in the northwestern sector (Martin-Vide 2001, LorenzoLacruz 2011). Thus, these 2 features observed should be more evident in the watersheds located in the south of Portugal. Is it really like that? Is there any way / analysis of representing it in the same figure? (The sample of series in the south of Portugal is, however, too small (just 2) to make any comparison, thus I suggest that you increase the number of watersheds in southern Portugal to be analyzed)”

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  $\lambda_k$  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.

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RC: “- Page 6824, lines 20-23. When one looks at the graphs in Fig. 6, it is noticeable that after the peak in the 60s, there is a decrease in the flood occurrence rates until recent years; in fact the authors state in page 8621 lines 7-8 that “the graphs with data until the late 1990s and 2000s exhibit lower occurrence in the more recent years.” At this stage the reader misses an interpretation of such decrease from the 60s. In the introduction (page 8611, lines 4-6) it is stated that circulation models project and aggravation in extreme precipitation in northern Portugal. Results here seem contradictory with such assertion, thus an interpretation and explanation of results should be given.”

AC: The sentence in the introduction, taken from Santos et. al., 2002, is merely illustrative of the issue of nonstationarity and is not irrevocably bound to the conclusions of our paper. Also it refers to scenarios of greenhouse gas emissions for the future. We will make the sentence clearer: “although general circulation models (GCMs) based on some scenarios of greenhouse gas emissions show an expected trend for an aggravation of extreme precipitation events in northern Portugal”. We thank the reviewer for bringing this to our attention. However we do not agree that it is contradictory. Furthermore our analysis concerns only the occurrence rates of the events in the past, and not the magnitudes or durations of the events themselves.

Reference:

Santos, F., Forbes, K., and Moita, R.: Climate change in Portugal: scenarios, impacts and adaptation measures: SIAM Project, Gradiva, 2002.

RC: “- Fig. 6. Could you re-organize the figure in such way that floods and precipitation graphs are more clearly distinguished? (in separated columns, for example)”

AC: We will reorganize the figure as the reviewer suggests.

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RC: “- Page 8627, line 3, (Lorenzo-Lacruz et al.): Please correct the surname of the coauthor “González-Hidalgo””

AC: We will make the correction in the revised manuscript.

RC: “- Page 8627, line 9, (Morán-Tejeda et al.): Surnames of co-authors should be written instead of their names. Please change them. ”

AC: We will make the correction in the revised manuscript.

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

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