Hydrol. Earth Syst. Sci. Discuss., 6, C1749–C1766, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C1749/2009/

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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Received and published: 8 August 2009

Reply to the Reviews on "Observed drought and wetness trends in Europe: an update" by I. Bordi, K. Fraedrich and A. Sutera

We appreciated very much the positive opinion of the Editor, Prof. Alberto Montanari, about our paper. We have taken into account his suggestions in the revised text of the manuscript, providing a discussion on the long-term persistence properties of the time series analysed. We thank the Reviewers for their careful reading of the paper and useful suggestions that helped improve the manuscript. Below we quote each comment, provide our brief response to it, and indicate the changes made in the text in italic.

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Interactive Discussion

Discussion Paper

General comments to the Reviews. There are common adagios in the whole three Reviewers' comments, namely: 1) How do our conclusions hold for "real data"? 2) Why do not use Hurst-Kolmogorov statistics? 3) Why use SSA rather than other methods? We thought that it might be useful to state our position on these three items before going into the details reviewer by reviewer. Point 1). This is perhaps the most dramatic comment. We have applied trend analysis to rain-gauge data in the past in a few world regions: Sicily (36 stations), Elbe basin (369 stations), Emilia-Romagna (a few tens), Western Iran (140 stations) and China (160 stations) and to the NCEP reanalysis grid points close to these regions. The work is distributed over a few papers listed below (Bordi et al., 2004a, b, Raziei et al., 2009). Our general conclusion was that the SPI time series using station data or reanalysis were similar at least at these very long time scales, which are proper for "trend" calculation. Here comes the shortcoming. We do not have access to the updated station data sets for the latest ten years, for which we have proved the dramatic change in "trend". Let us notice, however, that one aim of our paper was, as pointed out by Prof. Koutsoyiannis and using his own words, "to ridicule" this common kind of trend analysis. So, we feel that searching around for station data was not precisely fitting the aim of our paper. Nevertheless, if someone is willing to collaborate, we may join forces and do a comparison. Point 2). Long-range persistence properties of physical processes are well known to us, and our colleagues Cassandro and Jona-Lasinio (1978), studying the scaling properties at phase transition, made the connection with probability theory. One of us (K. Fraedrich), has computed the mentioned statistics for geophysical parameters in several papers (Fraedrich and Larnder, 1993, Fraedrich and Blender, 2003, Blender and Fraedrich, 2006 and references therein). We did not use this approach because: (i) remember the target of the paper spelled out by Prof. Koutsoyiannis, (ii) climate change debate rests mainly on the nature of the "post hoc, ergo propter hoc", i.e. the causal relationship between CO_2 increase and temperature increase which, here, means just determining a "trend". Our paper casts doubts on the nature of trend of a particular variable extensively used for monitoring drought conditions. As already mentioned, the instability of "trend" makes forecast and, by the same token, the evaluation of the tendency, meaningless statements. Now from practical point of view, our paper shows that proactive measures based upon these trend-changes may be as risky as “risk analysis” can be. In our view, adaptation may be a viable alternative with the consequence that it may be useless to prevent future climate changes (assuming we might) since they are known to happen and it is certain that they will happen, though for some unknown reasons. Point 3) SSA has been recently used by Rahmstorf et al. (2007) to show that climate change is accelerating. This paper, published on Science, made the news worldwide. We used the same approach not to make world news but, on the contrary, to advise colleagues how one may use a sound technique and reach different conclusion, especially when someone does not taper the data at their very end (as the aforementioned authors did not). SSA, however, based on solid mathematics such as the theory of dynamical systems, has its own merit in detecting “trend” since, at variance with other instances, the computed trend explains more variance (see PCA decomposition) than other techniques. Of course, the better performance is embedded in the “embedding dimension” (a big boy’s word for filtering). By the way, we would like to know how Prof. Koutsoyiannis would fit the use of this kind of words salad in his view of peer review system.

Referee #1. General comments. We have appreciated very much that Referee #1 choose to sign his review. We feel that such a decision, due to the topical and questionable matter addressed in the paper, also in relation to the recent scientific debate on climate change, assumes an important meaning for the scientific community. Thus, we totally agree with Prof. Koutsoyiannis and appreciate his “intellectual honesty”.

Specific comments. 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

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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.

