

Interactive comment on “Long-term meteorological and hydrological dryness and wetness conditions in the Zhujiang River Basin, South China” by T. Fischer et al.

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In the following sections, we listed the comments by reviewer #1 and #2 and appended to each comment our specific response!

Anonymous Referee #1

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The paper provides an analysis of hydrological drought and wetness in a sample river basin located in south China using observation data from 118 stations. In assessing

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drought and wet conditions in the area of interest, the authors applied the Standardized Precipitation Index (SPI) and the Standardized Discharge Index (SDI) on 24-month time scale. Principal Component Analysis (PCA) is then applied to the SPI-24 to identify sub-regions within the area having different drought/wetness variability, while spectral analysis is used to unveil the statistically significant periodicities in the time series of indices. The study, following the path of several recent papers on drought variability analysis and drought regionalization, is very interesting for its application to a region of interest from a climatic point of view. Thus, the paper deserves publication but a careful revision of the text should be done, mainly devoted to clarify some conceptual and methodological aspects. Some suggestions are:

1) Along the text and also in the title, the authors refer both to meteorological and hydrological drought and wetness. However, the applications of indices involve only the long time scale of 24 months, typically used for monitoring hydrological aspects. Short time scales of a few months are instead necessary to investigate the meteorological conditions. Thus, I suggest explaining this aspect properly in the text clearly stating that only hydrological conditions are addressed. This implies also a change of the paper title that should concisely denote the content of the paper. Also, attention should be paid in using the word “flood”, which denotes a meteorological phenomenon that is very different from “wetness”, and is not addressed in the paper;

Response 1) We agree on this comment that our study only investigates long-term hydrological aspects and not meteorological conditions. We have not been precise in the terminology as we considered the precipitation data (SPI-24) as meteorological conditions. However, as the reviewer pointed out, scientifically it does not make much sense when looking at long-term trends in precipitation data in this respect and so we have modified the text accordingly. We also agree that the use of the term ‘flood’, which is neither a long-term nor a meteorological event, is confusing and have revised the text accordingly. As suggested by the reviewer, we also changed the title as follows: “Hydrological long-term dry and wet periods in the Xijiang River Basin, South China”

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2) The abstract does not summarize clearly the study. The authors should refer to the time period considered and data used, as well as why the PCA is applied. The sentences “The principal component analysis reveals many spatial interdependencies in dryness and wetness conditions for the sub-basins and explains some spatio-temporal disparities. Moderate dryness conditions have a larger spatial impact than moderate wetness conditions in the sub-basins.” are not clear. It is difficult for a reader, who is not familiar with PCA and SPI, to follow the text. Moreover, the spatial impact of moderate drought cannot be deduced from PCA, since the loadings identify regions of common climate variability that is represented by the associated PC scores; but PC scores does not provide information on the SPI classes;

Response 2) We revised the abstract in order to be more precise and included the time period and data used. We have also briefly explained the use of the PCA and modified the entire section on the PCA. We also agree on the concerns of the reviewer on the impact of moderate drought. Therefore, we have modified the description of the results, findings and conclusion concerning the PCA and have aligned the sections of the manuscript (see also Responses 5 and 10).

3) The introduction should point out the aim of the paper and explain how the work is organized in order to highlight the relevance of the analysis also in relation to the previous studies on a similar topic (Bordi et al. 2003, 2004a, 2004b); (see at the end)

Response 3) We have revised the introduction and have directed it more to the aims and organization. We now include, mainly at the end of the introduction, a more detailed description of the aim of our study and relate it to previous studies (Bordi et al., 2003, 2004a/b; Subash et al., 2011) as suggested. We also refined the explanation of the approach and organization of the study by describing each chapter/approach and its purpose.

4) Section 2.3.1: Authors state that they use precipitation from 118 stations in the Xijiang River Basin. However, from Figure 1 it seems that most of that stations fall

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outside the basin under study. Also, the title refers to the whole basin (Zhujiang Basin). This aspect should be clarified. The methodology used for the computation of the SPI at the six sub-basins in Figure 1 is not clearly described. The right way is to compute the precipitation time series averaged over the stations falling in each sub-basin to use as input for the SPI. If so, the PCA is applied only to the six SPI time series? At page 10531, lines 13–14, it is not correct to state that the SPI-24 is suitable for monitoring meteorological drought (see point 1) before). Also “long-term” is not appropriate here. Lastly, more details should be given about SDI; which function is used for fitting the distribution of discharge data accumulated on 24-month time scale? Is it the twoparameter Gamma as in the SPI computation?

