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Comment

***Interactive comment on* “Brief Communication: Statistical detection and modeling of the over-dispersion of winter storm occurrence” by M. Raschke**

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Manuscript identification number: nheissd-3-1775-2015 Title: Statistical detection and modeling of the over-dispersion of winter storm occurrence Author: M. Raschke

Recommendation: The manuscript may become acceptable after major revision

General Comments:

The author presents a methodology to estimate over-dispersion from time series of

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windstorm losses derived by Karremann et al. (2014). While the manuscript reached basically the same conclusions as Karremann et al. (2014), it provides a very interesting discussion on the estimation of over-dispersion, its interpretation and implications. Therefore, I believe that this manuscript is potentially a very good contribution to NHESS. However, a few aspects should be revised / better explained before the manuscript can be accepted for publication.

Major Comments:

a. As the other reviewer also pointed out, NHESS is not a statistical journal. In my opinion, the current content of the paper is too technical for the readership of NHESS. Section 3 is particularly difficult to follow. While I believe the manuscript is already close to the word limit for a brief communication, I think it would be extremely helpful to expand the text to enhance its readability and its scientific value. This could be done either by turning this brief communication to a regular NHESS article, or by adding supplementary material to this brief communication (which is supported by NHESS, probably the easiest option).

b. The inclusion of some more “background information” would be extremely helpful for the reader. While the author should not repeat the all the methods of Karremann et al (2014), a short summary of their motivation, the analysed datasets, methodology and main conclusions (at best placed early in the manuscript) would be very helpful to understand the motivation and aims of the current manuscript. This should then be revisited in the discussion.

c. Along the same lines, it would be quite informative to add some general meteorological background to this issue. The occurrence of windstorm clusters in meteorology is actually a very old concept (e.g., Bjercknes and Solberg, 1992), and has been known for a long time as “cyclone families”. Given the occurrence of several recent winters with an anomalous number of windstorms within a short period of time (1990, 1993, 1999, 2007, and more recently 2013/2014), several papers have analysed clustering of

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windstorms from the statistical point of view (e.g. Mailier et al., 2006; Vitolo et al., 2009; Pinto et al., 2013). Recently, Pinto et al (2014) has given a “modern” interpretation of the clustering phenomena from the physical point of view – clustering occurs due to a combination of steering from the large-scale flow (and its quasi-stationarity over a period of at least one week) and the occurrence of secondary cyclogenesis (development of new cyclones on the trailing fronts of “parent” cyclones). So for Karremann et al (2014) the question was actually not if windstorm clustering exists or not (in physical terms, it does), but rather if clustering can be identified from time series of potential loss events in a statistical way. It is clear that it was difficult to reach robust conclusions based on the historical datasets (DWD, NCEP, ERAI), given the limited number of years (30) and the associated high uncertainties. However, this was the reason why Karremann et al (2014) support their conclusions with the GCM data, where the number of years is not a limitation. Given the above, it is actually very nice to see that the present manuscript reaches largely the same conclusions as Karremann et al. (2014) using different statistical methods. It is important to state this clearly in the manuscript as it will make the manuscript more relevant for the meteorology / natural hazards communities (which are admittedly often struggling with statistics).

d. Section 3 is very technical and thus very difficult to follow, both text and derivations. I would suggest adding more detailed explanations, maybe as a supplementary material.

e. Section 4 is also a bit difficult to follow, as the text is very condensed. Some more detailed discussion on the results presented in the table and the two figures would be helpful. Same applies for the GCM data. For example, it is clear from Fig. 2 that PD is not a good approximation for rare storm series (with 4 or more 1-year events per year). This should be discussed in more detail.

f. Section 5: Summary – here it would be relevant to link back the identified over-dispersion to the original aim of the paper and meteorology (see major points b and c).

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g. The paper could profit from detailed proof reading by a native speaker.

Specific comments:

1. There are quite a lot if typos in the manuscript, which should be corrected (e.g. “GCN”, “Karlemann”, etc.). All acronyms should be introduced and explained.
2. Page 1776, lines 16-19: In meteorological terms, over-dispersion of extra-tropical cyclones is not found over the whole spatial domain (North Atlantic and Europe), but rather “only” downstream and on the flanks of the main storm track area. There are physical reasons for this (see Mailier et al., 2006; Vitolo et al, 2009; Pinto et al., 2013; 2014). So the existence of areas with and without clustering has nothing to do with over-fitting of the parameters, but with the nature of the meteorological phenomena. Please rephrase
2. The variable used in Karremann et al (2014) is “potential losses associated with windstorms”, which is clearly defined in that paper and shortened as “LI”. This should be clearly indicated here (e.g. page 1776, line 23). Over-dispersion of windstorm occurrences has been considered elsewhere (e.g. Mailier et al., 2006; Vitolo et al., 2009; Pinto et al., 2013).
3. Page 1778, lines 16-19. There is some misunderstanding here. Per definition, the storms exceeding the loss correspondent to 5-year return level (6 events) are also included in the sample of storms exceeding the loss for a 1-year return level (30 events). The inclusion of more detailed information to describe the data would be helpful. Please enhance.
3. Section 3, pages 1779 and 1780 are extremely difficult to follow. I am sure that other non-statisticians will think the same. Please provide more detailed information of the derivation and what the parameters mean in practical terms (eventually as supplementary material).
4. Page 1781, line 8. I do not know if the AIC is very popular among statisticians, I have

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never heard of it. Please rephrase, and maybe add a reference with an application. Moreover, what it is an “appropriate model” (line 6) is always something subjective.

5. Page 1782, lines 18-20: And what does this mean? What is the conclusion? A sentence seems to be missing here.

References:

Bjerknes, J. and Solberg, H.: Life cycle of cyclones and the polar front theory of atmospheric circulation, *Geophysisks Publikationer*, 3, 3–18, 1922.

Mailier, P. J., Stephenson, D. B., Ferro, C. A. T., and Hodges, K. I.: Serial Clustering of extratropical cyclones, *Mon. Weather Rev.*, 134, 2224–2240, 2006.

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