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Natural Hazards and Earth System Sciences
Discussions



# *Interactive comment on* "Damage functions for climate-related hazards: unification and uncertainty analysis" *by* B. F. Prahl et al.

**B. F. Prahl et al.**

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We thank the reviewer for the valuable comments to improve our manuscript. Our response to the individual comments are given in blue below.

The study aims at presenting a unified approach to damage functions in the context of natural hazards. The approach consist of two steps. One is the definition of a parametric 'micro-scale' damage function describing the local damage as a function. This function depends on of the intensity of the hazard (hazard indicator, indicator e.g. storm surge) and a local parameter (e.g. altitude of building). In the second step, the macroscopic-scale damage is calculated as a convolution of the local damage function



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with the probability distribution of the local parameters (e.g. distribution of building altitudes with respect to zero sea-level). The macroscale damage would be, in this theory, just an aggregation of the local damages.

According to the authors, this approach can be applied to other types of hazards, like heat-waves, with little theoretical modifications in the basic structure of the theory.

I think that the goal of a unified description of natural damages is desirable, and that this study contributes to that goal. My main objection is, however, that the manuscript is not well written. Many paragraphs, even whole sections, are unnecessarily complicated, using jargon that is really not required and that quite possibly would put off interested readers. I include some examples below, but my general recommendation concerning the text itself is that it would require an extensive rewriting for the sake of clarity.

We thank the reviewer for pointing us into the right direction. This comment goes in line with similar comments from the other reviewers. Our intention was to deliver a comprehensive and precise, yet accessible, manuscript. As this goal has apparently not yet been achieved, we will carefully revise the manuscript, stripping overly complicated jargon and making our wording more concise.

One suggestion that the authors may want t consider is moving considerable parts of the appendix into the main body. This material actually contains some examples that seem quite helpful to understand the main text, and they would be more useful there that as only supplementary information that could be later read.

This point has also been brought up by the third reviewer. We will integrate Appendices A and B into the main body. Regarding Appedix C, we believe that information about the parametrisation is not essential in supporting the core message of the manuscript and should thus remain separate.



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The study also includes a discussion of the uncertainties attached to this approach, mostly stemming from the form of the microscale damage function, and of the relative importance of the contributions to uncertainty for different magnitudes of the hazard indicator. I missed, however, here a more through analysis or discussion about the uncertainties stemming from the tails of the local damage function. In the case of flooding of cities, it seems to me that the local damage function is bounded by the total costs of the building, but in other settings in which the costs are more problematic to estimate – for instance, crop failure after droughts – the uncertainty in the tails of the local function may constitute the major source of variance in the estimation of damage. This discussion would address the case of low-probability/high impacts risks. Regarding this point, I have here some doubts that the approach envisaged in the manuscript can be universally applied, but I may be wrong. In any case, a discussion also of the potential limitations in other applications seems warranted, as the authors claim that the approach is universal.

The reviewer has brought up an interesting point. In our view, the upper limit of the damage function is subject to uncertainty, which we labelled asset uncertainty (stemming from our focus on building damage). Our sensitivity analysis identifies this uncertainty as most relevant on the microscale level. On the macroscale, we see the effects of diversification such that the uncertainty in hazard magnitude dominates for large portfolios. It is, however, arguable that the choice of crop is highly correlated regionally. This would reduce the potential for diversification and increase the relevance of this uncertainty.
We will address this interesting point in our revision of the manuscript.

As I wrote, a universal approach also requires to have readers in mind that may have quite different backgrounds. I would strongly recommend the authors to have those readers in mind when revising the manuscript.

The reviewer rightfully asks for keeping the manuscript accessible for readers of



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different disciplines. However, the third reviewer calls for more technical precision. In the revised manuscript we will hence strip unnecessary jargon and carefully introduce specific jargon where required.

Some examples:

-a variance-based sensitivity analysis. As the overall damage is effectively an aggregation of microscale damages, the analysis should take the different scales into account. 6856

'variance-based sensitivity analysis' is unnecessarily unclear. Why not write 'analysis of the different sources of damage uncertainty' ?

The applied method has been named 'variance-based sensitivity analysis' in the literature. It should be distinguished from a general 'uncertainty analysis'. We will rephrase the paragraph to make this clear.

-'and (ii) the city, i.e. macro scale. As an intermediate step, we consider the sole effect of intrinsic uncertainties on the macroscale damage. On each scale, we use a Monte Carlo sampling size of 10 000 and obtain boot-strapped confidence intervals from resampling 1000 times. We apply the Jansen estimator (Jansen, 1999; Saltelli et al., 2010) to estimate the total-effect index. A detailed account of the standard methodology used is included in the Supplement.'

This is an example of what, in my opinion, is a wrong text structure that can be often found in the manuscript., The reader is first confronted by some description of the application of Monte Carlo methods, but the reader at this point does not know the purpose of this Monte Carlo sampling nor what is actually re-sampled. : what is sampled ? Which is the purpose ? Confidence intervals of what ? This is unnerving. Only later can the reader find a hint of an answer to those questions.



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We understand the objection of the reviewer. Unfortunately, we did not succeed in giving a brief overview of a rather complex procedure, but instead caused confusion with the reader. We will carefully revise this paragraph, putting things in a sensible order and integrating information from the supplement. Furthermore we will check the entire document for clarity.

-In the following sentence, the reader finds the sentence 'We apply the Jansen estimator (Jansen, 1999; Saltelli et al., 2010) to estimate the total-effect index. A detailed account of the standard methodology used is included in the Supplement'. The totaleffective index is, however, defined in the following paragraph. This places the attention of the reader under stress and provides a very unclear text.

