

## ***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 #1 for the thorough reading of our manuscript. The reviewer is very positive and raises three comments and a great number of technical corrections (typos and ‘clunky drafting’). We will reply here to the three comments. We agree with the technical corrections proposed and will correct the manuscript accordingly when given the chance to submit a revision.

Reply to comment #1 (pages 2895 and 2909) The reviewer is partly right: the article of Wigley originates from the year 1988 and has been reprinted in 2009 by Climatic Change. However, the article of Cooley in the same 2009 issue of Climatic Change is

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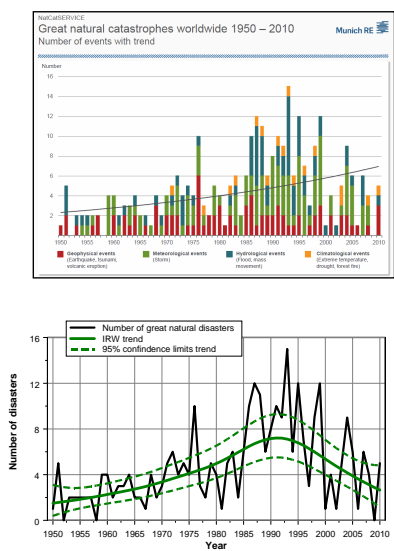
new, and thus only two years old. Perhaps we should not call it “the discussion between Wigley and Cooley” since it is only a one way discussion: Cooley reacts on the article of Wigley, not vice versa.

Reply to comment #2 (Section 5) We have another strong example of trend estimation in disaster data which we can add to Section 5. Again, the trend in the literature deviates from the trend we estimate by applying an IRW trend model. Figure 1 (upper panel) in this reply shows the number of great natural disasters published by Munich Re: ‘Topics Geo. Natural catastrophes 2009. Analyses, assessments, positions’, page 37, 2010. Clearly, the trend patterns differ in both panels. Where Munich Re reports a rising exponential, the IRW trend model shows an increase from 1950 up to 1992 and an increase afterwards. Note that the number of great natural disasters in 2009 was zero. This example shows again that it is wise to present more than one approach to the same data. Note that we do not state that one trend estimate is superior over the other. What we want to show is that different modeling approaches may lead to different results. It is important to discuss such differences. And if no differences occur in trend patterns, that result strengthens the conclusions drawn for the trends found. Thus, in both cases there is a gain.

Reply to comment #3 (Section 6) The reviewer finds that Section 6 contains some important conclusions and he or she asks for an extended discussion. We propose to extend the text with the discussion between climate and disaster experts given at [http://e360.yale.edu/feature/forum\\_is\\_extreme\\_weather\\_linked\\_to\\_global\\_warming/2411/](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. Furthermore, we propose to add a recent article and Commentary published in Nature. See the Editorial “Heavy weather” in Nature 477, September 2011, pages 131-132 and “Extreme measures”, published in the same 2011 issue. Finally, we can give some more explanation for what we mean in lines 18-19 on page 24: ‘In fact, statistical inferences are about chances for groups of events and not about individual events.’

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**Fig. 1.** Fig. 1. The upper panel shows the annual number of great natural disasters. Source of data are: website of Munich Re. The lower panel shows an IRW trend fit on logarithms of the same annual data.

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