Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-344-RC1, 2018

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Statistical theory of probabilistic hazard maps: a probability distribution for the hazard boundary location" *by* David M. Hyman et al.

Anonymous Referee #1

Received and published: 16 December 2018

The paper introduces a formal probabilistic framework which may be used to provide additional scientific hazard information. I am very positive about these attempts to make the hazard calculation formally robust, and so I am also positive about a publication on NHESS. However, I think that the paper requires some modifications and/or clarifications that should be addressed in a revised version. My main suggestions follow (not in order of importance).

- I think that the authors should make an effort to simplify their terminology. As I said in my review to a previous paper of the same group of scientists, such a simplification could facilitate the reading of this paper to a wider audience of volcanologists. At the

same time, I think that a simplification in terminology can be made without loosing the scientific and mathematical rigor.

- At page 2 the authors mention that using categorical boundary in the hazard is very common. This statement should be articulated better. It is true what said by the authors and I would add that the use of categorical boundaries may facilitate the comprehension of the hazard map to laymen. However, it has been also highlighted by several authors the risk of this scientific discretization, in particular when the categories are associated to some decision making. If the discretization is made to facilitate the decision making, it cannot be made only through pure scientific arguments.

- At page 2 the authors claim "An important deficiency in the analysis of the PHM is that previously, estimates of the likely hazard boundary (a single curve on the map) have not been computed by consistent methods." In my opinion this statement is too strong, and should be justified. Personally, I know several papers which may a "consistent" hazard mapping. In my understanding, the method provided here can increase the scientific information related to the hazard mapping, but it does not demonstrate that 'all' previous PHM efforts are not based on consistent methods.

- The authors use the word "ensemble" as a collection of outcomes of one model with different initial and/or boundary conditions. Correctly at page 3 the authors say that their framework precludes the possibility to handle different models. They justify this choice claiming that the use of one model and an appropriate subspace of the general parameters are enough to explore the full variability. I think that this statement is too optimistic and should be modified. As a matter of fact, in many natural hazards different models are commonly used to estimate the so-called epistemic uncertainty. Even when the physics is very well known such as, for example, in predicting the space-time evolution of hurricanes, different models provide different paths (the physics is so complicated that some models focus on describing better some aspects instead of others). For this reason, the term "ensemble" has been generalized since the first applications to include also the variability among different models. I think that this

points has to be discussed in the manuscript.

- At page 3 the authors say that the main goal of their work is to build statistically meaningful boundary of the area impacted by a flow. In the paper they claim that this procedure can be applied to the full hazard or to conditional hazards (e.g., scenarios). I think that this method is much more meaningful for a conditional hazard than for the full hazard. In fact, the full hazard is usually the product of a combination of the outcomes of different scenarios. So, the probability represented by the contour lines often do not represent any specific scenario. In this case, it is not clear what is the meaning of calculating the boundary of the area impacted by a flow, because, for example, different realistic scenarios can be either smaller or larger than the average value (which may be not related to any possible scenario). Conversely, when this procedure is applied to a specific conditional hazard, i.e., to the hazard provided by one specific scenario, calculating the boundary of the area impacted by a flow makes much more physical sense.

- In essence, in most practical cases the method formally estimates a PDF from an empirical cumulative function (the PHI parameter in the manuscript) which may be estimated by counting the frequency of simulations for which one specific site is hit by a flow. I think it may be interesting to compare the method with a simple and straightforward numerical derivation of the cumulative. I am aware that most of the times the numerical derivative is quite noisy, but I am curious to see if it is the same for this case.

- At page 18, the authors write "For probabilistic hazard assessments to be used in sophisticated applications including risk assessments by governments or actuarial assessment for insurance purposes, the full statistics of the PHM must be considered.". I think that this statement is too dogmatic. It is not the role of scientists to say what is important for the risk, but just to provide a wide range of possible outcomes of the hazard analysis. This is exactly what this paper does, but I would not say that the decision makers "must" use this kind of information instead of others.

- In the conclusions the authors write "hazard edge location and the uncertainty in that estimate.". This statement is confusing to me. Are they talking about the aleatory variability or about the epistemic uncertainty? These two interpretations have a quite different physical interpretation.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-344, 2018.