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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Multi-level emulation of a volcanic ash transport and dispersion model to quantify sensitivity to uncertain parameters" *by* Natalie J. Harvey et al.

Anonymous Referee #1

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The subject manuscript describes an inversion of ash cloud transport in order to make a forecast. The manuscript is poorly written and organized, and is hard to read critically. It has one glaring weakness in that Bayesian linear regression is used on a high dimension problem formulated by the authors. Yet the emulator operates in a way to just constrain those parts of the model that are constrained by the data. This is a desirable property, but carries with it a liability. With this multi-level approach, there is the possibility (very likely) that there is a highly nonlinear dependence on the network function of the parameter values, in which case an exact Bayesian treatment is no longer applicable. The posteriors may be multi-model, and possibly non-convex. None of this is addressed, and it is also very difficult to follow exactly how the model operated on the

May 14, 2010 ash clouds from Eyjafjallajokull volcano to provide the results in Table 4. My recommendation is that the manuscript be completely reorganized and rewritten, being more explicit in some areas but also putting some explanations in Appendices or deferring to previously published work. It is very difficult to assess the efficacy and veracity of this study. Yet this is an important problem that deserves better treatment than is presented here. This could have been a shorter and much more informative paper than it is in its present state.

Specific comments: 1) Use references with descriptions of how NAME works to shorten section 2. 2) page 5, line 24: particle density and particle size distribution are not known at the time of an eruption, so a forecast model must rely on a priori information. 3) page 13, line 17: only fine particles are considered, so why have particle size distribution as a variable, increasing the dimensionality, in this model (described in a later section). 4) page 14, line 20: 10,000 per hour, 1000 per hour are the rates of particle release, not the number of particles. 5) page 16, lines 15-20: these are properties. So remove the word 'must' and replace 'should predict' with 'requires'. 6) page 16: write out the function g, here equal to 'x' to the ith power. 7) page 16: define both expectation and correlation rather than just putting in E() and Corr() and expecting the reader to just know what this means. 8) page 17: same comment as above for Var() showing up suddenly in the text. 9) page 17: line 25: 'some of these problems are summarized'. Nice lead in, except that none of these problems are summarized later on. I suspect they got abandoned in an earlier draft. 10) 'expert judgements' throughout are the a priori in your Bayesian analysis. 11) page 18: The paragraph/sentence starting with "Such an approach" is a non-sequitor with the paragraph and sentence immediately above. What approach? 12) page 18, 1st paragraph: forecasting uses the posterior, which has the prior in it. Change 'For calibration and forecasting' to 'For calibration' 13) page 18: The reason that doing high dimension problems with a linear Bayesian analysis is very difficult is that linear regression is not well suited to high dimension problems. You don't have enough flexibility with fixed basis functions. This could have been alleviated in this paper by just considering two parameters for distal clouds; height and mass

flux at that height. 14) page 20, first paragraph: so, a prior was not used here? 15) page 22: give references for R squared. 16) page 24, line 24: change "..., whereas of course" to "..., even though". 17) page 24, bottom: so, the number of parameters could not be reduced below 4? What if you did not do this? Would you just end up with height and mass flux? I suspect so. 18) page 34, Cov equations at top: there are 2 parameter sets here. Need either references or a proof of the second equation.

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