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

**Anonymous Referee #1**

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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 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.



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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.

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.

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.

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.



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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' ?

-'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.

-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 total-effective index is, however, defined in the following paragraph. This places the attention of the reader under stress and provides a very unclear text.

-'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. '



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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 ?

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

-'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) ?

-'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 .

- '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 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.

-In equation 1, define g

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

-'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.

- 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 )?

- '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'



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- '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.

-' 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

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