

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

Anonymous Referee #1

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Manuscript # 2015_295 entitled ‘Epistemic uncertainties and natural hazard risk assessment. 2. Different natural hazard areas’.

This aim of this paper is to characterise the epistemic uncertainties associated with forecasting different natural hazards. The different analysis methods in each field are compared and similarities and differences highlighted.

As suggested by the editor I have focused specifically on the section dealing with volcanic eruptions and ash clouds (section 8). Overall, this section contains a useful description of the current uncertainty analysis being undertaken in the field of volcanic ash dispersion. However, the structure of the section is confusing in parts and several sources of epistemic uncertainty are missing from the description. Finally, explicit links to the uncertainty analysis types described in the paper’s conclusions would help to

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link this section more closely to the overall aim of the paper. This is an ambitious and important paper that is worthy of publication.

Comments 1. Page 34, lines 6-11: The authors are rather dismissive about ‘other types of observations’. Whilst there are obvious limitations associated with these low temporal or spatial resolution observations, they are often invaluable in measuring some of the physical properties of ash and ash cloud geometry that satellite retrievals currently need to make assumptions about in order to make concentration estimates. For example, ash layer depth, ash cloud height and particle size distribution. It might be better to acknowledge that combining these observations with the satellite observations can help to reduce the epistemic uncertainty?

2. Page 33, line 18: I think ‘... in order to make estimates on other physical properties such as ash column loading ...’ should be ‘...in order to make estimates of other physical ...’

3. Page 34, lines 3-5: The authors refer to ‘simulations of satellite imagery using a radiative transfer model’. This is a bit ambiguous. I assume they are referring to volcanic ash satellite retrieval schemes, but another interpretation could refer to simulations of satellite imagery using VATD output, such as performed by Millington et al. (2015). This should be stated more clearly. More generally, ‘the model’ is also ambiguous as there are several models in this section. Millington, S. C., R. W. Saunders, P. N. Francis, and H. N. Webster (2012), Simulated volcanic ash imagery: A method to compare NAME ash concentration forecasts with SEVIRI imagery for the Eyjafjallajökull eruption in 2010, *J. Geophys. Res.*, 117, D00U17, doi:10.1029/2011JD016770.

4. Page 34, line 13: ‘modelling of the hazard is, however, a problem of forecasting’. Since modelling in the previous 3 paragraphs was referring to volcanic ash satellite radiative transfer models, I was initially confused by this sentence. As the next few paragraphs go on to discuss VATD models, however, I assume modelling here refers to VATD models. It would be a good idea to clarify that from that point onwards the

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authors are discussing dispersion models.

5. Page 34: When discussing parametric uncertainty, the authors focus almost exclusively on of the eruption source parameters (PSD, ash density, MER). I was surprised that uncertainty associated with the input meteorological fields was not discussed. Was this left out deliberately? If so, why?

6. Page 35, lines 5-18: The structural uncertainties associated with missing near-source processes (bent plume, ash aggregation, effect of gravity currents) are dispersed through p34 and p35. Describing these missing processes in a separate paragraph might help with the clarity of the section.

7. Page 35, line 20: The authors refer to the 'dispersion parameters' but there is no description of what these are, or any of the other parameter uncertainties associated with processes that are represented in VATD models, such as wet and dry deposition, sedimentation, turbulence etc. Why is there no discussion of parameter uncertainties?

8. Page 35: In the conclusions section the authors nicely split uncertainty analysis into 3 types. The parametric sensitivity studies described on page 35 all fall under the forward analysis approach. It might make it easier for the reader to link to the conclusions section if the authors could refer to the analysis type here.

9. Page 36, lines 9-14. Similarly, how does the method of Denlinger et al. (2012) fit into the 3 types of uncertainty analysis framework? Is this a source inversion analysis?

10. Page 37, line 1-2. Given the operational time constraints, do the authors think that statistical emulators have a role to play in characterising volcanic ash forecasting uncertainties?

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