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

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Dear Keith and co-authors,

Please let me start by apologising for the length of time this paper has been in review. Until about a month ago I was waiting for comments from a third reviewer, whom I asked to look at an aspect of the paper that I myself am relatively unqualified to comment on. Unfortunately, despite repeated assurances the review didn't materialise. For some reason it then took the editorial office another month or so to close the discussion - I think there may have been some crossed wires with me here, and unfortunately I have been very preoccupied with other things.

The invited reviewers for this paper were all people who are qualified to comment on

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generic methodological aspects. You have doubtless read the two existing reviews already, and seen that they both express disappointment with the paper. Reviewer 1 finds it disjointed, and expresses a view that it would have been useful to understand which approaches might be more or less suitable for different hazard areas (and why). This reviewer was chosen for their "landscape-scale" appreciation of the subject matter. Reviewer 2 was asked specifically to comment on aspects relating to climate and related issues: this reviewer is also very critical, and adds some detail to the concerns raised by Reviewer 1. Specific concerns here are that the paper is not accessible to a wide readership (this links with the "disjointed" concern from Reviewer 1) because, for example, technical or subject-specific terms are not defined clearly for the benefit of those who are unfamiliar with them. This reviewer also notes that the paper overemphasises the authors' own work to an inappropriate degree. I strongly endorse this view, and note that a similar comment was made in relation to the companion paper.

To the reviewers' reports, I add here some detailed notes of my own. Thus: given that the aim of this NHESS special issue is to showcase new approaches for estimating risk and uncertainty in natural hazards, any review articles will ideally go some way beyond the "standard" level of review in this area. I must confess that although the present submission makes some very valid points, much of the material feels rather familiar and to echo points that have been made repeatedly elsewhere, albeit often in hazard- or discipline-specific outlets. Given the very substantial effort that has been made in the last few years to bring communities together and transfer ideas between disciplines, in the UK in particular, I am disappointed that the current submission misses an opportunity to "set the record straight" by articulating valid viewpoints that have perhaps received limited exposure in natural hazards communities. Indeed, at some level I don't think the contribution is really a "review" so much as an "opinion piece" - and I have some doubts as to whether all of the authors have *really* signed up to all of the opinions expressed therein (certainly, there are some statements in the paper that surprise me when I look at the author list). As with the companion paper: if this article is to deliver on what it promises, it needs to be much better balanced and to show considerably

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more awareness of other relevant literature.

The following are some aspects regarding which I have serious concerns:

- There are implicit criticisms of probabilities as being potentially inappropriate for representing different types of uncertainty: but in all cases (for example the material in lines 82-2) these are predicated on an interpretation of "probability" to which a Bayesian, for example, would not subscribe. Thus, many NHESS readers will interpret probabilities in the way that the authors imply: but for a review of this nature there is an obligation, I think, to acknowledge and articulate clearly the alternative viewpoint in which probabilities are used merely as a calculus to represent one's knowledge about the state of the world. I do not for a moment disagree that other uncertainty concepts may be useful (as indicated in lines 82-85); but it must be made absolutely clear, with appropriate explanation, that the arguments against the use of probability are perhaps more precisely regarded as arguments against the *interpretation* of probabilities that many hazard scientists are familiar with: the problem is not necessarily with the probability calculus per se.

- There are two or three places in the paper where rather sweeping statements are made about the disadvantages and (implied) inapplicability of specific techniques, but where the fundamental problem seems to be with the implementation rather than the underlying concept itself. For example:

. Lines 205-206 "even experts find it difficult to estimate probabilities for sources of epistemic uncertainty with any degree of confidence". This is a standard criticism levelled at probabilistic elicitation exercises in the natural hazards community. However, the problem arises at least in part because natural hazards experts invariably are not trained in how to interpret probabilities in such situations, and elicitation exercises also are often carried out by those lacking appropriate technical (i.e. mathematical and statistical) skills and awareness. I am also aware that these kinds of techniques *are* routinely used in other application areas, and I believe a considerable amount of work