Reply: We are grateful for the positive comments on the scientific relevance of the topic addressed in the paper. Prof. Koutsoyiannis fully captured the essence of our paper, say our attempt to question the usefulness of trend analysis in hydrologic time series to put forward any conclusive statement. Due to the nature of the time series available, in fact, the interpretation of the results may change drastically when an update is taken into account. Of course, this casts doubts on some conclusions drawn about recent climate changes and, thereby, on some of the causal relationships inferred (for example, the ones detailed in Rahmstorf et al., 2007).

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 1.9o×1.9o, 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.

Reply: The question raised by Prof. Koutsoyiannis is allowed and concerns the main

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limitations we usually have both with reanalysed data and observations. Although we are Physicists, we generally agree with Hydrologists on the greater trust in observed data and usually recommend checking the reliability of reanalysis outputs against observations (see the conclusions at page 3904, line 21). However, for the kind of analysis proposed in the paper, i.e. large-scale analysis of drought and wetness, long time series that are updated and uniformly cover the area of interest are necessary. As known, the coarse spatial coverage, the lack of updated observations and the existence of gaps in the time series lead to many low quality and in-homogeneity problems. Due to these shortcomings an alternative and plausible way seems to be the usage of the reanalysed data, which are based on an assimilation scheme. The NCEP/NCAR reanalysis is the only one that is continually updated and, for this reason, it is widely used by the scientific community for large-scale studies. The model used to provide the reanalysis outputs is the same along the years considered, while the main limitation concerns the assimilated data. After 1979, in fact, satellite data were introduced into the assimilation scheme to implement the observations coming from the conventional instruments. Thus, usually, some criticism has been raised for the years before 1980s. In addition, also in 1948–1957, there was a change in the assimilation system, when the upper-air network was established. However, in view of the recent change unveiled in our analysis for Europe, also for the latest decade a careful check against reliable observations is recommended, even limited to case areas (see also our general comments). In the revised manuscript we improved the description of the NCEP/NCAR data set.

Section 2: “forecasting spectral model with 28 “sigma” vertical levels and a triangular truncation of 62 waves, equivalent to about 210-km horizontal resolution. The model is based on the assimilation of a set of observations, such as land surface, ship, rawinsonde, aircraft and satellite data (Kalnay et al., 1996). These data were quality controlled and assimilated with a data assimilation system kept unchanged over the reanalysis period.” “However, it is worth to notice that although the reanalysis system remained essentially unchanged during the more than 60 years processed (apart minor corrections provided after some errors' detection), there were two major changes in the observing system. The first took place during 1948–1957, when the upper-air network was established, and the second in 1979 when the global operational use of satellite soundings was introduced (Kistler et al., 2001)."

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).

Reply: We totally agree with Prof. Koutsoyiannis, the comparison with observations for the four grid points/locations considered should be very interesting. Unfortunately, such observed data are not available to us and according to our experience (at least in Italy and Germany) it is hard to have free access to long and consistently updated time series of rain-gauge data. However, if rain-gauge data are available for the area close to the grid point in Greece, we are interested to collaborate and complement the analysis. Nonetheless, we like to mention that in some areas where we had both rain-gauge data and reanalysis, a check has been done (see Bordi et al., 2004a, b for Sicily, Elbe basin, and China, Raziei et al., 2009 for western Iran) and the overall results suggested a satisfactory agreement. In the revised text we have just mentioned (section 4) the usefulness of such a comparison.

Section 4: "However, to increase the confidence on the reanalysis data here used and corroborate the obtained results, a careful comparison with observations should be done, even for case areas. This kind of additional analysis is not provided here for the lack of updated observations, but it is highly recommended and will be the topic of

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future investigations. For this purpose, it is important to stress the need for high quality data collection, quality control and updating of databases, including a broad spatial coverage."

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).

Reply: We fully agree with Prof. Koutsoyiannis on the interpretation of our findings within the context of HK dynamics. We have not considered the HK approach for the reasons listed in the general comments.

5. In an HK perspective, what is observed in this data, i.e. the absence monotonic (lin-

ear 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).