Response 4) The 118 stations are located within the Xijiang River Basin, but a total of 192 stations in the entire Zhujiang River Basin. We changed Figure 1 and eliminated the stations outside the Xijiang Basin in order to be consistent with the term ‘Xijiang’. We have also revised the entire manuscript and title, and followed the suggestion to use Xijiang instead of Zhujiang. We have also revised the entire part on the calculation of the Thiessen polygons and the SPI. The calculation of the SPI was done for the sub-basins, based on the average precipitation in each sub-basin and not on single stations. Hence, we applied the PCA only to the SPI of the six sub-basins. We hope that with the revision of the chapter it is clearer now for the reader. We also changed the terminology (long-term) according to the reviewer’s comments, and improved the description of the calculation of the SDI.

5) Section 2.3.2: “We use the PCA to sum up the spatial patterns of co-variability of dryness and wetness according to the SPI-24 series at different stations”, what does it mean? And the PCA is applied to all 118 stations? Then, please clearly state that the loading patterns, properly normalized, represent the correlation between the SPI time series at the stations and the associated PC scores time series. Is it so in your Figure 8?

Response 5) The sentence was not very well written indeed. We revised section 2.3.2.

C5400

in order to explain the application of the PCA in much more detail. We applied the PCA to the SPI of the six sub-basins and not to the 118 stations. This means that the normalized loading patterns represent the correlation between the time series of the six values of the SPI (one for each sub-basin) and the associated PC scores (this is displayed in Figure 8). This approach places higher focus on the sub-basins (which are of higher concern for water managers in the region as well as for the hydrological aspects of the study) instead of using 118 meteorological stations and regionalizing them. Although the variability was displayed already, we wanted to provide numerical estimations for the co-variability of the sub-basins by applying the PCA to the sub-basins. Therefore, we describe the spatial homogeneity or heterogeneity in the appearance of dryness and wetness conditions of the six sub-basins with the PCA's loading patterns. The results are used to underline the regional disparities and inter-relations of dry and wet periods at the sub-basins scale (see also Response 10).

6) Section 2.3.4: The description of the extrapolation of the SPI time series is confused, no optimization is applied;

Response 6) We revised the description of the extrapolation in the section and added more details. We agree that we have made a mistake on the missing optimization and have changed the text accordingly by referring to the use of few significant periods (but not an optimization of the data itself).

7) Section 3.1: A comparison between annual precipitation in the whole Xijiang basin and the discharge at a single station might appear not correct to a reader; a plausible explanation should be provided. The end of the section is not clear;

Response 7) We agree that the sentence was misleading. We revised the text and provide an explanation why we compare the time series. We also changed some parts at the end of the section in order to emphasize what we want to accomplish with this comparison – showing the existing strong correlation of both time series, and explaining the need of further research in periodicities of dry and wet periods.

C5401

8) Section 3.2: Does Figure 5 show the averaged SPI-24 over the whole basin? If yes, in this way you lose the SPI classes; the SPI for the whole basin should be computed using the averaged precipitation over the basin;

Response 8) We have first calculated the averaged precipitation over the basin (based on the area weighted average of all 118 stations), and later calculated the SPI-24 for the basin's average. Due to the use of the average precipitation, the SPI classes as defined by Lloyd-Hughes and Saunders (2002) cannot be used for the SPI-24 of the entire basin, as the values are much lower in magnitude. We are aware that we lose the SPI classes with the averaging, but have now expressed this problem in much more detail in the methodology chapter and in section 3.2 to remove any obscurity.

9) Section 3.3: Why the author use “moderate drought” for events characterized by $SPI < -1$? They are just dry events, all SPI dry classes are considered. The same holds for wet events. See also Table 1;

Response 9) We agree that we have mixed terminologies (see response 1). Of course, the terms drought and flood should be replaced by dry and wet events. Also, the ambiguous use of the SPI classes was not clearly described (as mentioned in response 8). We have carefully revised the entire manuscript, and are now always referring to dry/wet conditions, dry/wet periods, or dry/wet events. We have revised Table 1 accordingly.

