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Climate of the Past Discussions

Interactive comment on "Inferences on weather extremes and weather-related disasters: a review of statistical methods" *by* H. Visser and A. C. Petersen

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We thank anonymous reviewer #2 for a thorough reading of our manuscript. Unfortunately, the reviewer's comments are mainly negative. The reviewer states in the beginning that he or she does not see how the manuscript could be improved. In this reply we will respond to the reviewer's comments #0 through #4. The comments #5 up to 11 are minor, and could be easily implemented in a new version of the manuscript. To our opinion a number of comments of the reviewer are correct (especially the comments #1 and #2), while we disagree with other comments (comments #0, # 3 and #4). We will clarify this below, following the order of the comments of the reviewer.

Reply to comment #0 The reviewer states that the manuscript does not fit in the scope of Climate of the Past. We do not agree. First, we had contacted the Editor in Chief Dr. Wolff with the same question, before writing our review, who replied that he expected it to fall within the journal scope. Second, if we type the catchwords "weather extremes" in the 'find option' on the CP website, we find 32 articles in CP. If we type "climate extremes", we find 58 hits in CP, and if we type "disasters" we find 4 hits. Clearly, the topic of weather and climate extremes has been dealt with many times in CP. The addition of weather- and climate-related disasters is rather new. However, since disasters have huge human impacts, we find it a valuable addition to our review, and certainly of interest to CP. We also refer here to the IPCC-SREX special report on weather extremes and weather-related disasters, the summary of which will appear at the end of November 2011 (the full report is due in December 2011).

Reply to comment #1 We agree with the reviewer that Section 2 needs a 'facelift'. But we do not agree with all the points made in the comment. First we point out where we do not agree: (i) that we would not have described how return periods are calculated and (ii) that the focus on stationarity assumptions in articles would not have been clear. Then we will give an outline how we could improve Section 2 along the lines suggested by the reviewer. We illustrate the calculation of return periods (with uncertainties) in Figures 1 and 6. Many articles which aim at calculating return periods do not explain it clearly to the reader. That is just what we wanted to do in Section 2.1. Thus, we feel it is just opposite to the remark of the reviewer: we illustrate return periods while many other do not. On the remark of ordering the literature as for the measure of stationarity authors assume in their study: we have just followed the ordering of articles as has been done in Coles (2001). This book on statistical modeling of extreme values is an important reference in almost any article in the recent climate literature on extremes. The ordering by Coles is by ordering methods as for their stationarity assumptions (his Chapters 3, 4 and 5). Then, he discusses non-stationary sequences in his Chapter 6. We follow the same categorization of articles, where we add one important issue, not mentioned in Coles: choosing stationary sequences for short periods of time, and then comparing characteristics between PDF estimates over distinct sub-periods. Our categorization in Sections 2.2, 2.3 and 2.4 clarifies this. Still, we agree with the reviewer that Section 2 may not have been a clear enough review for many readers. We propose to reshuffle the text as follows:

Section 2.1 Extreme indicators. Explains the type of extreme indicators one can choose (block extremes with r-largest values included, threshold extremes and disaster extremes). Section 2.2 Methods for making inferences on extremes. Explains trend methods, return periods (also r-year return periods) and the way PDFs can be compared over different periods in time. Section 2.3 Stationary processes. We highlight four approaches: (i) non statistical, (ii) GEV distribution, (iii) POT with GPD distribution, and (iv) normal and log-normal distributions. Section 2.4 Block-stationary processes. We highlight the same four approaches: (i) non statistical, (ii) GEV distribution, (iii) POT with GPD distributions, and (iv) normal and log-normal distributions. Section 2.5 Non-stationary processes. We highlight the same four approaches: (i) non statistical, (ii) GEV distribution, (iii) POT with GPD distributions, and (iv) normal and log-normal distributions. Section 2.6 Software. Here we shortly describe the software available for estimating extremes. The software for estimating STMs (as used in this manuscript) is freely available from the first author (H. Visser).