Reply: In the paper we used the term “nonlinear” just to contrast with “linear”, and the word “periodicity” because in previous papers (Bordi and Sutera, 2001, Bordi et al., 2004a) we found, using both reanalysis data and observations for case regions, typical periodicities characterizing the SPI time series on a 24-month time scale. Moreover, using few periodic components that mostly contribute to the power spectrum variance of the SPI signals, we evaluated the potential predictability of drought. Finally, we refer to “long-term fluctuation” to distinguish from short-term fluctuations with time scales less than 2 years that are filtered out when the SPI-24 is considered. However, we agree with the Reviewer that the use of these terms might be misleading. Thus, we revised the text replacing, whenever it is appropriate, such terms with “multi-scale fluctuations”.

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).

Reply: We agree in principle; however, it is worth to mention that even in the well-established phenomenon of phase transition, the critical exponent (a big boy’s word

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for the power law decay of the correlation function) must be determined at the second decimal digit. Would geophysical data be so precise to allow such a precision?

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.

Reply: We feel that for a scientist it is always useful to understand the physical causes leading to the observed change because this contributes to shed light on the relationship between atmospheric dynamics and hydrological cycle. We recognise that often the knowledge of the causes does not provide useful deterministic predictions, but surely helps us to better understand what is this “changing climate”, if any, and its impact on water resources. As R. Feynman would say, to make a new theory is a matter of guessing, then compute the consequences, then check with known observations, finally predict something not observed yet. Although in a cryptic fashion, we meant that recent observed changes in the stratospheric dynamics might lead to fluctuations of baroclinic developments in the troposphere. As you may see we are just at the stage of an “educated guessing”.

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).

Reply: As stated in the general comments and at point 6., we agree that the nature of the SPI time series analysed is consistent with the HK dynamics. However, our doubts mainly concern the capability in computing H coefficients with the needed accuracy for such a short time record.

Section Conclusions and discussion:"Another approach that might be useful for planning proactive plans is the use of Hurst-Kolmogorov (HK) stochastic dynamics (Hurst, 1975, Montanari et al., 1997, Koutsoyiannis, 2003, 2006, Cohn and Lins, 2005, Fraedrich and Blender, 2003, Blender and Fraedrich, 2006, and references therein). Results here presented, in fact, are consistent with HK dynamics since the SPI time series appear to be not stationary and characterized by multi-scale fluctuations. Furthermore, the estimation of the Hurst coefficients of the SPI series for sample grid points (here not shown) provide values greater than 0.5 suggesting long-range persistence in the time series. However, such estimations appear to be unstable with respect to the method used for their computation and longer time records are requested to compute them with the needed accuracy."

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.

Reply: We agree; however, in the text we use "statistical significance" referring to the linear trend according to the classical statistical theory. Since we prove that the underlying dynamics is nonstationary and nonlinear, the classical statistical approach fails.

- The symbol "R _ square" is not a proper symbol–the dash could be taken as a minus sign. I would suggest replacing it with R2.

Reply: We agree and revised the text according to the suggestion of the Reviewer.

Referee #2. General comments. The authors would like to thank Referee #2 for his positive and helpful comments that contributed to improve the presentation of the paper. About drought prediction, the main question raised by the Reviewer, we wish to point out that the aim of the paper was not to provide a new or alternative method for forecasting drought and wetness events. The objective of the study was just to capture the attention of a reader on the limitations of trend analysis, nowadays widely applied for climate change detection. It appears, in fact, that an update may drastically change the interpretation of the results that up to some years ago seemed to be clearly and univocally stated. In our opinion, however, the outcome of the nonlinear trend analysis may be useful to capture the overall tendency of the SPI time series and, to a less degree, may provide an added value to drought forecasting. We like in fact to distinguish between "tendency" and "forecasting". They are different concepts although both are useful for planning proactive measures against the negative effects of drought phenomenon. For example, let us consider the grid point over Greece (Fig. 5a). During the latest decade the data suggest that there is a tendency towards near normal conditions, while the SPI index shows the occurrences of alternating near normal and moderate/severe droughts. Thus, the nonlinear component of the SPI series appears a more appropriate measure than the linear fitting in capturing the overall tendency of the signal. On the other hand, forecasting drought remains a difficult task for the random character of the SPI signals that are characterized by multi-scale fluctuations. In previous works (Bordi et al., 2004a, Bordi and Sutera, 2007) we have investigated the potential predictability of drought events, assessed through the SPI, using Auto Regressive models, the Gamma Highest Probability (GAHP) method, or the summation of periodic components that greatly contribute to the power spectrum variance of the SPI signals. The latest two methods provided interesting results; however, some limitations must be mentioned. In the case of GAHP method there is the assumption that precipitation for the future month is the most probable value described by the probability density function of precipitation for that month, while in the latest method needs several periodic components to properly reconstruct the SPI signal and extrapolate it forward. The concluding section of the paper has been improved accordingly.

