

## ***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 #2**

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The manuscript presents a process to compare model outputs with real world data that focuses on understanding the relationships the model inputs have on the model outputs with respect to real world observations from Nevado de Colima volcano. The paper is well written and logically structured however it is extremely long and could be shortened. While there are no obvious grammatical issues to rectify the paper is at times difficult to read due to the reliance on what seems like a very complicated set of statistical notations. In general I think the paper should be accepted and would make an excellent contribution to the topic area. The paper would attract significant interest from a wider range of researchers struggling with the application of essentially presenting and validating predictive geophysical simulations. I only have a number

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of comments or questions around the content of the paper that need to be clarified. While the approach, methods and research is very robust and accurate I think my first concern is that there is a reliance on applying models on models of models etc. with respect to analyzing the outputs of the three rheological models. It seems as though there is a missing discussion or justification of why those rheological models have those input parameters to start with. The models inputs are not arbitrarily chosen, they are incorporated or derived to represent a particular natural phenomena observed either in experimental or natural flows. While it is important to recognize the influence these input parameters have on a model outputs it seems that the original physical-numerical models that the rheological simulation was originally designed for has been over looked or ignored in favor for what I believe is a sensitivity analysis of inputs. I would also expect that more attention is paid to the rheology of the actual of Nevado de Colima (1955) flow and is better discussed with respect to the rheological model being applied or essentially outputs tested against. This brings me to my second concern is that the sites where outputs are being compared to in the real world seem static approximations of various flow characteristics whereas the comparisons from the model outputs seem to be time vary. An example is that velocity of the real world flow seems to be estimated as a single value (or a range due to the uncertainty on the outcome of the Pierson (1985) measure) yet compared to a range of velocities from the rheological model output generated over time as the simulation, simulates the flow moving past that point. Wouldn't it be better to compare the same time varying velocities between both the real flow and the simulations at the same points in time? Is it not better to look at the whole "hydro-graph" of velocities? We do know that the velocities of these types of flows do vary and pulse considerably throughout its progression. I do wonder what effect this comparisons would have on the overall technique being presented. A minor issue is that while the Pierson (1985) method is a standard method to calculate velocity from inundation it is not the most reliable measure to determine the velocity of a granular flow. Overall the paper was very interesting and makes an important point that we should move away from applying these flow simulations predictively by only calibrating

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them based on passed events but present the outputs and their internal variability and uncertainties as a set of plausible outcomes. This paper should be published and will make a good contribution to the field of hazard simulations.

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