Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-251-RC2, 2017


Natural Hazards and Earth System Sciences Discussions



# *Interactive comment on* "Epistemic uncertainties and natural hazard risk assessment. 2. What should constitute good practice?" *by* Keith J. Beven et al.

**Anonymous Referee #2**

Received and published: 12 October 2017

The paper has considerably changed - and improved - compared to an earlier version. Still I would suggest to consider some major issues:

1. I struggle a bit to imagine a potential audience for whom the paper is really useful (i.e., introducing something new rather than summarising known issues). Since the paper aims to lay out what constitutes good practice, a natural target audience would be not only experts in the field, but also researchers trying to get into the field. But several parts, in particular section two, is extremely dense and difficult to read for non-experts. One example is the explanation of micro-correlations, which is essentially not understandable from the text. Here it would help to provide more (or better) expla-



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nations and, for the different analysis approaches, a comparative discussion to make the paper more accessible. During rewriting, the authors should also consider the terminology. As stated previously, the term "simulator" is widely used in the statistics community, but a climate modeller, e.g., might not realise that it might refer to a climate model. Such key terms should simply be defined when used for the first time.

2. I still find the manuscript somewhat unbalanced. Almost one third of the references are self citations. In particular I wonder whether well known general statements have to be backed up by self citations if there might be older standard works available (e.g., page 4, line 31).

3. It should be worked out that the distinction between aleatory and epistemic uncertainty is often not clear cut, but may evolve in time, or may be simply due to pragmatic reasons. So often we treat uncertainties as aleatory rather than assuming that they are intrinsically stochastic. This is implicitly mentioned in the manuscript, but could be made more explicit.

4. I find the recommendations regarding the treatment of future projections (last paragraph on page 10) somewhat misleading. It is not in general justified to treat GCM or downscaled simulations equally - this is a question of what the models are supposed to represent. A GCM might miss local feedbacks and thus provide an implausible change signal. So here a decision has to be drawn on a case-by-case basis whether GCMs should be considered or downscaling is necessary (we have three possibilities: 1. GCM and RCM can be treated equally, 2. RCMs are credible, GCMs implausible, 3. the RCM is doing something wrong and the GCM is credible (of course one might also consider a fourth case where no model is fit for purpose.)

5. Related to this issue: the discussion of RCMs (page 16, line 16-19) should be modified. The cited studies suggest that standard RCMs (which do not resolve convection) do not realistically simulate the response of extreme convective precipitation, but very high resolution RCMs may indeed (which explicitly represent deep convection). This

finding also backs up my statement 4 (that different types of models serve different purposes and should not be treated equally).

Final comment: I mildly disagree with the other reviewer regarding non-probabilistic approaches. In particular wrt time varying risk and extrapolation (such as in the case of climate change), the issue is not about estimating probabilities. Here a Bayesian approach might help to formally attach probabilities to certain future simulations, but I am wondering whether this would not give false confidence - because in many situations we simply do not have the knowledge to come up with meaningful prior distributions.

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