



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

**B. F. Prah et al.**

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### **Authors' response to Referee #2**

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 paper compares the performance of four damage functions for European windstorms over Germany, applied to the same underlying wind gust datasets. The paper points out that the physical arguments of the widely used cubic damage function are in-

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valid due to the subtraction of a threshold, resulting in steeper increase in loss with gust speed, comparable to the slope found in the power law function. I believe the paper presents new and interesting results which are worthy of publication, although there are a few issues which should be clarified/corrected, outlined in the specific comments below.

## Specific comments

1. Each model uses wind gusts as the meteorological variable (from either DWD or ERA-Interim), yet throughout the paper they are referred to as winds. As these two terms have quite specific meanings I think it is important to maintain the distinction to avoid confusion.

[We agree and consistently apply the term 'wind gust' in the revised manuscript.](#)

2. Page 5836, line 22: According to “Sigma: Natural catastrophes and man-made disasters in 2013” (Swiss Re, 2014), the largest loss European windstorm since 1980 was Daria in 1990 (although only slightly higher than Lothar). Perhaps it is worth mentioning this uncertainty.

[Having double-checked both MunichRe and SwissRe publications, the different ranking is due to the reporting of original values \(MunichRe\) and values indexed to 2013 \(SwissRe\). Since a fair comparison of storm severity should be based on indexed values, Daria's impact was indeed more severe and the manuscript has been changed accordingly.](#)

3. Page 5840, line 6: It would be useful to refer to Figure 3 here, so the reader can see the size and location of the administrative districts.

[It is included in the revised manuscript.](#)

4. Page 5841, line 15: Could you show the locations of the selected DWD stations

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(perhaps add to Figure 3)? This would help the reader understand how well each area is covered.

In the revised manuscript, we added a separate panel to Fig. 1 (see Fig. 1 herein), showing both the DWD stations and the ERA Interim grid cells.

5. Page 5843, lines 1-2: Are the gusts from ERA-Interim the maxima over the 3 hourly periods, or ‘instantaneous’ values (maximum over a model timestep)? If the latter, this would mean in some areas the maximum gust is underestimated in some locations for some events, which could perhaps be another of the reasons for the higher variability with ERA-Interim (page 5854, lines 5–9)

We used the 10m gusts values of each 3-hourly timestep. From these data, we took for each day the maximum over the 8 timesteps. There are certainly many sources of uncertainty in both datasets. While it would be highly interesting to investigate further, this is clearly beyond the scope of the present paper.

6. Page 5843, line 17. For completeness and repeatability, could you state what the radius was?

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

7. Section 3.3: In the appendix for this section, equation A9 says the loss for  $v < v_{98}$  is 0, although according to Donat et al (2011a) it should be  $K2$  (B in their notation).

We thank you for spotting this mistake and amended the equation accordingly. As you may see from Fig. 2, the calculations were correctly based on the actual equation by Donat et al., resulting in a ‘baseline’ damage  $K2$  (aka B) at all gust speeds.

Also, this damage function is usually weighted by population density. I can see this is unnecessary for analysing single districts (if each district is fitted separately and you assume an even distribution of buildings in each district), and I

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assume you simply added the predictions from each district to get the predicted countrywide loss rather than recalibrate at the country level. Could you confirm if this is the case? I think it is worth pointing out in the text why you do not need the population density in this study.

Yes, this is the case. We added the following paragraph to the revised manuscript:

*For data scarce applications, it may be opportune to resolve regional portfolio differences via population density as a proxy for (insured) value and obtain a global parametrisation via regression on the national level (e.g. Donat et al., 2011a). In contrast, the finely resolved loss data for our study allowed a local parametrisation and the simple summation of loss to the national level.*

In addition, on page 5848, line 5 and Page 5867, lines 8-12, it is mentioned that the calibration of model K is performed by linear regression. Could you be more specific on the linear regression fitting method (least squares, etc)? To make the fits less weighted on the extremes and more comparable to the other calibration methods, wouldn't it have been possible to bin the loss data as for model X? It would be interesting to mention how this would change the results (especially in the robustness of the model fit in Section 4.3).

We applied least-squares regression. This information has been added in the revised manuscript. In our understanding, binning can reduce the weight of densely populated data regions. As such, it would rather increase the relative weight of extremes in model K. For model X, binning was applied in conjunction with a logarithmic transformation. However, we do not want to impose such strong alterations on model K as this would create an altogether new model.

8. Page 5850, lines 14-19. Here it is mentioned that model H uses claims data in addition to loss data, therefore would be expected to perform as well or better than the other models. However, isn't this also the case for model P, where the claims ratio is needed to work out the occurrence rate?

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No, the occurrence rate simply approximates the probability that any damage occurs within the entire district at a given gust speed. It is estimated from the loss ratio, where values above zero denote an ‘occurrence’ and zero values denote the absence of such.

9. Page 5883: On the right hand panel of Figure 2, is the occurrence rate the same as  $C_H(v)$  in equation A14 and  $p(v)$  in equation A4? If so, could you make the terminology consistent?

No,  $C_H(v)$  and  $p(v)$  are not equivalent, as the first relates to an entire district and the latter to a single building. The occurrence rate is an empirical measure of the probability of occurrence  $p(v)$ . Regarding Fig. 2, in the revised manuscript we changed the right-hand axis label to ‘probability of occurrence’ as the focus should be on the model fit, rather than on the empirical values.

