Nat. Hazards Earth Syst. Sci. Discuss., 2, C634–C639, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C634/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "On the clustering of winter storm loss events over Germany" *by* M. K. Karremann et al.

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Received and published: 13 May 2014

1 General comments

I find this an interesting paper, providing useful new results regarding the poorly understood phenomenon of windstorm clustering: this is of particular relevance for the (re)insurance industry. Based on a comparative analysis of different datasets, the authors show convincing evidence that the probability of occurrence of clusters of storm-induced losses is not adequately modelled by a Poisson distribution (this distribution is widely used in the industry for modelling occurrences of catastrophic events).

In my opinion the paper should be published, after clarifying a few points mainly re-

garding the methodology.

2 Specific comments

1. page 1917, lines 23-25:

   > "Following the normalisation with the 98th percentile,"

   what is being normalised to what?

   > "the values were interpolated using distance weighted interpolation to the 0.25° grid of the population density"

   Which values were interpolated? The exceedances above the 98th percentile, or the normalised exceedances (perhaps divided by the 98th percentile)? For a given population gridpoint, how many (neighbouring) values were interpolated?

   This delicate point needs to be clarified extremely well: I suspect that the interpolation of intense windspeeds is a tricky procedure, given the very strong spatio-temporal variability which characterises windspeeds and particularly wind gusts. Ideally, the Authors should include some discussion referring to further scientific literature on the topic.

2. page 1919, lines 10-15: "The assignment of gridded wind data ...". It is not clear, here, what is being assigned to what. The caption of figure 1 seem to suggest that wind gusts from the DWD dataset or 10m wind speeds for the other datasets are attributed to each cell of the population density grid. This point needs to be clarified in the main text, perhaps even adding an example for a specific gridpoint, as the results might heavily (and, possibly, very heavily) depend on this choice, as the Authors themselves seem to suggest (e.g. on page 1926, lines 8-10).

Indeed, losses are typically due to intense wind gusts, which have a very strong spatial variability (see the point above).

3. Page 1920, line 5:

> "In this study, a new approach is used to separate events from daily maximum data:"

Unclear: do the Authors mean that a new approach is adopted to identify individual, distinct loss events in the daily time series? Please rephrase.

4. Page 1920, line 6:

> "The local temporal maximum of LIraw (MIraw) for a three-day gliding time window is defined as event."

This is unclear. How comes that the method is able to identify storms "Vivian" and "Wiebke" as distinct individual events (compare page 1925, line 5), which are separated by 1 day, if the maximum over 3-day gliding window is used? Please explain carefully, preferably with an example from the time series (preferably "Vivian" and "Wiebke" themselves).

5. Page 1920, line 15:

> "For each grid point ij, . . . is identified around each event date and aggregated to the LI3D (MI3D) of the corresponding date."

Unclear: what does it mean that the maximum is "aggregated" to the LI/MI? Please explain (possibly with an example).

6. Page 1921, line 1:

> "Only spatially coherent wind fields are accumulated to the events".

This is unclear in may different ways: what does it mean that wind fields are accumulated to the events? What does it mean that only spatially coherent wind fields are accumulated, that is, how was it ensured that only the spatially coherent fields were selected? How was this all implemented concretely?

7. Sections 3.3 and 4.3: I do not understand how the calibration is carried out, how exactly the time series of daily maxima or of individual events obtained from the GCM is transformed: figure 4(d) and sentences

> "The correction is done by adapting the relative frequency of events per CWT in the 25 GCM simulations to the number of events per CWT in the ERAI data (see 4.3)"

on Page 1923, line 25 and

> "This bias is corrected assuming the same frequency of events per CWT as in ERAI for GCM data."

on Page 1926, line 25 leave unclear (to me) what is actually being done with the time series. This point is important, as one of the main claims of the paper is that using the GCM data leads to a reduction of uncertainty, see the Abstract. Depending on how the calibration is performed, the reduction of uncertainty might even be a trivial result providing no additional information whatsoever.

8. Page 1925, line 23:

> "For each threshold, the selected LI samples (30, 15 and 6 events, respectively) are shown in Fig. 3."

It may be helpful, especially for a reader who is unfamiliar with the return periods/levels, to emphasise that the 5-year events are also 2-year and 1-year events: the blue bars in Figure 3 are counts of numbers which are also included in the

green and red bars. In other words, “return period” should not be taken as equivalent to “intensity”: the return period is a property of a set of events (not of individual events), where the set contains a mixture of events having different intensities. In this sense, I definitely agree with the spirit of the statement on page 1932, lines 9-13:

> “Future work should focus on an adaption of the choice of events per return level as it could be improved by considering a mixture of events with different return levels within one winter”,

although I find it unclearly written (please rewrite). It might be worth to slightly expand this discussion, perhaps in Section 4.3.

9. Page 1927, lines 23 and following: the Authors perform a sensitivity test by removing selected single years from the time series and refitting their models, however little comment is provided about the outcome of the test: what are the conclusions?

10. Page 1928, line 9:

> “This enables more accurate estimates of the return period as well as lower uncertainties (Table 2)”

How is the uncertainty in Ψ actually computed?

3 Minor points

1. Page 1925, 1st line of Section 4.2: Figure 3 is not a histogram, it is a bar plot.

2. Figure 3: it might be beneficial to use one and the same range [0;6] on the y-axes, to highlight the similarities and differences between the three datasets.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 1913, 2014.