Furthermore, we agree with the reviewer that some important articles are missing in the present manuscript. Indeed, we overlooked the important reference to Coelho et al. (2008). We scanned the literature again and we propose to add the following 10 articles:

On peak-over-threshold and the generalized Pareto distribution: 1) Renard, B., Lang, M. and Bois, P.: Statistical analysis of extreme events in a non-stationary context via a Bayesian framework: case study with peak-over-threshold data. Stoch. Environ. Res. Risk Assess. 21, 97-112, 2006. 2) Coelho, C.A.S., Ferro, C.A.T., Stephenson, D.B. and Steinskog, D.J.: Methods for exploring spatial and temporal variability of extreme events in climate data. J. of Climate 21, 2072-2092, 2008. 3) Sugahara, S., Porfirio da

Rocha, R. and Silveira, R.: Non-stationary frequency analysis of extreme daily rainfall in Sao Paulo, Brazil. Int. J. of Clim. 29, 1339-1349, 2009. 4) Acero, F.J., Garcia, J.A. and M. Cruz Gallego: Peaks-over-threshold study of trends in extreme rainfall over the Iberian Peninsula. J. of Climate 24, 1089-1105, 2011.

On non-stationary GEV distributions: 5) Hanel, M., Buishand, A. and Ferro, C.A.T.: A nonstationary index flood model for precipitation extremes in transient regional climate model simulations. J. of Geophys. Res. 114, D15107, 2009.

6) Hanel, M. and Buishand, A.: Analysis of precipitation extremes in an ensemble of transient regional climate model simulations for the Rhine basin. Clim. Dyn. 36, 1135-1153, 2011.

On inferences for block-stationary data: 7) Ferro, C.A.T., Hannachi, A. and Stephenson, D.B.: Simple nonparametric techniques for exploring changing probability distributions of weather. J. of Climate 18, 4344-4354, 2005.

On software for extremes: 8) Stephenson, A. and Gilleland, E.: Software for the analysis of extreme events: the current state and future directions. Extremes 8, 87-109, 2006. 9) Gilleland, E. and Katz, R.W.: New software to analyze how extremes change over time. Eos 92(2), 13-14, 2011.

On spatio-temporal techniques: 10) Vanem, E.: Long-term time-dependent stochastic modelling of extreme waves. Stoch. Environ. Res. Risk Assess. 25, 185-209, 2011.

Reply to comment #2 In a revision, we propose to make a clear distinction in Section 3 between PDFs estimated on block maxima or threshold crossings on the one hand, and PDFs estimated on daily data to generate the simulation examples (this Section and Appendix A), on the other hand. Indeed the present text leads to confusion for the reader. This correction can be simply done, especially after the 'facelift' of Section 2. Furthermore, we will clearly define the term "trend" in the new Section 2.2 Thus, irritation will be avoided.

Reply to comment #3 The reviewer does not see the value of Section 5 ("Uncertainty information"). He or she states that much of the work cited is based on our own articles. E.g., "the uncertainty class 2 is apparently only populated by one article from the authors themselves." Furthermore, the need for differential statistics is unclear to the reviewer. Additionally, the reviewer states that differential statistics is only used by the authors themselves. We do not agree with the reviewer. First, we find the use of uncertainty information of utmost importance (Section 5.1). That also holds for Section 5.2: 'Best modeling practices and uncertainty'. We will not give a lengthy reply here for the rationale of using statistics in general or for the topic at hand: weather extremes and weather-related disasters. As we state in our manuscript page 20, lines 13-17, the topic of uncertainty is very important for the IPCC. The IPCC gives thorough guidelines for the 2013 reports. See http://www.ipcc-wg2.gov/meetings/CGCs/Uncertainties-GN_IPCCbrochure_lo.pdf. Another text illustrating the importance of statistics in climate research is given in the Preface and Introduction of the book "Statistical analysis in Climate Research", by H. von Storch and F.W. Zwiers (1999). Also best modeling practices are of great importance. We have illustrated that by the trend example shown in our Figure 7. It happens too often that authors take a (trend) model without any argumentation and show their results. No discussion is given why this method is chosen and we do not know if other methods would have given other results! In our reply to reviewer #1 we give another example of trend estimation, where the authors do not give any argumentation. Another trend model shows a completely different pattern over time. Second, the reviewer states that Section 5 relies too much on research of our own. That is really not true. We define three levels of uncertainty information (pages 20/21 of our manuscript). It appears that many articles fall in class # 2, and not our own research alone. On page 21, lines 20-24, we give 7 high-level literature references for authors giving 'uncertainty class 2' information (with a reference to footnote 1, given on page 13 of our manuscript). Besides this argument, we note that our trend method and, more general, Structural Time Series Models (STMs), lend itself ideally for generating 'Class 2 information'. E.g., STMs are the only models which allow to