10. Page 5854, lines 15-18: As your loss data is daily, yet storms can do damage over more than one day, could you clarify if the number of days for each loss class given in Table 1 are all from different storms? If so, did you have to discard some days to make sure this was the case?

Generally, we apply the loss classification regardless if any two days belong to the same storm, or not. While all days in loss class I relate to different storm events (see also next response), this is, for example, no longer the case for loss class II, where 34 days relate to 26 different storms (8 storms cover 2 days each). In our view, this could only become an issue, if insurance data were biased towards attributing storm loss preferentially to the peak (day) of the storm event. However, at least for our own damage function, our previous research showed that the loss profiles for entire storm periods can be successfully reproduced, and the damage relationship appears to hold for smaller and larger loss days of the same event (cf. Prahla et al. 2012, Fig. 4).

11. Section 4.3. Could you clarify whether you mean the six most severe storms as  
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ranked by loss? Do these 6 storms correspond to the 6 extreme daily losses in loss class I?

Yes, the six storms were ranked by loss (we added this in the revised manuscript). They correspond to the six extreme daily losses in loss class I, but include further daily losses that fall into the period of the storm events stated in Tab. 5.

12. Section 5: I am not sure I understand the argument on the 2nd and 3rd paragraphs of page 5858. If each building has roughly equal value, then  $LR(\text{affected})$  is approximately equal to  $LR(\text{all})/CR$  (which from Figure 6a appears to be the case), so the fact that  $LR(\text{affected})$  remains roughly constant implies that for non-extreme gusts, as the gust increases the number of buildings making a claim increases, but they each claim roughly equal amounts (which is consistent with the theory that there is a threshold value for claims).

Yes, we do agree.

It is not until very extreme gusts that the loss for each building starts to increase with gust. However, if this is the case, how does the threshold explain the increase of CR as  $v^{10}$ ?

Firstly, we have to apologise for creating the impression that claims and loss ratio increase as a power-law with exponent 10. We simply wanted to indicate that the curves increase substantially faster than a cubic power-law. Since this aspect caused confusion also for the third reviewer, we entirely removed the allusion to a power-law with exponent 10 in the revised manuscript.

Secondly, the threshold explains the strong increase of CR by significantly reducing the number of claims for non-extreme gusts. To avoid overloading Fig. 6B, we have not drawn the CR curve, but as you correctly stated above, it can be inferred from the relationship  $LR(\text{affected})=LR(\text{all})/CR$ .

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Technical corrections/typos:

All corrections suggested below made their way into the revised manuscript.

1. Page 5869, line 21: "...normal distribution describing the random fluctuation" (remove the "by").
2. The term "wind source" used on page 5851 (line 4), page 5867 (line 1) and page 5870 (line 1) is unclear. Perhaps change to "wind data source" (or preferably "gust data source").
3. Page 5838, lines 3-4 "Theoretical foundations and implications of each model are discussed in order to mainstream terminology and conceptual structure of storm damage functions." Missing two "the"s in front of "theoretical foundations" and "conceptual structure".

## References

Prahl, B. F., Rybski, D., Kropp, J. P., Burghoff, O., and Held, H.: Applying stochastic smallscale damage functions to German winter storms, *Geophys. Res. Lett.*, 39, L06806, doi:10.1029/2012GL050961, 2012.

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

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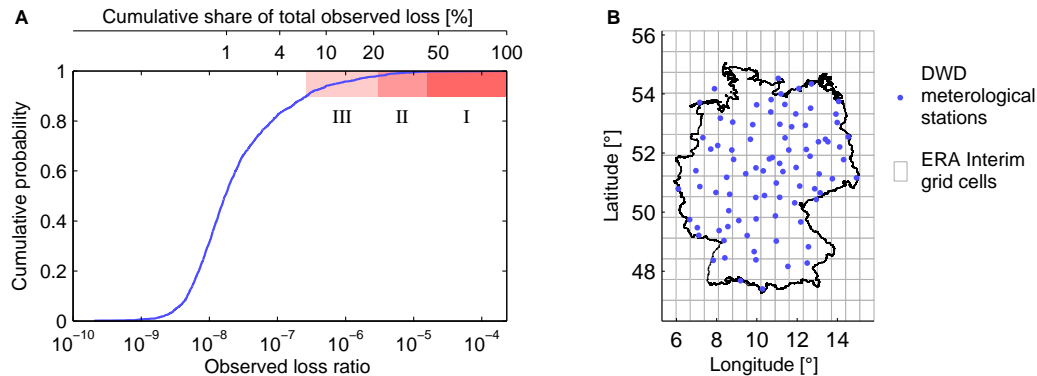
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Fig. 1: Panel A shows the empirical cumulative distribution function of loss days in Germany during the winter half-year. The observations comprise 2000 loss days, which exhibit a steep increase of loss at the upper end of their distribution. The shaded area indicates the days within the upper 0.1 quantile, subdivided into the three loss classes defined in Table 11. The top scale shows the share of total loss that is accumulated for all losses smaller than or equal to a specific loss ratio. Panel B shows the spatial distribution of the employed DWD stations and the ERA Interim grid cell resolution.

**Fig. 1.** Figure 1 of the revised manuscript

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