Section 5, Conclusions and discussion: "…also in relation to a changing climate. The nonlinear trend analysis proposed here is able to capture the overall tendency of the SPI time series, and only partially it may provide an added value to drought forecasting. We like, in fact, to distinguish between "tendency" and "forecasting", which are different concepts both useful for planning proactive measures against the negative effects of droughts. For example, let us consider the grid point over Greece (Fig. 5a). During the latest decade the data suggest that there is a tendency towards near normal conditions, while the SPI index shows the occurrences of alternating near normal and moderate/severe droughts. Thus, the nonlinear component of the SPI series appears as a more appropriate measure than the linear fitting in capturing the overall tendency of the signal, though it is unable to properly forecast dry events. On the other hand, forecasting drought remains a difficult task for the random character of the SPI signals that are characterized by multi-scale fluctuations. In previous works (Bordi et al., 2004b, Bordi and Sutera, 2007) we have investigated the potential predictability of drought events, assessed through the SPI, using Auto Regressive models, the Gamma Highest Probability (GAHP) method or the summation of periodic components that greatly contribute to the power spectrum variance of the SPI signals. The latest two methods provided interesting results; however, some limitations must be mentioned. In the case of GAHP method there is the assumption that precipitation for the future month is the most probable value described by the probability density function of precipitation for that month, while in the latest method needs several periodic components to properly reconstruct the SPI signal and extrapolate it forward."

Specific comments: Lines 4-5 page 3892: the authors use the wording "meteorological and hydrological aspects", which is not well defined. I would relate "meteorological" with "seasonal" and "hydrological" with "bi-annual" more clearly.

Reply: We changed the abstract and stated more clearly the meaning of meteorological and hydrological aspects in the text.

Abstract: “In characterizing the meteorological and hydrological aspects, the index is computed on a seasonal and on a bi-annual time scale.”. Introduction: “…Bordi and Sutera, 2007). Meteorological drought is usually an expression of precipitation departure from normal conditions over a period of time of a few months, while hydrological drought refers to deficiencies in surface and subsurface water supplies due to precipitation reduction over an extended period of time of one year or more. Thus, the different time scales used for the computation of the SPI reflect the impact of drought on the available water resources; typically 3-month time scale is used to characterize meteorological conditions while 12 or 24-month time scales are used to monitor hydrological drought. Moreover, since the index is standardized wet conditions can be monitored as well.”

Pages 3896-3897: the Singular Spectral Analysis is summarised in a synthetic way using equations. Would it be possible to add some lines to state (in simple words) which are the distinctive features of non-linear trend analyses, what distinguish this method (SSA) from other techniques and why it has been chosen? Reply: We have revised the text following the suggestion of the Reviewer (see also general comments).

Section 2: “This method better captures the intrinsic nonlinear behaviour of non-periodic and non-stationary signals with respect to other methods based on preselected basis functions, such as the Fourier transform or polynomial fitting. Moreover, since it is based on the principal component decomposition, the computed trend explains more variance than other techniques.”

Technical corrections. Table 1: the confidence bounds are listed in the wrong columns.

Reply: Probably this mistake occurred when the .doc file has been saved into .pdf. We have corrected the table in the revised version of the paper.

Referee #3. General comments. We are grateful to Referee #3 for providing his detailed comments that helped us to better describe some key concepts.

Details 1- page 3892, lines 25-26: the terms drought and dry spell are used in the same sentence without giving the respective concepts. They are different phenomena and it is important to provide their concepts, simply, to avoid misleading interpretations by readers. This is also important because droughts and dry spells may be differently affected by climate change, which is discussed on the next page.

Reply: We agree, we have included in the introduction the main concepts of drought and dry spells.

Introduction: "Drought is a natural and recurrent feature of climate that originates from a deficiency of precipitation over an extended period of time, usually a season or more. It should be considered relative to some long-term average conditions often perceived as "normal". Thus, a dry spell, i.e. a period characterized by abnormally dry weather lasting from several days to a few months, must be expected as part of the natural sequence of events. However, if a drier than usual period continues for many months or years it is unlikely this is part of the normal continuum of events, and we refer to it as a drought event (Pereira et al., 2009)."

