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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Performance of storm damage functions: a sectoral impact model intercomparison" *by* B. F. Prahl et al.

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Authors' response to Referee #3

We thank the reviewer for the valuable comments for further improvement of our manuscript. Our response to the individual comments are given in blue script, with important changes to the manuscript highlighted by italic type.

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 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.

   We think that this is a very good suggestion helping to improve the structure of the manuscript. Accordingly, we moved the paragraphs between page 5838 line 11 and page 5839 line 7 to Section 2. Additionally, we also move the method descriptions of the binomial test and the MPE/MAPE metrics (page 5854 line 19 to page 5855 line 10) to the methods part of Section 2. All methods are now consistently described in 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.

   We have moved the text on lines 15 (p 5857) up to line 4 (p5858) to the introduction in the revised manuscript.

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.

The mentioned papers provide an interesting perspective on the issue of storm severity. We include them in the revised manuscript as follows:

A storm damage function describes the relation between a storm's intensity and the typical monetary damage caused. While on the continental scale storm intensity can be best described by complex storm severity indices (Deroche et al., 2014; Roberts et al., 2014), local losses are ultimately caused by surface winds. As the magnitude of storm loss is highly sensitive to changes in wind speed ...

2. Section 5 is poorly argued and written. i) As mentioned above, the first few paragraphs should really be in the introduction.

   See answer above.

   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 then 10th power of windspeed, so I find the authors argument here unconvincing.

   We apologise for the somewhat misleading comparison with the 10th power of wind speed. It was not our intention to suggest such a power law model, instead we wanted to give an indication how steep the increases of the CR and LR

curves are and that they are considerably steeper than a cubic power law. We removed the allusion to the 10th power both in the text and Figure 6 of the revised manuscript.

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.

Above all, we have to emphasize that the Klawa and Ulbrich loss model is fundamentally different from the cubic wind dependence that is discussed in Section 5. In Section 3.3 (p. 5848, lines 21 and following) we explain that the inclusion of a wind threshold makes the Klawa and Ulbrich loss model incompatible with the kinetic energy argument. Accordingly, we demonstrated that its local gradient at extreme gusts comes close to a power law with exponent 10.

Our qualitative argument is highly relevant to larger losses, particularly for the extrapolation of extreme losses. While the damage functions are well supported for non-extreme losses, conceptual considerations play an important role for the extreme losses, where data support is more or less sporadic. Our argument for the reconciliation with a cubic wind relationship serves as both a proposition for future model design and a criticism of current models.

We have substantially rewritten Section 5 (see below) and we believe that the intention and the argument of the section are much clearer now.

All of the four different damage functions discussed herein exhibit a loss increase *that is much more rapid than a cubic power law derived from physical considerations about the kinetic energy of the wind mass. In this section, we propose a simple mechanism to reconcile the steep loss increase with a cubic power law. With our hypothesis we intend to expedite the discussion on the overall shape of the damage curve, since its behaviour beyond the support has strong implications for the extrapolation of loss.*

Fig. 6a shows the average loss increase obtained when superimposing data from all German districts. Visual comparison with the power law guiding lines suggests that both the LR and the CR curves increase significantly faster than the 3rd power of wind gust speed. Moreover, the average LR of affected buildings (i.e. those for which an insurance claim was filed) remains approximately constant over a wide range of wind gust speed. This implies a minimum loss threshold for damage compensation to be claimed. Such a threshold could be caused by insurance deductibles, but may also arise from small damages that either go unnoticed or are fixed autonomously.

We make the hypothesis that the steep loss increase that is observed from the GDV data may be a consequence of the presence of such a loss threshold. Mathematically, when applying a threshold T the expected loss ratio $\mathsf{LR}_{\mathsf{all}}$ is given by

$$\mathsf{LR}_{\mathsf{all}} = \int_T^\infty L f_v(L) \mathrm{d}L, \tag{1}$$

where $f_v(L)$ denotes the uncertainty distribution of the loss ratio L at gust speed v. The claim ratio CR follows from the CDF of the uncertainty distribution, $F_v(L)$, as

$$\mathsf{CR} = 1 - F_v(T). \tag{2}$$

The loss ratio of affected buildings $\mathsf{LR}_{\mathsf{affected}}$ is then simply given by

$$\mathsf{LR}_{\mathsf{affected}} = \frac{\mathsf{LR}_{\mathsf{all}}}{\mathsf{CR}} \tag{3}$$

Assuming a log-normal uncertainty distribution, Figure 6b illustrates the effect of a loss threshold on the expected LR obtained from a simple cubic loss-wind relationship. As a result, for low wind gust speed $\mathsf{LR}_{\mathsf{affected}}$ remains close to the threshold value, while $\mathsf{LR}_{\mathsf{all}}$ steeply increases. The noise level of the GDV data however prevents decline below a minimum loss level, approximately corresponding to a single damaged building per district portfolio.

Minor Comments

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”.

We apologise that the title of our manuscript may have caused some confusion. The intention was rather to use the word ‘sectoral’ as an antonym to ‘cross-sectoral’. We thank the reviewer for his comment and propose to change the title to “Comparison of storm damage functions and their performance”. We would also like to emphasise that the compared damage functions are general mathematical relations between wind and monetary loss. As such they are not limited to a specific geographical region and have general applicability beyond insured loss.

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.

