

Interactive comment on “Mapping the susceptibility of syn-eruptive rain-triggered lahars at Vulcano island (Italy) combining field characterization and numerical modelling” by Valérie Baumann et al.

Anonymous Referee #1

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General comments

In this study, the authors investigate lahar initiation at La Fossa Cone, Vulcano. Field studies are used to estimate permeability and soil-suction of 'primary' (i.e. tephra fall) deposits, with samples of both Tephra fall and lahar deposits collected for laboratory analysis. Both the field and laboratory data were used to constrain properties for an analysis of cascading hazard (tephra fall -> lahar) using two models (of tephra fall and slope stability).

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Unstable areas at La Fossa are identified using two scenarios, a vulcanian eruption scenario and subplinian scenario. The volume of potentially unstable tephra deposits in two catchments are identified through a shallow landslide slope stability model, and clarify critical tephra fall depths that may result in lahar initiation, for a limited range of rainfall scenarios.

The scientific methods used in this study are well established in previous work and are appropriately detailed and applied here, with some issues expanded on in 'Specific comments' section. However, while the manuscript presents a wealth of data and results, the scientific significance of this work is unclear. Interpretation of the results is limited to surface-level comparisons with other examples of lahar generation. This leads to conclusions that simply repeat results and don't appear substantial to the reader. The final conclusion ("...a comprehensive assessment of unstable volumes that could potentially trigger lahars ... requires dedicated numerical simulations combined with detailed field observations and geotechnical analysis"), is relevant and valuable, but is not supported by the manuscript's presentation, text or discussion.

To be acceptable for publication, I suggest the manuscript needs to be restructured in a way that (a) clearly identifies the importance and objectives of this study, (b) concisely presents data relevant to support the study, and (c) critically highlights results of importance and explains their significance to the reader. Some more detailed comments to be considered in the restructuring is highlighted in the following "specific comments" section.

Specific comments

Page 2, line 18: It is important to clarify that overland erosion and shallow landsliding is the most common mechanism (see e.g. Pierson and Major 2014), as opposed to the only two mechanisms for generating lahars.

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Page 3, line 19-20: "Nonetheless, the use of physically-based models ... is necessary ...". I think this is intended to be the crucial aim/thrust of the manuscript, but is not justified from the text before it. What are the weaknesses in an approach such as Tierz et al.? Why is coupling a physical model preferable/necessary? Aspects of Volentik et al. (2009) and Galderisi et al. (2013) might help explain the benefits of such an approach.

Page 3, line 33 and on: "This approach provides the first integrated attempt ..." to page 4, line 6 "offers an innovative treatment of cascading effects". Both Galderisi et al. (2013) and Volentik et al. (2009) used a similar approach that integrated tephra fall models with a slope stability/shallow landslide model, so I don't necessarily agree with the statements in this paragraph. In combination with the previous comment, I think coupled approaches need to be discussed in more detail to highlight the significance of this work (in my opinion: better considers impact of rainfall on stability, identifies thresholds for initiation and provides fully quantitative estimates of lahar initiation).

Field and laboratory methods and results The field sampling is unclear to me in section 3.1. What is meant by collecting 8 samples "...to retain primary physical characteristics"? Is this where in-situ permeability/soil suction tests were done? On page 5, line 2 - A further 11 samples are collected, looking at Fig. 3 I presume from the Pal D deposit (1 sample), and then at two locations for the 1880-90 vulcanian eruption. This corresponds to samples V1, V2 and V3. However, line 3-7 then discuss location of samples (4 from S La Fossa, 2 from NW La Fossa, 2 from Palizzi valley), but that only sums to 8 samples.