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has been done on the elicitation of probabilities in such a way as to be relevant for the subsequent uncertainty analysis (this is, however, an area in which I was hoping to obtain additional input from the third reviewer). It is certainly challenging, and in general it requires a skilled and experienced facilitator who knows what questions to ask and how to ask them (as, indeed, you acknowledge on lines 379-382): in non-critical situations therefore, the costs of such an approach might be deemed to outweigh the benefits. But in this kind of review article, you have a responsibility to paint a balanced picture and to ensure that the *narrative* (rather than just the occasional parenthetical remark) is faithful to this picture.

. Lines 262-264 "Use of simple aleatory error based likelihoods or probabilities does not allow enough potential for surprise from arbitrary rather than aleatory future occurrences": again, this confuses concept with implementation. I agree 100% with the statement as written; but the implied conclusion (that the problem is with the use of likelihoods or probabilities for aleatory uncertainties) does not follow. In my view the key word in the quotation above is "simple". This is particularly relevant given that it follows on from a discussion of stochastic downscaling with bias corrections. Bias-correction approaches are mostly jaw-droppingly naive, and there is plenty of literature around that not only makes this point but also highlights the existence of much more sensible downscaling approaches that address many of the concerns (why are there no citations to papers by, for example, Douglas Maraun and co-workers?). Of course, you can never rule out the "black swan" (I hate that expression) - there's a really nice example in one of Stuart Coles' papers, involving a flash flood in Venezuela that was off the scale by comparison with anything in the historical record. But you can certainly improve by one or two orders of magnitude on "typical" current practice: just ensure that the work is done by, or in collaboration with, people who have the skill set and required training to make a decent job of it. In terms of handling epistemic uncertainties in natural hazards, lack of appropriate skills is at least as big a problem as anything else that is mentioned in the paper!

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. Line 491 "disinformative": please define precisely what you mean by this - I have never seen a clearly articulated definition. My best is guess that you think data are "disinformative" if the analysis would be better (in some sense that I don't fully understand and that I would like you to define) without them than with them. However, there is no such thing as negative information. In an ideal world, one would acknowledge explicitly the potential of the data to be incorrect for whatever reason, and would formally incorporate this into the analysis. This might result, for example, in the "disinformative" data values having negligible influence on the results. The thinking here once again seems predicated on the assumption (which is, unfortunately, reasonable in many situations) that the risk assessment is being done using naive and simplistic methods, and by someone who lacks the skills fully to address the problems of combining data and models in a complex situation. It may be judged that it would cost too much to hire somebody to do this kind of work in any particular application, or that it would be too time-consuming; but let's be clear that the problem about "disinformativeness" is not a philosophical problem as the paper seems to suggest: it's a logistical problem about resource availability / allocation. Thus, in lines 505-508 there's a hypothetical example involving mass balance errors in data: this is easily resolved *in principle*, simply by allowing for uncertainty and incorporating the requirement for mass balance formally *and appropriately* into the analysis.

. Lines 663-665 "there are dangers in applying Bayesian statistical theory, particularly in using a simple error model and associated likelihood function to represent epistemic uncertainties". It's the same thing again: the problem is the "simple", not the "Bayesian statistical theory". You might not be willing to expend the effort to build a sufficiently realistic representation to overcome the problem: but the danger then is a consequence of your decision, not of Bayesian methodology per se.

Other, minor comments are as follows:

- Lines 126-127 "changing in water level to discharge rating curves after major events": something wrong here (or at least the syntax / punctuation is such as to obscure the

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intended meaning).

- Lines 260-261 "this method will generally overestimate the information content of the historical data": can you clarify what you mean by this?

- Lines 278-279: see general comment above about expert elicitation. The current statement here needs to be tempered accordingly. That said, the following sentence about precautionary or robust decision-making is certainly sensible.