Right. This will be revised together with the previous example.

-'The total-effect index denotes the fraction of output variance (variance in macroscale loss) that has been caused by the variance of the respective input variable including all variance caused by its potential interactions (correlation) with other input variables. '

Is the 'total-efect index not just the amount of variance of a dependent variable 'damage' explained by an input variable ? Why not use simpler terms that are more commonly used by the vast majority of readers ?

We agree with the reviewer that this paragraph requires clarification. In the particular example, it is however important to mention the potential interactions.

-'variance caused by its potential interactions (correlation) with other input variables. For the employed damage function, first-order effects dominate, and secondary interaction play a minor role only for small inundation/flood levels.

At this point, the reader has been confronted only by one input parameter of the local



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damage function, lambda. What could secondary interactions between one parameter be? Are there any primary interactions ?

Here the terms secondary effects and interaction got mixed up. Secondary effects are interactions between two variables. We will correct this.

-'error, is of particular importance, as it represents the only extrinsic uncertainty and hence does not scale with the inverse root of portfolio size. The complex behaviour seen, can be decomposed into two main aspects. Firstly, the relative importance of'

Which complex behaviour ? Seen where (maybe a Figure) ?

This refers to Fig. 5c mentioned at the beginning of the paragraph. We will rephrase this.

-'role as the dominant source of uncertainty. In contrast to the intrinsic uncertainties, whose standard deviation increases approximately with the root of the portfolio size,'

Intrinsic uncertainties have previously been defined as those arising from the form of the local damage function. How can they now depend on the overall portfolio ?. I think that the authors mean that the aggregate effect of the intrinsic uncertainties increases with the square root of the portfolio size, but not the intrinsic uncertainties themselves. This is again an example of inaccurate language scattered throughout the manuscript.

The reviewer is correct. We will revise the manuscript to avoid such inaccuracy.

- 'Since the shape of the microscale damage functions is solely dependent on the hazard magnitude, different shapes could be obtained via simple axis transformations.'

This happens only for one particular one-parameter family of damage functions chosen by the authors, but not in general (?). Also, why use the expression 'axis transforma-



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tions' when the authors just mean re-scaling ? Axis transformations imply a change of coordinates in a multidimensional space, and I do not think this is the meaning the authors want to convey here.

The reviewer is right in objecting to the way we have phrased our statement. We intend to make the point that in this special case a re-scaling of the x-axis suffices.

-'The damage function provides a means to estimate the loss caused by hazard events of a specific magnitude. It requires the definition of an indicator, or proxy, for the hazard magnitude, which should provide the highest explanatory power in regard to the damage type under scrutiny.'

So the hazard indicator, according to this definition, does contain an element that actually belongs into the definition of risk, since it is optimized with respect to the damage ? In my understanding both should be kept separate. I think that a clarification would be helpful for the reader.

We are unclear about this comment and what the reviewer means by the definition of risk. The term 'risk' is frequently split into the components hazard, vulnerability, and exposure. In our view, the magnitude of the hazard determines the damage. However, it is unclear in practice to measure this magnitude directly, and damage assessment relies on hazard indicators that offer a high correlation with damage. We will clarify this point, accordingly.

-In equation 1, define g

Will be done.

-Fig 3 is mentioned before before Fig 2 in the main text. This is odd and distracting.

We will set the figures in the right order.



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-'Figure 1 shows three different examples of previously published damage functions. '

Prior to this sentence, Fig. 2 and Fig 3 have already been mentioned. I would write 'Fig 1 _also_ shows..', to avoid confusion.

We will reorder the sub-figures and set the figures in the right order with the text.

- Similarly, the model can be set up the mortality rate is a measure of fa

The word model is used here for the first time. which model (probably the one described in the equation, but the reader has to guess)?

We will be clear about this in the revised manuscript.

- 'tational error should be negligible, uncertainty due to mathematical approximation and overall conceptualisation (e.g. functional form and parameter choice) plays a significant'

consider replacing 'overall conceptualisation' by something more intelligible like 'model structure'

Thank you for the helpful suggestion.

- 'role. Schröter et al. (2014) show that models of increasing complexity not only reduce modelling uncertainty but also foster transferability.'

I am not sure this is true. The use of complex models does not necessarily result in smaller model uncertainty. The model is just more complex, not more certain.

We will discuss the circumstances under which this statement hold in the revised manuscript.



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-' against reported losses. On the most general level, data uncertainties can be separated into intrinsic and extrinsic uncertainties. Intrinsic uncertainties arise from local variation or random fluctuation within the considered portfolio and affect the damage assessment of each individual portfolio item. In contrast, extrinsic uncertainties arise from external modelling or measurement and globally affect the entire portfolio. As such, they must be considered for the application or validation of the macroscale damage function.'

I found this paragraph extremely confusing. The paragraph is dealing with data uncertainties. But then, external uncertainties arise from _modelling_ (??). Also the structure of the paragraph, if I understood it right, can be simplified. Does intrinsic uncertainties just denote random effects and extrinsic uncertainties denotes the overall systematic bias ? If yes, why not use those terms ? Alternatively, the authors should define what they mean by data. It seems to me that the word data is used quite sloppily through the text

We will revise the paragraph. 'External modelling' refers to taking a modelled hazard magnitude (e.g. from a climate model) as input variable. We acknowledge that the wording has been ambiguous and will be improved. We will also improve the classification, stating clearly which sources of uncertainty we consider and which we do not consider.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6845, 2015.