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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.

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We thank Reviewer 1 for valuable and constructive comments. All specific and technical comments (including those indicated on the provided PDF) have been addressed (see detailed description below):

1) 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 im-

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portance 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

The paper has been substantially restructured and rewritten based on comments of both reviewers in the attempt to clearly identifies the importance and objectives of this study, concisely presents data relevant to support the study, and critically highlights results of importance and explains their significance to the reader.

2) 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

We thank the reviewer for this comment, however, in our statement in the introduction, we already say that “sheet and rill erosion” and “infiltration of slope surface by rainfall” are the main two mechanisms that trigger lahars in association with heavy rain. As a result, we think that the statement is sufficiently inclusive of all possible mechanisms.

3) 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.

We agree with the reviewer that this part was not very clear, so we have expanded it and added some reference to Volentik et al. 2009 and Galderisi et al. 2013 (introduction).

4) 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).

We agree that our work is not the first attempt to couple lahar triggering with probabilistic modelling of tephra sedimentation (see previous comment). However, our work certainly represents the first "integrated attempt to quantify the source volume of lahars as a function of probabilistic hazard assessment for tephra fallout (with TEPHRA2), numerical modelling of lahar triggering (with TRIGRS), field observations (including primary tephra-fallout deposits, geology, geomorphology and precipitations), and geotechnical tests of source deposits." We have expanded the description of the Volentik et al. 2009, Galderisi et al. 2013 and Tierz et al. 2017 studies in order to clarify this point (introduction).

5) 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.

We agree that the sampling description was unclear. We have rewritten this whole part.

6) 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 grainsize 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 range in Biass et al. 2016 and the values found in this study.

It is correct that Biass et al did not use this grainsize because the input of TEPHRA2 is the total grainsize distribution and not the distribution at individual locations. As a result, the TGSD used in TEPHRA2 and the GS analysed in this paper cannot be compared. However, the grainsize of tephra-fallout deposit was analyzed to compare it with that of lahar deposits in order to relate the source area with eh remobilized material. In addition, grain size distribution of source area is required to estimate the Soil Water Retention Curve of soil using the Kovac and Aubertin model necessary for TRIGRS (see section 3.2). In fact, this model requires grainsize data including the diameter corresponding to 10% and 60% passing on the grain size curve (i.e., D10 and D60), and the liquid limit. We have, therefore, kept the Mdphi vs sorting plot in the main text and moved all tables and figures of grainsize distribution in the supplementary material.

7) 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 grainsize is necessary here, beyond a few sentences, and would recommend shifting figures 5 and 6 to supplementary material.

We reorganized the table 3 and put tables 1-3 in supplementary material. Figure 6 was moved to supplementary material and we have kept Fig 5 in the main text to illustrate the grainsize similarity between primary and lahars deposits.

8) 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)?

Yes, they are of same size and we have added an explanation on how we have defined them (section 4.3 Modelling).

9) 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

Good suggestion! We have made the figure greyscale compatible.

10) 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.

We agree with this comment. However, we also feel that Fig. 10 helps visualize results of Table 7. Given the importance of these results, we feel that this Figure should be kept.

11) 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)?

Thanks for this comment. We have explained in the text that the thickness was considered constant over the whole source area (section 4.3.2)

12) 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?

We agree that this statement was confusing. We have rewritten this all section.

13) Page 15, line 14 - page 16, line 2 : I do not understand the relevance to Cordon Caulle in the entire section, and it seems misguided. The friction angle of a granular material is controlled by the distribution (_) of grainsize, asperity and roughness; not mean grain size. Hydralic 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 ...).

We agree with these comments and this part has been removed.

14) 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.

We agree with this comment and added some lahar volumes for the volcanoes listed in the table and discussed it in section 5.1.

15) 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.

We have removed this statement and rewritten this part.

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