

Interactive comment on “Epistemic uncertainties and natural hazard risk assessment – Part 1: A review of the issues” by K. J. Beven et al.

Anonymous Referee #2

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The authors attempt to present a comprehensive review about the role of epistemic uncertainties in natural hazard risk assessment. Before publication, however, substantial work needs to be carried out to improve the paper.

I. My major criticism is that the authors have not clearly defined the audience of the manuscript. NHESS is an interdisciplinary journal read by all different types of earth scientists, practitioners and decision makers. Topics include, among others, hydrology, geology, climate sciences, geomorphology. Yet the paper fails to be accessible for these audiences, for several reasons: 1. several specialist concepts are only briefly introduced, if all. For example, bias correction or downscaling are rather complex issues, but are not even explained in two lines. Even references to state-of-the-art overviews are missing. 2. also general statistical or risk assessment concepts are not well ex-

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plained. For instance, the SEJ approach is presented, but not explained. 3. The paper tries to be comprehensive, but as a result often merely paraphrases issues rather than guiding the reader about problems and issues. It is often lost in detail, doesn't see the woods for the trees. 4. The language is often not appropriate. For instance the term simulator is widely used in the statistical literature, but it is basically unknown to people using these simulators. They would write of dynamical models, climate models, hydrological models etc. But still the term has not been clearly introduced in the paper. This (and similar issues) make the paper difficult to read for non-experts. All in all one sometimes has the impression that the authors simply want to impress the reader about how much work they have done in the field rather than providing a useful account of the issues.

II. Related to the previous point: given the scope of the journal and the experience of the authors, I find it slightly irritating that at least one third of the references are self-citations - many key publications from the individual disciplines are missing. For instance in climate science there is a whole bunch of key papers written by, e.g., Reto Knutti, Claudia Tebaldi, or David Stainforth, which has not been cited. Further examples will be listed below. I would guess that the same holds for other disciplines.

III. Again, related to the issues in I: often, the manuscript does not attempt to bring across the most relevant points. In one particular case I find this dangerous and misleading: the author at several places state the role of unknown unknowns, but fail to work out and highlight the associated problems. They nonchalantly state that "modellers are pragmatic realists" who know that models are simplified representations of reality, and at other places mention the possibility for surprises. They even state that experts often underestimate the uncertainties about issues not directly related to their own field of research. So they claim that the assumptions made in developing such simplified models need to be communicated to stakeholders. But I think this is a dangerously naive view of how stakeholders perceive science. In many cases, surprises are really to be expected, and scientists, if thinking clearly about their work, know

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that their uncertainty assessments are basically useless because the assumptions are overly simplified. But still, they sell these results to stakeholders, mentioning the uncertainties, and wrap them in nice scientific parlance. From my own experience with stakeholders I know that they are often completely unaware of the weak basis our predictions are based on, because they lack the scientific knowledge of really grasping the seriousness of our model deficiencies. Here I would really urge to add some real world examples, where predictions have utterly failed because models were too simple. A famous example is the recent financial crisis, but most likely the authors know much better examples from natural hazards. I found none, because in all the examples listed - Katrina, Aguila, Fukushima - it was about wrong management decisions.

IV. Actually, the authors themselves fall into the trap I described above. I found several examples where they lay out specific examples and then fail to properly describe the associated epistemic uncertainties because they lack the specialist knowledge. For instance, the authors describe downscaling and bias correction, but place naive statements such as "to correct for any bias predicted and observed values" because simulator variables are not commensurate with observations. Bias correction is a typical example of a wide spread technique that is applied without much understanding of the underlying climate and climate model errors such that bias corrected multi model ensemble projections might provide severely ill-designed uncertainty assessments which might lead to wrong adaptation decisions.

Specific issues:

page 7330, lines 4 to 18: here, human reflexive uncertainty should be discussed. In particular in climate sciences, it is only possible to provide probabilities (if at all) conditional upon emission scenarios, because the evolution of climate in the future will influence mitigation decisions in an essentially unpredictable way.

page 7341, lines 21-28: in the last ten years there have been several now classical review papers about statistical downscaling and bias correction which have not been

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cited here (e.g. Fowler et al., *Int. J. Climatol*, 2007; Maraun et al., *Rev. Geophys.*, 2010). Also the body of critical papers about bias correction is growing fast, but none have really been cited here.

page 7349, line 22: I think the paragraph about sensitivity studies is key, also in the light of the issues I discussed above about surprises. For instance, recent papers showed that our current generation regional climate models might not be fit for purpose to simulate heavy summertime extremes (e.g., Kendon et al., *Nat. Clim. Change*, 2014; Meredith et al., *J. Geophys. Res.*, 2015). These problems have been revealed by sensitivity studies and highlighted that traditional uncertainty assessments would have utterly failed.

page 7351, line 18: there are no projections on decadal scales, only predictions. On such short time scales, scenario uncertainties don't play any relevant role yet, i.e., one really produces actual predictions.

page 7352, lines 20-22: such sentences need much more emphasis!

page 7353, lines 1-2: remember, there is no free lunch. If there is a fundamental problem, also sophisticated Bayesian approaches will not provide a way around.

page 7353, line 15: grammar, verb is missing

page 7356, line 3: arise, not arises (refers to issues)

page 7357, line 7-17: this paragraph needs a much more critical stance, as discussed above. I fear the approach laid out here simply helps to avoid liability, but does not improve the basis for decision makers.

page 7357, line 18-: this paragraph is one example (of several) where the authors are lost in detail. It is completely inaccessible to non-experts. Please provide explanations or delete it.

page 7358, line 17-: this paragraph is too vague

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page 7360, line 19-21: too vague, and even misleading (see discussion above).

page 7361, line 6-13: surprises are not just possible, but very likely! The whole paragraph is too vague and uncritical.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 7333, 2015.

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