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Comment

Interactive comment on “Performance of storm damage functions: a sectoral impact model intercomparison” by B. F. Prah et al.

Anonymous Referee #3

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The manuscript compares storm damage functions, which relate observed wind gusts to insured losses. Four different storm damage functions are compared using a dataset of German insured losses and two wind datasets (DWD wind station observations and ECMWF Interim reanalysis). Given that storm damage functions are used in a number of different fields (e.g. climate modelling) the results of the study will be of wider interest to the academic community and researchers in the insurance industry. The manuscript is well written, hypotheses are clearly stated and results and conclusion are generally clearly explained.

I have a couple of major comments on 1) the structure of the introduction and 2) on section 5. Although I've classed these comments as 'major', I think they should be relatively easy for the authors to address. On the proviso, that the authors address

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the major and minor comments, I'd recommend that the manuscript be published in NHESS. Major comments

1. The introduction very quickly becomes a rather detailed discussion of the methodology. Some text later in the manuscript would make more sense in the introduction and some relevant literature is missing. To make the introduction much more readable the authors should: i) Move some of the detailed material on the methods to section 2. ii) The short introduction in section 5 seems to be one of the key motivations for this study, and this text should be moved to the introduction. iii) There are other papers which consider how useful some of the metrics discussed here are in the context of insurance loss (e.g. Deroche et al. 2014, Roberts et al. 2014). These papers take a different approach than that addressed in this paper, but given they provide some additional context they should be mentioned here.

2. Section 5 is poorly argued and written. i) As mentioned above, the first few paragraphs should really be in the introduction. ii) Line 5: The manuscript claims that Fig 6a. shows that the Loss Ratio and Claim ratio curves in Germany increase approximately to the 10th power of windspeed. It's unclear which part of the range of losses the authors are referring to, but certainly for most economically relevant larger losses for windspeeds ($> \sim 25\text{ms}^{-1}$) then the LR and CR curves increase at substantially less than 10th power of windspeed, so I find the authors argument here unconvincing. iii) The authors then argue that the steepness of the windspeed-loss relationship and a cubic relationship can be reconciled as there is an implicit minimum loss threshold. This argument is very unconvincing. Firstly, the argument is qualitative and provides no evidence that it is relevant for the more economically relevant larger losses. Secondly, elsewhere in the paper the authors provide a quantitative argument that the cubic Klawa and Ulbrich loss model is steeper than cubic because of the 98th percentile critical windspeed threshold. This makes the argument presented in section 5 redundant. The authors need to either dramatically improve section 5 or remove it.

Minor Comments

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Title: The word “sectoral” is vague and not very helpful. I’d suggest replacing this word with “insurance” or “insurance loss” to help the reader get an idea of what the paper is about. The title should also contain the words “European windstorms”.

Abstract: Line 14: The last line needs to be rephrased. Explicitly state which models you think are best as the “probabilistic” is too vague. The word “variability” is vague in the context of the abstract and needs to be explained here or replaced.

1. p5836. Line 20 “Across Europe, losses from meteorological events are mainly caused by winter storms and comprise 68% of total insured loss,…”

This sentence is ambiguous, could you rephrase it to clarify what you mean by total insured losses. Is this the total of all losses, or the total of meteorological losses?

2. p5836. Line 24. The introduction should consider more reference about changes in European storminess than those only considering modelled insurance loss (as these rely upon only a handful of climate models). I’d suggest adding a sentence or two and reference some of the work with the CMIP3 and CMIP5 climate models (e.g. Ulbrich et al 2008, Zappa et al 2013).

3. Section 2. The authors make the decision to only use wind data from weather stations that have a nearly complete record. Given the heavy skew towards large losses, would it not have made more sense to make use of weather station data where there were complete observations for the largest loss windstorms? This would presumably allow a larger range of weather stations to be considered. I’m not expecting you to redo your analysis, but I wonder if you think this would a better approach.

4. p 5843. Line 17. What was this radius of interaction?

5. p 5847 Line 9. What are these ‘additional important parameters’?

6. P5850 line 5. Define the parameter H1 in equation 5.

7. p5852. Line 22-26. No evidence is provided for the sentences referring to Kyrill.

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Either explain in more detail or remove these sentences.

8. p5853. Line 13-15. The residuals are not shown on fig 4, so these sentences don't make much sense and should be removed (or the residuals plotted).

9. p5859. Line 14. As mentioned in the major comments above I found the qualitative argument about the impact of the minimum threshold very unconvincing and these statements should be removed.

10. p5862. Line 14. This sentence should refer to the two specific probabilistic loss models you've considered. You haven't shown that probabilistic loss models always perform better than "non-probabilistic" models.

Table 1 . What is 'no' in column 3? Over what time periods does this span. Please explain in the caption.

Figure 2. Labels on sub plots are missing (a), b) etc...)

References

Deroche, M.-S., Choux, M., Codron, F., and Yiou, P.: Three variables are better than one: detection of european winter windstorms causing important damages, *Nat. Hazards Earth Syst. Sci.*, 14, 981-993, doi:10.5194/nhess-14-981-2014, 2014.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5835, 2014.

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