2- Page 3894 lines 24-25. Following the advice of a open reviewer, it seems appropriate to elaborate on the methodology for data reanalysis despite an appropriate reference is given

Reply: We improved the description of the model used in the NCEP/NCAR reanalysis.

Section 2: "forecasting spectral model with 28 "sigma" vertical levels and a triangular truncation of 62 waves, equivalent to about 210-km horizontal resolution. The model is based on the assimilation of a set of observations, such as land surface, ship, rawinsonde, aircraft and satellite data (Kalnay et al., 1996). These data were quality controlled and assimilated with a data assimilation system kept unchanged over the reanalysis period." "However, it is worth to notice that although the reanalysis system remained essentially unchanged during the more than 60 years processed (apart minor corrections provided after some errors' detection), there were two major changes in the observing system. The first took place during 1948–1957, when the upper-air network was being established, and the second in 1979 when the global operational use of satellite soundings was introduced (Kistler et al., 2001)."

Section Conclusions and discussion: "…especially the confidence on reanalysis data against observations in the recent years and the possible impact of the assimilation system on trend detection… "

3- Page 3897: why to introduce the term "western Eurasia"?

Reply: We have deleted the word "western" in the revised text.

4- Page 3898 lines 5-6: Quote some of the referred "previous papers"

Reply: In the revised text we quoted the previous papers. Section 3.1: "…also, previous papers (Bordi and Sutera, 2001, 2004) are based …".

5- Sections 3.1 and 3.2 have titles referring to meteorological and hydrological drought and wetness and refer to time scales of respectively 3 and 24 months. However it is hard to believe that a 24 month time scale of SPI refer to hydrological droughts or wetnesses. It may very well refer to a meteorological drought (wetness). The SPI-3 month may very well do not correspond to a drought but to dry spells. Therefore, I suggest the authors either to discuss the concepts of meteorological and hydrological drought and wetness before using these in a section title, or to change these titles to reflect the time scale of the analyzed SPI.

Reply: We changed the title sections according to the Reviewer's suggestion. Let us mention, however, that our nomenclature apes the one used by U.S. drought monitoring center.

Section 3.1, title: "Spatial extent and trend of meteorological dry and wet spells" Section 3.2, title: "Spatial extent and trend of hydrological drought and wetness"

6- Figure 3 could be discussed with more detail and the figure caption could have a bit more indication for readers about the meanings of results presented, i.e. where the trend is for dry or wet periods

Reply: We agree, in the revised text we have improved the caption of Figure 3. Fig. 3, caption: “Negative values of p1 denote a tendency towards drier periods, while positive ones towards wetter periods.”

7- Fig 4a is referred in the caption only as first loading; thus any reader must go in the text to understand what is presented. I suggest that the figure caption is more explanatory since a figure must be understandable by itself

Reply: We improved the caption of Figure 4 as given below.

Figure 4, caption: “(a) first loading, i.e. the first spatial pattern properly normalized that represents the correlations between the corresponding PC score and the SPI time series, and . . .”

8- The paragraph starting in last line of page 3900 ends 5th line of 3902. Why not to ease reading breaking it into 2 or 3 paragraphs?

Reply: We agree, we changed the revised paragraph accordingly.

9- It could be good to add some references to papers referring to the verification of trends or no trends relative to droughts or wet periods in any region of Europe.

Reply: In the introduction we have already referred to some papers on trend analysis in Europe. We have added more following the suggestion of the Reviewer.

Section 4: “In the international literature there are several papers addressing the trend detection in drought episodes both at regional and large-scale level in Europe, using different data sources and methodologies (see for example Hisdal et al., 2001, Lloyd-Hughes and Saunders, 2002, Moreira et al., 2006, Briffa et al., 2009, Trnka et al., 2009), but no comprehensive study has been carried out with updated data. In agreement with

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our findings, all these efforts demonstrate the spatial variability of the detected trends and the crucial influence of the time records selected for the analysis."

Acknowledgements: "We like to thank Prof. D. Koutsoyiannis, Prof. A. Montanari and two anonymous Reviewers for their fruitful comments and suggestions that contributed to improve the presentation of the paper."

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Please also note the Supplement to this comment.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 3891, 2009.

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