10) Section 3.4: The last sentence at page 10537 is unclear or wrong. Moreover, why the loading values at Figure 8a are so low? They vary between 0.36 and 0.44 denoting a low correlation between the SPI time series at the sub-basins and the associated PC-1 score in Figure 7. Is the loading properly normalized? Please check. Moreover, why do you not apply the PCA and Varimax rotation to the whole stations in order to identify possible sub-regions having different dryness/wetness variability? Or if the regionalization is not an objective of the study, why applying PCA? You have already considered different sub-basins and you know the time variability there (Figure 6);

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Response 10) We understand the concerns of the reviewer, and are grateful for his comments on the results of the PCA and the application of the Varimax rotation. We have changed the misleading sentence. We assume that 67% of the variance in the SPI-24 is resulting from a homogeneous climate regime over the Xijiang River Basin. The similar correlation of the loadings of PC-1 has been calculated correctly. The correlation coefficient of 0.36 to 0.44 is not very strong, but not so low either when taking the monthly values of 47 years into consideration. We did not apply the PCA and Varimax rotation to all 118 stations, as we were more interested in the different sub-basins from the hydrological point of view. Even if we already analyzed and displayed the variability, we wanted to provide numerical estimations of the co-variability of the sub-basins by applying the PCA to sub-basins (see also Response 5).

11) Section 4: The authors should highlight the relevance of their results. The periodicities unveiled in the paper are close to the ones found in other remote regions; this aspect should be pointed out;

Response 11) This is an interesting point. We have now changed the text and related it to several research articles and hypotheses by Bordi et al. (2003,2004a/b,2007), Boryczka and Stopa-Boryczka (2006), and Subash et al. (2011).

12) Figure 9 and 10: Please include in the figure captions the meaning of the horizontal dashed line. In Table 1 the mean magnitude of dry and wet events instead of magnitude should be more informative.

Response 12) All suggestions by the reviewer have been applied. The horizontal line in Figures 9 and 10 displays the 90% confidence interval for significant periods in the power spectrum.

General comment: I think that the paper is not well written and this diminishes the work done. I encourage the authors to provide a careful revision of the text.

Response GC) We carefully revised the text at numerous paragraphs and a native

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English speaker has proofread it afterwards.

Newly added references:

Bordi I., K. Fraedrich, J. Jiang, A. Sutera, 2003: Dry and wet periods in Eastern China watersheds: patterns and predictability. *Journal of Lake Sciences*, 15, 56–67.

Bordi I., K. Fraedrich, J.-M. Jiang, A. Sutera, 2004a: Spatio-temporal variability of dry and wet periods in eastern China. *Theor. Appl. Climatol.*, 79, 81–91.

Bordi I., K. Fraedrich, F.-W. Gerstengarbe, P. C. Werner, A. Sutera, 2004b: Potential predictability of dry and wet periods: Sicily and Elbe-Basin (Germany). *Theor. Appl. Climatol.*, 77, 125–138.

Boryczka and Stopa-Boryczka, 2006: Cyclic Temperature and Precipitation Fluctuations in Poland in the 19th-21st Centuries. *Miscellanea Geographica*, 12, 43-53.

Subash, N., A.K. Sikka, and H.S. Ram Mohan, 2011: An investigation into observational characteristics of rainfall and temperature in Central Northeast India - A historical perspective 1889–2008. *TheorApplClimatol*, 103 (3-4), 305-319.

Anonymous Referee #2

The authors present a paper on the application of the Standardized Precipitation Index (SPI) and the Standardized Discharge Index (SDI) to identify dry and wet periods in the Xijiang River basin, a sub-basin of the Zhujiang (Pearl) River. A principal component analysis was applied to the SPI-24 series. Moreover, the SPI-24 and SDI-24 series were analyzed by means of Fourier and wavelet transform. Significant periodicities detected in the SPI and SDI series were then extrapolated to the year 2030. Overall, the manuscript contains a lot of valuable information. However, it needs to be revised based on the comments listed below. Due to the applied nature of the research undertaken, it seems appropriate to name the method and to refer to other publications offering an in-depth description of the method. Nevertheless, more detail is needed in some places:

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1) - Chapter 2.3.1: For SPI and SDI: The authors should explain the fitting procedure.

Response 1) We have included a brief explanation of the fitting procedure. We also recommend the reader to check several excellent studies on this issue for further details (e.g. Bordi et al., 2004a).

2) - Chapter 2.3.2: Description of procedure and outcome do not match. The authors describe that they used “the PCA to sum up the spatial patterns of co-variability of dryness and wetness according to the SPI-24 series at different stations”. Accordingly, I expect the loading patterns of the principal components (Fig. 8) to show spatial variations in each of the sub-basins.

Response 2) We agree that the description of the PCA method and the results have been misleading. We have changed both sections according to the comments made by reviewer 2 and reviewer 1. As we did not apply the PCA to the stations, but to the sub-basins, no spatial variation within each sub-basin can be displayed (only between the six sub-basins within the entire basin).

3) - Chapter 2.3.3: The authors need to explain how significance testing was carried out in the case of the Fourier and the wavelet transforms. Was white noise assumed? If so, why?