- Lines 297-298 "Western societies increasingly seek to place blame ...": is this true? The only controversial issue of this nature that I can think of is L'Aquila (e.g. in the case of Hurricane Katrina, it sees to me that it was entirely appropriate to question to role of the relevant authorities) - and L'Aquila is the only example that is given. Perhaps you mean: "Following the legal case resulting from the L'Aquila earthquake, there is increased concern among the scientific community about being held responsible for natural disasters if scientific advice is subsequently deemed to have been inappropriate [INSERT PLENTY OF REFERENCES]".

- Line 397: I'm not sure that "Paper 2" has been mentioned anywhere previously. In any case, for avoidance of ambiguity (I initially read this as "Graham's second paper") it would be better to write something like "(e.g. Graham, 2000; see also the accompanying paper in this volume)". Similarly line 832.

- Lines 452-454 "The flood defence example is one where the analysis can be extended to a full risk-based decision analysis, where costs and benefits can be integrated over the expected frequency distribution of events". I agree with this: the hydrologists are some way ahead of the game because this is a discipline where there is a decades-old culture of thinking stochastically. It's the issue of having the right skill set again: it is quite possible that when other disciplines have acquired the same skill set, their own risk assessments can be transformed in a way that is currently hard to imagine.

- Line 457 "annual exceedance probability": whatever that means in a nonstationary

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climate!

- Lines 479-480 "the consequences of failure might be high impact": probably you mean either "the consequences of failure might be serious" or "failure might have a high impact".

- Footnote 4: it seems to me that if a reader doesn't know what a fat tail is, they're unlikely to have encountered the term "kurtosis" before!

- Lines 517-518: "given that we lack the ability" => "if we are unable or unwilling", I think.

- Lines 588-592: this material about the stability of bias corrections is again a criticism of naive approaches. There is nothing (except lack of awareness and / or time) to prevent us from modelling the potential for the bias to change with the dynamics - indeed, some of us do this routinely. Obviously one can never escape from a fundamental assumption that the model structure continues to hold in the future, but if the stationarity assumptions are embedded much more deeply within the model structure - at the level of *physical* rather than empirical relationships, for example - then we gain increased confidence.

- Lines 623-624: I agree with this statement about a possible use of Bayesian updating - it is useful to make this point I think.

- Line 654: "assumption" => "assumptions"

- Footnote 6 is not necessary, and is needlessly obfuscatory. Nobody needs to know about Borel spaces, they are defined merely so that mathematicians can sleep relatively undisturbed.

- Line 699 "conditionality of the outputs": what does this mean?

- Lines 715-718: I would strongly advocate adding http://www.stats.gla.ac.uk/∼adrian/papers/graphics-for-uncertainty-paper.pdf, and

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references therein, to this list. The paper is under review, I believe.

- Lines 726-729: this material about the resolution of visualisations is in fact merely a modern manifestation of an issue that has been known about for decades: how many decimal places to use in tables of results, what contour spacing to use on a map etc.

- Lines 819-823: this issue of whether the next event will be informative or disinformative is easily handled in principle, simply via an appropriate representation of the potential data structure (e.g. by embedding the "information class" of an event as a latent variable). Another example where problems go away if you have better awareness of the possibilities.

The bottom line is that, unfortunately, this paper needs a *very* large amount of work to deliver what it promises, at least in the context of a special issue that claims to showcase the state of the art. Some of the less balanced discussions can perhaps be fixed reasonably straightforwardly; but it will take considerably more work to deal with the lack of appropriate citations to the wider literature (noted by Reviewer 2 in the context of the climate literature; I am guessing that the comment applies at least to some other hazard areas as well). It is hard to know how best to deal with this. Perhaps the path of least resistance would be to reframe the paper (and, maybe, its companion) as more of an opinion piece than a review, and to make absolutely clear that it represents the collective views of the authors and does not attempt to be comprehensive. If you do this then at least you won't be promising something that you can't deliver in the time available. Nonetheless, even an opinion piece should be balanced, scholarly and even-handed; and the limitations should be clearly acknowledged.

I hope you will appreciate that these comments, although robust in places, are offered in a constructive spirit.

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