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## **Comment on nhess-2021-320**

Laura Sandri (Referee)

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Referee comment on "Evaluating and ranking Southeast Asia's exposure to explosive volcanic hazards" by Susanna F. Jenkins et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-320-RC2>, 2022

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Review to the manuscript "Evaluating and ranking Southeast Asia's exposure to volcanic hazards" by Susanna Jenkins et al.

The paper presents a massive effort to evaluate hazard and exposure to four different hazardous events accompanying explosive volcanic activity, i.e., ground load of tephra from fallout, large clasts' impact, invasion from dome-collapse PDCs and from column-collapse ones, from 40 different volcanic centres in Southeast Asia. The goal is to quantify and rank such exposure, in order mostly to identify objectively the volcanoes that pose the highest threat to population, roads, buildings, crops and urban areas (these are the five exposure "layers" analysed), and in particular to spot "hidden" high-threat volcanoes that may have been overlooked or underevaluated in these terms.

I call it "massive effort" as, in practice, the presented study relies in total on several hundred thousands of simulations of the four hazardous events at the 40 volcanoes, and other tens of thousands of exposure evaluations.

In my opinion the paper is well worth for publication, provided some aspects are made clearer in a revised version.

Main points are:

1) Lahars: in the present version (if I am not wrong) the reader needs to read down to the end of the "Limitations" section (Section 5) to discover why they are not considered in the present manuscript. I myself, as a reader, have been wondering why lahars were not taken into account in assessing volcanic hazard from Southeast Asian volcanoes (while, for example, large clasts' impact is) until I got to that section in the end of the paper. However, this is a strong limitation that should be put forward from the beginning, in my opinion. I acknowledge that it would be an extra effort to account for lahar hazard in a similar way as done for tephra load or large clasts' impact, and I am not asking to do this.

I also agree with what the author state in line 645, i.e. that the available empirical models are not able to simulate some of the known flow types, and more in general are not able to capture the details of lahar propagation. However, probably the same can be said for the energy cone model, that still is used in this study, and I agree on using it in a "hazard perspective", as I am one of the authors of a cited paper (Tierz et al, 2016) in which we showed that, notwithstanding its oversimplifications, the energy cone model is able to statistically reproduce the recorded PDC-invaded areas from Vesuvius and Campi flegrei eruptions.

So I would advice to add a paragraph already in the Introduction section on why only the 4 hazardous events are considered, and not lahars, for example. If possible, it would be interesting to see whether lahar hazard and exposure could be taken into account at a few volcanoes (maybe volcanoes characterized by very different lahar hazard), and see whether the introduction of lahars would significantly change their relative ranking (a kind of sensitivity analysis on the hazard events considered). I realize this is probably a large extra work, so I leave it just as a suggestion.

2) Similar considerations apply to the conclusion that Krakatau is the last in the ranking... The reason is probably that tsunamigenic explosions have not been considered in the hazard and exposure analyses, and this affects the ranking. So I think this should be mentioned.

3) Another point not clear to me is what probabilities have been used to evaluate the VEI scenarios when they are considered separately in the "short-term" evaluation. In general, I would suggest:

- throughout the manuscript, avoid using "short-term" and "long-term" to distinguish between the two cases illustrated, as they are effectively both "long-term" assessments: for the tephra fallout in particular this is misleading, as in a real short-term assessment one would never use the yearly or monthly statistics of the wind to assess tephra fallout hazard, but would use the current weather forecast for the upcoming days. My suggestion is to use "conditional to an eruption size" and "absolute" respectively for what is now called "short-term" and "long-term", because this is what they are (as also called by the authors in lines 147-148).

- rewrite section 2.3: I did not understand how the frequency magnitude relationships are calculated (a paper in prep is cited, so I could not get to know more). Also I assume that, when the "short-term" evaluations are computed, no probability of the VEI is used to weight hazard and exposure (correct?), while when all the VEIs together are considered ("long-term" or absolute case) the annual probabilities of the different VEIs are used to weight the hazard and exposure (according to the total probability theorem). However, lines 311-315 are quite confusing: from those, apparently also for the separate VEI scenarios (short-term case) a weight is given, but I cannot understand why. This should be made much clearer. In this light, also lines 603-611 could improve in clarity: in fact, it is not a matter of annual probabilities of VEI3, 4 and 5 separately that change the relative ranking of Cereme and Raung: it is the fact that an explosive eruption from Raung is much more likely (any size, it is the sum of the three annual probabilities that is much larger for Raung than Cereme, so I think that the lines 608-611 could improve in clarity if the sum of the three VEIs' probability are mentioned, rather than the three separate).

4) Where official hazard maps are available (lines 103-105), I wonder whether it would be possible to compare them with the hazard output from the present study. I think this would strenghten the approach.