Aided by confusion in sampling, the first two results sections (4.1, 4.2) are difficult to follow, presenting a large amount of information not directly relevant to the manuscript. The purpose of field and laboratory characterization is to identify parameters of Pal D and 1880-90 deposits for TRIGRS. While grain size is a consideration for lahar generation and tephra fall, the authors do not use any of this information in their study. Tephra simulations use Biass et al. 2016 results, and no comparison between the chosen size

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range in Biass et al. 2016 and the values found in this study. The sample naming scheme is hard to follow in context, switching from unit to location to site to unit in the tables, and unit to site to sample number in the figures.

I suggest simplifying both the description of sampling and presentation of results from field and laboratory analyses. In this study, we are interested in the Pal-D and 1880-90 tephra and lahar units. Individual data from each location (Tables 1-3) can be provided as supplementary material, and results should focus on how Geotechnical (table 4) and input parameters (table 5) are derived from your sampling campaign. I fail to see how the extensive study on grain size is necessary here, beyond a few sentences, and would recommend shifting figures 5 and 6 to supplementary material.

Page 12, line 33: Were two upper catchments exactly the same size, or were they of similar size? How were catchment boundaries defined (i.e. from the slope or drainage networks/external data)?

Figure 9: It is better to make this figure greyscale compatible and easier to interpret. I would suggest something like using dashed lines for 25

Section 4.3.1 - This section seems to show that increasing the tephra thickness above a certain threshold will increase stability, nicely leading onto section 4.3.2. Figure 10 doesn't seem to add much to this discussion over table 7, so I would recommend removing it.

Page 14, line 2-4: It is unclear how deposit thickness was increased. Was this assuming a constant depth of deposit across the entire NW and S area, or was it applied as a proportion of the isopachs (either Tephra2 or observed)?

Page 15, line 1 and on: "... total pressure head has a higher maximum displaced to higher tephra fallout deposit thickness..." What does maximum displaced mean here?

Page 15, line 14 - page 16, line 2 : I do not understand the relevance to Cordon Cauille in the entire section, and it seems misguided. The friction angle of a granular material

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is controlled by the distribution (σ) of grainsize, asperity and roughness; not mean grain size. Hydraulic conductivity for these two different eruptions would be expected to differ, as the Pal D deposits are much coarser than Cordon Caulle lapilli. Some 'washing' of fines over time may occur, but if differences in measuring techniques cause a 2 orders of magnitude difference in conductivity, then the techniques are unreliable. This section is better served by starting with Page 16, line 3 (Table 8 ...).

Another consideration in section 5.1 is the volume of lahars. Lahar volumes for all the other examples in table 8 are quite large, in comparison to the smaller lahars at Vulcano.

Page 20, lines 6 - 8: How was the assessment of unstable areas found to be accurate? Without validation against a specific event (or set of events), a *methodology* has been shown to identify unstable areas.

Technical corrections

Note, given the need for extensive changes, I have not comprehensively identified minor technical corrections (some in the attached PDF), I would recommend a detailed proof read to identify further grammatical and typographic issues.

Page 2, line 13: A reference is probably necessary here.

Page 10, line 23 - 24: For such a small lahar, the errors in measurement with a hand-held GPS and thickness estimates would be significant. I suggest removing the sentence "The area of the front lobe ..." and simply explain two samples were taken from a recent lahar.

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References

Volentik, A., Connor, C. B., Connor, L., Bonadonna, C. (2009). Aspects of volcanic hazard assessment for the Bataan nuclear power plant, Luzon Peninsula, Philippines. Volcanic and tectonic hazard assessment for nuclear facilities. Cambridge University Press, Cambridge, 229-256

Galderisi, A., Bonadonna, C., Delmonaco, G., Ferrara, F., Menoni, S., Ceudech, A., . . . Gregg, C. (2013). Vulnerability Assessment and Risk Mitigation: The Case of Vulcano Island, Italy. In C. Margottini, P. Canuti, K. Sassa (Eds.), Landslide Science and Practice (pp. 55-64): Springer Berlin Heidelberg.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-77/nhess-2019-77-RC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2019-77>, 2019.

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