Response 3) The significance was tested by using the 90% confidence level according to Becker et al. (2007). We used the Chi square distribution at 4 degrees of freedom, but no white noise was assumed. We have revised the section and included the description of significance.

4) - Chapter 2.3.4: The authors need to explain why they expect periodicities to be stable in time.

Response 4) We use the extrapolation method to project possible dry and wet periods for the future. To do so, we have to “assume” that the periodicities are stable in time, or have to integrate assumptions how they will change. As previous studies (e.g. Gemmer

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et al., 2011; Fischer et al., 2012) have only found few significant trends in observed precipitation pattern on monthly basis, we expect that there will be no major changes in the frequency of dry and wet periods (i.e. periodicities are stable) in the near future (until 2030). For long-term predictions, we would rather have to assume changes in the behavior of the climate system.

5) The title of the manuscript requires revision. According to the time period under observation, “meteorological” is not the correct term to use. “Climatological” should be used instead. Also the phrase “dryness and wetness conditions” (like “meteorological” used throughout the manuscript) should be avoided. I would make use of the expressions “dry periods” and “wet periods”.

Response 5) We agree and revised the entire manuscript by deleting the term “meteorological” and changing the expression of “dryness and wetness conditions” to “dry and wet periods”, also in the title.

6) The language needs to be improved throughout the manuscript.

Response 6) We have made multiple changes in the text, revised the text to our best knowledge, and asked a native English speaker to proofread.

7) page 10531, row 9-11: I recommend that the authors provide a table. Also, instead of “drought” the term “dry” (=extremely dry, severely dry, moderately dry) is the better choice.

Response 7) Yes, we agree and have changed the terminology accordingly.

8) Page 10535, row 16: $1 < \text{SPI}_{-1.5}$ for moderately wet, Page 10535, row 17: $-1.5 < \text{SPI}_{-1}$ for moderately dry

Response 8) As we put our focus on all values of the SPI-24 above 1 (wet events) and below -1 (dry events), we have changed these parts by excluding the word moderately and rephrasing the sentences.

C5406

9) Fig. 9 and 10: x-axis: less minor tick marks would improve legibility; dashed line needs to be explained

Response 9) We improved the caption of Figure 9 and 10 by mentioning that the dashed line corresponds to the 90% confidence interval of significant periods

10) Page 10535 (10538), row 17: “[: : :]” and that the cycles in the SPI-24 dominate cycles in the SDI-24.” The authors need to explain how they come to this conclusion based on Fig.9.

Response 10) We agree that this has not been addressed in the text and rather believe that it should be moved. Hence, this conclusion has been deleted at this section and moved to the manuscript’s conclusions. It has been revised and now focuses on the high correlation of the SPI-24 and the SDI-24.

11) Fig. 11: legend needed

Response 11) We improved the caption of Figures 11 and 12. Now they include: low (gray) to high (black) significant amplitudes of the frequencies.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10525, 2012.

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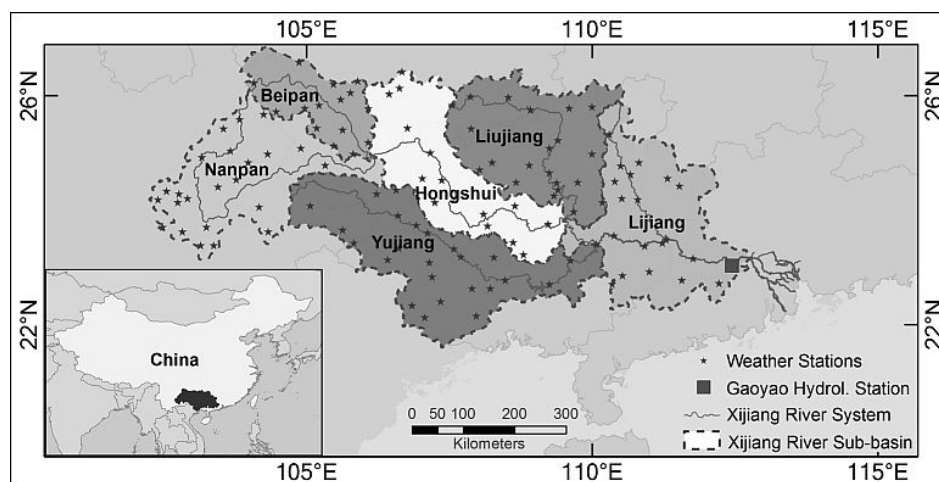


Fig. 1. Fig. 1: Location of the river system, the six sub-basins, and the meteorological and hydrological stations in the Xijiang River Basin in South China

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