We have rewritten the last lines as follows:

While the choice of gust data has little impact on the evaluation of German storm loss, spatially resolved coefficients of variation reveal dependence *between model and data choice. The comparison shows that the probabilistic models by Heneka et al. (2006) and Prahl et al. (2012) both provide accurate loss predictions for moderate to extreme losses, with generally small coefficients of variation. We favour the latter model in terms of model applicability. Application of the versatile deterministic model by Klawa and Ulbrich (2003) should be restricted to extreme loss, for which it shows the least bias and errors comparable to the probabilistic model by Prahl et al. (2012).*

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?

We mean total insured loss caused by natural catastrophes. This has been added in the revised manuscript.

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).

We suggest to cite the review paper by Feser et al. (2014), which includes studies based on CMIP3 as well as on CMIP5 models. It also includes both of the aforementioned studies. We have added:

While there is no consensus on changes of winter storm frequency, a growing body of research supports a future increase in storm intensity (Feser et al., 2014).

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.

Yes, it would make sense to put more importance on data availability for large loss events. However, we still require a nearly complete record in order to avoid potential bias (e.g. from a station that is only partially available for a few years towards the end of the modeling period). We would expect only few, if any, changes to the set of wind stations, since the second condition is much more restrictive than the first. The effect would be a change in model uncertainty, not effecting the relative comparison of our models.

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

The optimal radii of interaction were approximately 130km and 60km for DWD and ERA Interim, respectively. We added this information in the revised manuscript.

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

We refer to remaining model parameters that are described in Section A2 of the appendix. We made this clear in the revised manuscript.

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

H1 denotes the maximum exceedance level at which damage reaches 1, i.e. complete destruction. We added this information to the revised manuscript

7 p5852. Line 22-26. No evidence is provided for the sentences referring to Kyrill. Either explain in more detail or remove these sentences.

The statement is not essential for our line of reasoning and was removed.

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).

We rephrased the paragraph for greater clarity:

Secondly, the model variability appears nearly symmetric on the log-scale, indicating a strongly skewed distribution. In this case expected values can be significantly lower than potential losses within the upper tail of the distribution, with the consequence that some observations may drastically exceed expected values drawn from simulation.

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.

Having rewritten large parts of Section 5, we also updated this paragraph as follows:

Before we discuss the detailed results of the comparison, it is of great importance to acknowledge the effect of deductibles on the shape of damage functions derived from insurance data. Care must be taken as to what extent physical damage concepts, such as a cubic wind-damage relationship, may be applied to insured storm loss. In this regard, all four compared damage functions exhibit a much stronger increase of loss, which is in good agreement with the GDV data employed herein. However, by introducing a simple loss threshold we could demonstrate how such a steep damage function for winter storm loss could be reconciled with a purely cubic wind-damage relationship. If, as climatological research suggests, future storm intensities increase beyond current levels, the overall shape of the damage function plays a crucial role for the extrapolation of future losses. With our threshold hypothesis we intend to expedite the discussion on the validity of damage functions beyond their original data support.

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.

We rephrased the paragraph as follows:

Both probabilistic models provided good results over a wide range of loss (moderate to extreme), with their model differences being much smaller than the general variability of losses. On the regional level, they yielded smaller coefficients of variation than the two deterministic models. While models H and P exhibited comparable results, a slight preference could be given to model P in terms of robustness and applicability. With regard to the broadly-skewed uncertainty of loss estimates, probabilistic models can give a better picture of potential loss and should generally be preferred. However, uncertainty estimates for extreme loss remain a concern and should be subject to further research.

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

We added the following: *The three loss classes defined for the winter half-year. Given are the number of observations, the related quantiles, and the accumulated loss share for the period 1997 to 2007.*

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

Labels were added to all sub-panels.

References

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

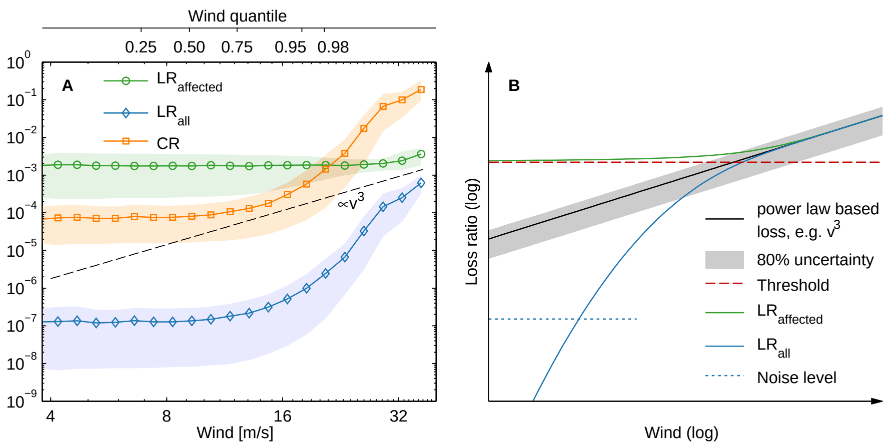

Fig. 6: Panel **(A)** shows the overall DWD gust dependence of the loss and claim ratio for all buildings and the loss ratio only of affected (i.e. damaged) buildings. Shown on a log-log scale, the solid curves represent expected values across all available districts and loss days, while the shaded areas indicate an 80 % uncertainty interval for observations. The dashed line provides a guide to the eye representing a power laws with exponent 3. The upper scale indicates the respective wind gust quantiles. Panel **(B)** shows schematically the decomposition of the loss ratio of a cubic loss-wind relationship subject to a minimum loss threshold. With a lognormal uncertainty distribution, indicated by the shaded 80 % uncertainty bounds, a picture similar to Panel **(A)** arises.

Fig. 1. Figure 6 of the revised manuscript