Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2015-295-EC1, 2016

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Epistemic uncertainties and natural hazard risk assessment – Part 2: Different natural hazard areas" *by* K. J. Beven et al.

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Received and published: 13 March 2016

I was a little surprised to see that the Copernicus editorial system has automatically closed the discussion on this paper, because there are several other reviewers who haven't submitted their reports. I wanted a range of views from different hazard specialists, but only a couple of the areas are represented. Nonetheless, the comments received so far on this and on the companion paper are fairly consistent and in accordance with my own assessment, so it probably does no harm to return this one to the authors for a *very substantial* revision at this point. I'm afraid it needs a lot of work, if it is to fulfil its promise of a wide-ranging review in a special issue devoted to state-of-the-art treatment of uncertainty. Similar comments will come back on the companion paper in due course; I am still waiting for a final referee report on that one however, and I want to see that report before finalising my own comments.

The present paper sets out to review the ways in which generic issues of epistemic uncertainty, set out in the companion paper, have been addressed in different natural hazard areas. It inevitably inherits the perspective of the companion paper, so some of my major concerns about that paper carry through to this one as well. One of the most serious concerns is that both papers need to provide a much more balanced perspective: at present several techniques are criticised inappropriately, in a way that confuses the underlying concept with the way in which the techniques are often applied by non-specialists. I will say more about this in my report on the companion paper itself. However, for the present paper as well there are a few places where assertions need to be reframed to present a more balanced case.

An obvious general comment about the present paper is that it is very long. There is inevitably some overlap with the companion paper, but there are also a couple of places where essentially the same material is repeated and it would be good to identify opportunities to reduce the length.

A final general comment is that the treatment of the different hazard areas is rather uneven: for some areas the paper provides a reasonably comprehensive review of the landscape, but for others I have the sense that it does not so much provide a comprehensive review of the relevant issues as promote the relevant authors' own work. This is not really appropriate. The material on pyroclastic flows stands out particularly in this respect (although it is not the only offender): this reads less as a a comprehensive review of uncertainty-relevant issues than as a list of gaps in our understanding of Vesuvius.

Specific comments are as follows:

- Lines 25–28 "In each case it is common practice . . . underestimation of the resulting uncertainties": this perspective is inherited from the companion paper. As will be indicated in my comments on that paper, it is not universally accepted and the problem is not so much with the concept per se as the fact that it is often implemented by those without the skill set to do it properly. This needs to be reframed.

- Line 92: "can estimate" ⇒ "an estimate"?
- Line 93 "[classical flood frequency analysis] is not easily modified to allow for future change": I'm not sure that I agree with this, there are plenty of opportunities to incorporate nonstationarity into models of the underlying distributions — and, indeed, this is often done. I'm not sure that we should be promoting myths that originate with authors who are simply unaware of what is possible.
- Line 102 "Poisson distribution of occurrences": the GEV is a distribution of maxima, it has nothing to do with a Poisson distribution of occurrences. The Poissonness or otherwise of event occurrences therefore is irrelevant to whether or not the GEV is appropriate for modelling maxima (lines 107-108). The confusion here is with peaks-over-threshold approaches using the Generalised Pareto Distribution. This material would benefit from careful scrutiny by someone who is familiar with the relevant technical details of extreme value theory (there is at least one person on the author list who should have spotted this, which makes me wonder whether all of the authors have seen the manuscript?).
- Line 111: "less" ⇒ "fewer".
- Line 168 "epistemic uncertainty will remain a constraint on accuracy". This is true, but it would be helpful to articulate the implication which is that the best you can do is to ensure that your epistemic uncertainty is quantified and communicated clearly, so that users know at least the order of magnitude they can expect for discrepancies between what is predicted and what might occur.
- Line 203 "chance of flooding": does this mean "probability of flooding"? If so, change the legend: "uncertainty" is too imprecise a term. If it doesn't mean

"probability" (for example because it is simply a proportion of simulations) then this needs to be explained clearly, and there needs to be an acknowledgement that a proportion of simulations does not necessarily provide a decision-relevant probability that could be used in a formal risk assessment framework. This point has been emphasised repeatedly by more than one person, at every PURE consortium meeting where this kind of work has been presented.

- Line 282: "116 of which" ⇒ "116 of whom".
- Line 500: delete "of" at end of line.
- Lines 527-530 "A particular feature of fitting such a stochastic model is that whether a model appears to give a good fit to the observed statistics might depend on the particular realisation ...": this seems to be the same point as lines 149-152, so there is an opportunity to lose a few lines. More generally, it is naive: the point is to assess whether the observed statistics (which are samples) could have been generated from the distributions implied by the simulations. There are people who have thought about this kind of thing rather carefully: their work should be represented.
- Line 557: delete "an" in "an upstream boundary conditions" (or make "conditions" singular)
- Line 846 "It has become established . . .": the jury is still out, isn't it? That is: if one considers aftershocks then recurrence is obviously non-Poisson, but the "quasi-cyclical" claim is not universally accepted I think. I note that the seismologist reviewer has serious concerns about the earthquake material in the paper and, in fact, recommends that the paper should be rejected. I don't particularly want to go that far, but the individual concerned knows what they are talking about and is not the kind of person to offer unfair criticism: the points made in his / her report require serious attention.

- Lines 1035-1036: the reported cost of the Eyja event seems very variable, depending on where you look! Wikipedia thinks that the cost to the *airline* industry was about 1.2 billion dollars in total, which seems inconsistent with the assertion that the total cost was a billion euros per day.
- Figure 3, caption "A negative BTD signal . . .": this is not clear to me, apart from anything else because there is no legend to tell us what the colour scale means.
- Line 1328: why *European* windstorms? Are issues of uncertainty different in Europe from elsewhere?!
- Line 1332: can you give a reference for the $200 billion figure? Is this in Europe or globally?
- Line 1368: why is "models" bold?
- Line 1393: this "wrap-up" section repeats material from the companion paper (e.g. in lines 1396-1400) so there is some scope for pruning back here to reduce the length.
- Line 1422: "exceed"⇒ "exceeded".
- Lines 1545-1546 "We hope that in making this comparison it will be researchers in different areas": something wrong here.

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