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Climate of the Past Discussions

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Interactive Comment

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

Anonymous Referee #2

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The manuscripts aims at providing an overview about the existing statistical literature on extreme events in a climate context, with a personal view of the authors about pros and cons of various methods. the manuscript closes with some recommendations for the analysis of extreme events.

In my opinion this is is a weak manuscript. It is not well organized it does not provide a helpful overview for the reader interested in analyzing extreme events; the language is too often imprecise and sometimes misleading. the closing section is a collection of general personal views on statistical modelling, not really related to the topic of extreme events; important papers on extreme events in climate research are not included.

Unfortunately I do not see how the manuscript could be improved, and therefore I am afraid that I cannot recommend publication of a revised version either.

0) My comment #0 would be that I do not see why the manuscript fits in the scope of Climate of the Past.

1) The manuscript is disorganized and will not be helpful as a literature review. It starts providing some general overview about extreme events, but it fails to clearly describe the basic statistical approaches to the analysis of extreme events: extrema over predefined blocks with application of Generalized Extreme Value Distributions or Peak-over-Threshold method and application of Generalized Pareto Distribution . Actually, quite surprisingly the word 'Pareto' does not appear in the manuscript at all. The manuscript then focuses on the aspect of the stationary of the statistical processes. This is important, but why focus on this specifically ? The manuscript at this point has not even mentioned how 'return values' are estimated, how parameters of GEV are estimated from the data, how to estimate uncertainties in those parameters, etc. These aspects would be much more important that 'stationary' for any reader interested in an overview of extreme value analysis.

Some papers on the problem of extreme value analysis on non-stationary processes are glossed over, e.g. Coelho et al. 'Methods for Exploring Spatial and Temporal Variability of Extreme Events in Climate Data', J of Climate 21, 2072.

2) More particular questions are dealt with in way that will be confusing for many readers. When describing probability density functions (PDFs) it is very often unclear whether those PDFs refer to the underlying statistical processes, e.g. the daily mean temperature, or to the block-extremes from these data, e.g. the annual daily temperature. The term 'trend' is also used in a quite unclear way. Sometimes it refers to a long-term linear trend, sometimes to a low-frequency behavior of a time series, sometimes to the conditional mean of the response variable in a regression analysis (in the manuscript mu_t) . Many readers will be irritated to read that a linear trend depends on

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time, whereas what the authors mean is the value of the straight line at different times

3) I could not see the value of section 5. The classification of the papers according to the information they provided is not useful and also tailored to the previous work of the authors. One class, class number 2, apparently is only populated by one paper,. written by the authors themselves. This class should contain papers that indicate uncertainties in the 'differential statistics'. Well, I fail to see the value of this information on differential statistics, and apparently I am not alone since no other paper is cited with type of information than that by the authors themselves. To classify the available literature in this way is quite rich, in my opinion.

Also in this section the authors confuse the sensitivity to the choice of statistical model (structural uncertainty citing Jones and Moberg) with dependence on the method to estimate parameters of the same model, citing Young.

4) I also failed to see the value of section 6. It does not provide an overview about how other authors do or do not connect the occurrence of extreme events and climate change, and it just limited to repeating the platitude that a single extreme event cannot be directly linked to climate change. However, the more interesting papers on this type of connection are ignored or cited wrongly. , For instance Pall et al. analyse the difference of two large ensemble of simulations, with and without anthropogenic forcing, and estimate the changes in probability of occurrence of a certain type of event, not of a particular event

More particular comments:

5) 'to extremes please refer to Hegerl and Zwiers (2007), Zwiers et al. (2011) and Min et al. (2011). A critical view has been given by Stephens et al. (2010). For a review'

Stephens does not focus on extreme events nor provides a critical review of attribution of extreme events. It is a paper that evaluates precipitation in climate models

6) 'There is a wide range of studies which assume the data at hand to be stationary.

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Interactive Discussion

Discussion Paper

Stationarity means in practice that the data are stable over time (no trends, breaks, shocks,#

This style in this paragraph is an example of a loose type of language that is often used in the manuscript: Data cannot be stable over time'. data are numbers. the authors mean that the underlying processes that generates the data does not change in time.

7) '– In dendroclimatology tree-ring data are often detrended ïňĄrst and residuals are'

The 'detrending problem' in dendroclimatology has nothing to do with the statistical trend estimation the authors are talking about. The authors are discussing non-stationariy in extreme values. The tree-ring detrending means to subtracts the effect of tree-age on tree growth, as younger trees tend to grow more vigorously than older trees. It is extremely confusing for any reader to mingle both types of detrending ÂÍ

8) 'This long list of trend approaches holds for trends in climate data in general. However, the number of trend models applied to extreme weather indicators, appears to be much more limited. The trend model almost exclusively applied, is the OLS straight line. This model has the advantage of being simple and generating uncertainty information for any trend diïňĂerence [μt $-$ μs] (indices t and s are arbitrary time points within the sample period)1 . Uncertainty estimates are available since the slope of the trend is estimated along with its uncertainty. Examples of OLS trend ïňĄtting are given by Brown'

Uncertainty estimates in OLS are simply available only in the cases where the OLS assumptions are fulfilled, which is mostly never the case: residuals tend to be serial correlated and non-normally. OLS is specially sensitive to those deviations of the assumptions.

9) 'Klein Tank et al. apply the Student's t-test, while Alexander et al. apply Kendall's tau- based slope estimator along with a correction for serial correlation according to a study of Wang and Swail. Karl et al. (2008, Appendix A) choose linear trend estimation

in'.

Wang and Swali is not in the reference list

10) 'We have seen in Sect. 2 that methods fall apart with respect to their assumption of stationarity (Sects. 2.3, 2.4 and 2.5). At ïňĄrst glance one may judge this choice as a matter of taste. As long as one makes his or her assumptions clear, all seems okay at this point. Of course, there is no problem as long as the data at hand are truly stationary, such as in the study of Wehner (2010) who estimates GEV distributions to'

Again, casual, imprecise language: Stationary only applies to the processes, not the data; even adopting the language of the authors; real data are never truly stationary, even model data arent either.

11) 'timization and appears to be a straight line, mathematically equal to the OLS linear trend. The innovations (= one-step-ahead prediction errors) show perfect normal behavior and we conclude that a straight line, along with normally distributed residuals,'

This is quite sloppy language in statistics. No data can show perfect normal behaviour. The authors mean that they do not show obvious non-normal behaviour.

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