calculate differential uncertainty information on flexible trends (the OLS straight line is a special case of the IRW trend model we present in this manuscript). Indeed, there is not much mathematical choice at this point. This is also the very reason we have given IRW examples in Figures 1, 4 and 6. There simply are no other trend models which can give this level of uncertainty information. Perhaps our categorization of uncertainty information might be confronting for some authors. E.g., the reviewer misses the reference to Coelho et al. (2008) (his comment #1). Coelho et al. use a local polynomial fit with a sliding window of 20 years (their page 2077) for finding the time-varying threshold. This trend choice does not give any uncertainty information. No argumentation for this choice is given either. Furthermore, they give return periods in their Figure 9. No uncertainty information is given. Therefore, their article would fall in our 'Category 0'. Why is differential uncertainty important? The strength of using a statistical model is that we can compare estimates over time on their significance (differences are compared to the noise level inherent to the data at hand). The value of having differential uncertainty information is that we can say that a trend estimate in the final year 2010 and the trend estimate in any year in the past is statistical significant or not. And if we have a return period for TXXt data crossing 35 °C of one in 30 years in 2010, and of one in 60 years in the year 1990, that difference looks impressive. However, the difference is not necessarily statistical significant. It depends on the level of noise in the TXXt data. The reviewer comments on the references to Jones and Moberg and to Young. We will improve the text at this point. A final remark. We do not understand the reviewer's last sentence: "To classify the available literature in this way is quite rich, in my opinion."

Reply to comment #4 The reviewer finds it a platitude to repeat that single extreme events cannot be linked to climate change. Furthermore, the reviewer states that we have wrongly cited Pall et al. We disagree on both points. First, it may be a platitude for the reviewer, but there are numerous examples of such a coupling in the grey literature, websites and newspapers. In addition to our example given in Figure 8: if we type in Google "Pakistan flooding 2010 climate change", Google replies with nearly 21 million hits! And if we add the word "Facebook" to these catchwords, we still have 4 million hits. This illustrates the wide-spread coupling of individual extremes/disasters to climate change. An interesting discussion between top specialists on this topic (both in the field of climate extremes and disaster statistics) is given at http://e360.yale.edu/feature/forum_is_extreme_weather_linked_to_global_warming/2411/. It is interesting to note that different opinions are given here. Thus, the term 'platitude' is not completely correct in our vision. A last remark is that Nature does not find the topic a platitude either. See the Editorial "Heavy weather" in Nature 477, September 2011, pages 131-132, and the article of Q. Schiermeier "Extreme measures", published in the same issue. Perhaps, Section 6 is a matter of taste: reviewer #1 wants more text on this topic, while reviewer #2 calls it a platitude. We find it the responsibility of researchers to think about how their results on weather extremes and disasters will land in the press and websites. Therefore, we would plea to keep Section 6 into an update of our manuscript. Second, the remark on Pall et al. is not interpreted correctly to our opinion. The clue is our wording on page 24, line 22-24: '... many are suggestive about the connection while they focus actually on the changed chances.' In the next line we give the example of Pall et al. published in Nature 2011. An example of that suggestive aspect is best given by repeating their title: "Anthropogenic greenhouse gas contribution to flood risk in England and Wales in autumn 2000." This title suggests a directly coupling between human-induced greenhouse gasses on the one hand, and a specific flood event (disaster) on the other hand. The article contains more of such suggestions. The reviewer is right that Pall et all. analyze changes in chances. But still we feel that this article is too suggestive, especially seen through the eyes of journalists.

Conclusion We feel that the manuscript fills an important gap in literature on analyses of climate extremes and disasters: an overview/review of methods researchers use in their analyses, with special attention to statistical oriented approaches. Such a review is even more important since the impact of weather extremes may well increase in the near future. The upcoming IPCC SREX report is an example of a growing attention

for disasters. The reviewer states that this Manuscript does not fit in the philosophy of CP. We have given arguments why the manuscript does fit in the philosophy of CP. The reviewer has raised a number of good points and we can re-write the manuscript along these points. At some points we disagree with the reviewer (the importance of our Sections 5 and 6). We have given our arguments for that.

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