

Interactive comment on “Probabilistic forecasting of plausible debris flows from Nevado de Colima (México) using data from the Atenquique debris flow, 1955” by Andrea Bevilacqua et al.

Anonymous Referee #1

Received and published: 14 December 2018

The paper introduces an innovative procedure to model a debris flow through a hierarchical inversion method, with an application to the Nevado de Colima (Mexico) volcano. The paper is well written and this innovative procedure has the potential to improve our capability to model some hazards related to the volcanic activity. In my opinion this paper is certainly suitable for publication on NHESS. However, I suggest authors to address the following issues to make the paper stronger, clearer, and readable for a wider audience.

First, the authors report some statements about "falsification principle" that appears inappropriate. Popper introduces the falsification principle (which is at the core of the

C1

scientific enterprise) considering the comparison of a model with "independent" data. This is not done here, because the comparison is made with the data that have been used to build the model. At best, this procedure can be defined as a consistency test. It is true that one model can be falsified also through such a kind of retrospective testing, but this is certainly not the golden rule to falsify a model, which, again, requires independent data. For instance, a strongly overfitting model will never be falsified using past data, but it will be certainly with future independent data. I suggest removing these statements or clarify their meaning in this paper.

Second, it is not clear to me how a model that has been calibrated inverting the data of one specific event can be used to provide a comprehensive hazard. At a first glance, this model can describe well "that" specific event; of course the model will have uncertainties that are purely epistemic (due to the fact that I do not have the real parameters and model to describe "that" event). However, this uncertainty cannot be used as an aleatory variability to describe what may happen in the future. At best in this way the authors may describe the hazard conditional to the occurrence of debris flows that have a similar source of the 1955 event. Put another way, uncertainty on the fit of one event cannot describe the variability of any kind of future event. If I misunderstood what the authors mean here, I suggest clarifying if the authors are describing "that" flow, a "similar" flow, or "any possible future" flow (as expected by a hazard model).

Third, I like very much the hierarchical structure of the inversion technique that is summarized by Figure 1. Nonetheless, the authors use a terminology which is unfamiliar to most volcanologists. To make the paper readable by a wider audience I suggest simplifying the terminology. I think that using a simpler terminology does not preclude the necessity to maintain the mathematical rigor.

Finally, as regards technical issues, the authors should double check if the symbols introduced in the paper are described in the paper when they appear. For instance, the symbol F_0 introduced in the point 1 of the introduction has been described only after few paragraphs.

C2

