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

B. F. Prahl et al.

boris.prahl@pik-potsdam.de

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Authors' response to Referee #1

We thank the reviewer for his interesting comments. Our answers to the individual points are given in blue script, with important changes to the manuscript indicated by italic type.

The paper we are reviewing here represents an interesting approach towards the analysis of three pressing issues in the area of natural hazards.

First the evidences collected up to now show that extreme meteorological events will be a key driver of impact from climate change, hence a profound knowledge on the accuracy of mathematical modelling alternatives to capture the hazard profile is an essential issue to address, some comments could be opportune on the stability of the conclusions if new hazard profiles could emerge, focusing not only on the capacity to model extreme events but on its flexibility.

The comment addresses an important issue. As storm loss is ultimately dependent on surface wind, we believe that such damage functions would be capable to reflect changes in hazard profiles conditional on the capacity to obtain suitable wind fields. We have added the following paragraph to the discussion:

> *As a growing body of climatological research indicates, an increase in future storm intensity (see e.g. the review article by Feser et al., 2014) could lead to the emergence of new hazard profiles. Conditional on the accurate reproduction of local wind characteristics, gust-based damage functions can provide a flexible tool to assess these changes.*

Second, there exists a broad set of uncertainty sources that affect the actual nature of damage value and the accuracy of the evidence collected to characterize it. In this case on one hand we face the problem that accumulated damages may emerge after a sequence of catastrophic events that may offer a misguiding image on the performance and vulnerability of the built capital when exposed to an specific hazard level (not necessarily the highest), and on the other we might be working with a truncated damage distribution due to thresholds included in the insurance contracts and public regulation and the derived incentive effects on individuals behavior. I would recommend to systematize a set of careful comments that limits the validity of the statistical analysis to guarantee that irregular behavior, be it temporal (due to accumulated unobserved damage and collapse), spatial or sectoral due to different vulnerability and recovery capacity levels on geographical areas or economic sector and finally wealth level as we might face different insurance related behavior according with wealth level (different contracts, different assets quality, economic wealth effect etc..) in particular some reflection on the eventual heteroscedascity of insured value due to different values of declared to actual property value ratio could be of interest. Some comments on the relevance of temporal integration of damages to capture the actual accumulative exposure as explanatory variable would also be useful.

Yes, we agree on the importance of uncertainty sources for the analysis of storm damages. We follow the reviewers suggestion by adding a new paragraph to the discussion in Section 6:

> *It is worthwhile to note that the coefficient of variation indicates a strong level of residual error variance even for the best-performing model. The advantage of DWD over ERA Interim gust data (cf. Fig. 3) suggests a strong influence of uncertainty in wind gust data. However, there are also a number of potential uncertainty sources connected to the employed insurance data. Uncertainties may arise from gradual damage accumulation masking the effect of individual storms, from incentives for insurance holders (e.g. deductibles), and from wealth levels that affect both building quality and insurance taken. While the employed data does not allow a stratification of losses along socio-economic dimensions, our regional calibration implicitly accounts for spatial variations due to regionally differing vulnerability and wealth patterns. An altogether different situation would arise for models calibrated on a national scale, where such effects must be considered explicitly.*

The third relevant question that emerges is related with the purpose of the paper itself.

As it is explicitly declared in point 5 "Towards a synthesis of storm damage functions", we face here an opportunity to show how a relevant research with a robust statistical analysis would provide the reader with a criterion to select an accurate model to perform further analysis, hence some clarifying table or graphical representation on the validity of the tested solution and the restrictions for its use would be of great help for this purpose and a clearer description of the steps towards a synthesis would also enrich the document.

In our opinion, the purpose of the paper is twofold. Firstly, through the comparison we provide the reader with selection criteria for picking the right model. We do agree that a clear synopsis in the form of a table does enrich the manuscript and we have added this to the conclusion (see next comment). Secondly, in Section 5 we go one step further, by considering how the steep ascent of the damage functions for European winter storm loss could be reconciled with theoretical expectations that on first sight appear incompatible. This is intended to expedite the discussion on valid curve shapes beyond the data support from winter storms, with strong relevance for the extrapolation to extremely severe storms, given that climate change is predicted to have an amplifying effect on storm intensity.

On the conclusions of the work a clear conclusion should be derived from the document, the results of the models tend either to overestimate or underestimate average or extreme damages, some table or graphical description clarifying the ranking of the tested solutions on each of the cases will improve the readability of the document.

We have added a table (see Table 7 aka Fig 1 herein) which provides a stratified ranking of the four damage functions. In response also to reviewer #3, we have further sharpened our findings both in the abstract and the conclusion. In the abstract we now write:

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).

The conclusion now reads as:

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 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.

As a final comment I would like to focus on the title that puts the finger on the sectoral differences on mathematical functions performance. Under this title one would expect to observe some kind of measures that compare the accuracy of the damage functions when applied to domestic assets, industrial facilities... but none of this is observed in the document, the analysis presented compares different mathematical formulation and checks its validity to predict damages for one single sector. I'd recommend some

clarification about this.

We apologise that the title of our manuscript caused this misconception. The intention was rather to use the word 'sectoral' as an antonym to 'cross-sectoral'. We thank you for explaining your thoughts and propose to change the title to "Comparison of storm damage functions and their performance".

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

Table 7: Ranking of the four damage functions according to their prediction quality, variability, and applicability.

Extreme loss predictions (loss class I)		
1. {	P	least error, small bias
	K	small error, least bias
2.	H	slightly worse error, moderate positive bias
3.	X	strong error and strong positive bias
Moderate to large loss predictions (loss classes III and II)		
1.	H	good prediction, positive bias for ERA, smallest bias for DWD)
2.	P	good prediction for large loss, positive bias
3.	X	reasonable prediction for large loss, smallest error and least biased for moderate loss
4.	K	reasonable prediction, strong bias flipping from negative to positive
Variability on district level		
1. {	H	best for DWD, overall good for ERA Interim
	P	very good for both gust data sources
2.	K	generally better for ERA Interim; best in north-eastern, worst in southern Germany
3.	X	worst for DWD, large variability for ERA Interim
Model applicability		
1.	K	simple calibration, also on extreme losses only
2. {	P	requires data for all sizes of loss
	X	requires large training dataset
3.	H	both number of claims and loss data required

Fig. 1. Table 7 of the revised manuscript