5) Finally, somewhere in the Discussion it should be acknowledged that the use of fixed ESPs as representative for a spectrum of similar-scale scenarios is justified at proximal-medium distances, where it tends to produce a hazard assessment that is conservatively higher than when considering a continuous spectrum of ESPs; however we must be aware that it can significantly underestimate hazard assessment in the distal areas, where the impact of the lower-probability upper-end members (that is, within the same VEI class of the representative scenario considered with specific ESPs, but at the upper end of the VEI class and not in the middle) are predominant (e.g., Sandri et al, 2016, Nat Sci Rep).

Minor points:

- Line 38: "Of these Southeast" --> "Of these recorded Southeast"
- Line 39: "in reflecting previous eruptive activity": I am not an English native speaker, but it seems to me that this sentence is not very clear. I suggest changing it into "in reflecting our knowledge of the previous eruptive activity" or something like this.
- Table 1: shouldn't the two companion papers by Magill & Blong (2005) on Bull Volcano be added here? Or you consider them as a "studies that considered the hazard to a site, rather than from a volcano" (as mentioned in the caption)? (not sure if the caption applies to a large city as well)
- Line 80 and others: I could not find the paper by Biass et al (2014) in the reference list. Please check it all (I did not check all the references one by one)
- Line 143: "differing probabilities" --> "differing exceedance probabilities"
- Section 2.1: to me it appears super technical. I suggest moving this section to Appendix.
- Line 187: again, I cannot refer to Biass et al (2014) with certainty as it is not in the ref list, but I think here it would be fair to mention Folch et al, 2009, Comput Geosci
- Line 194: in this study, aleatoric uncertainty is addressed, but not epistemic (fixed ESPs are assumed, one simulator per hazardous event). So I think it would be correct to remove the word "epistemic" from here.
- Line 198: I think Appendix A is very important. If allowed (in terms of number of pages and floating bodies of the paper) I would suggest to include it in the main text.
- Line 214-215. The sentence "The ESPs ... across volcanoes." does not make much sense. I am not sure in what way fixed ESPs ensure convergence. In any case, results across volcanoes would be comparable even if ESPs would not be fixed but sampled from distributions, and still would be if these pdfs would be different and tuned on each volcano according to its eruptive behaviour. I suggest removing this sentence.
- Line 255: this is important: it is not clear to me how the authors link the two values for the initial flow volume to the annual rates of VEI3, 4 and 5... Do they assign them a relative likelihood to the two volumes, and this is fixed regardless the VEI class? This should be clarified, at least I did not find it in the text.
- Line 340 and 350: it could help the reader to know how these numbers (15240 hazard outputs, 11400 exposure estimates, then 498 probabilistic hazard outputs) come out. I can guess 3 VEIs x 3 hazards x 40 volcanoes + 2 volumes x 1 hazard x 40 volcanoes, for the probabilistic hazard outputs, but it does not match...
- Line 356: "Our modelling finds..." --> "Our modelling confirms..."
- Figure 4a: what are the two grey lobes (one to the west and one to the north)? The buffer? Also, I cannot find in the panel the 2010 PDCs mentioned in Lines 365-367.
- Lines 382-384: "Figures 4e and 4f show two different ways of plotting our tephra fall results: i) as the probability of exceeding a certain tephra load ( $\geq 5$  kg/m<sup>2</sup> in Figure 4e), and ii) as the tephra load expected at a given probability (50% in Figure 4f)" --> "Figures 4e and 4f show: i) the probability of exceeding a certain tephra load ( $\geq 5$  kg/m<sup>2</sup> in Figure 4e), and ii) the tephra load expected at a given probability (50% in Figure 4f)"
- Line 389: "low probability scenario" --> "low exceedance probability"
- line 403: " For all regions and all hazards, the distribution of population exposure is often

asymmetrical" --> "For all regions and all hazards, the distribution of population exposure across different volcanoes is often asymmetrical"

- Line 413: "than for tephra fall." --> "than for tephra fall, as expected."

- Line 417: New line after "volcanic islands."

- Lines 419 and 421: I think Figure 8 is referred for the first time after Figure 9.

- Captions of Figures 5, 6, 7, 8, 9 (and maybe elsewhere): these are not "conditional probability of occurrence of 50%" but "conditional exceedance probability of 50%", I think. Please change if it is so.

- Figures 6 to 9: these plots are very "dense". To improve readability, could the authors specify whether darker bars are "covered" by lighter bars? I mean, do the bars overlap, correct? They do not represent a relative proportion, right? These bar plots may be confusing, as sometimes they are used for relative frequencies, but I guess this is not the case. I think it should be made clear to improve readability.

- Line 492: I think here it would be fair to add Macedonio & Costa, 2021, NHESS

- Line 509: again, 50% exceedance probability

- Lines 508-513: Actually, in Java panel in the top part of Figure 10, Krakatau seems to be the one with the smallest (close to 0) monthly variation in the population exposure to tephra fallout (VEI 4, P=50%). I do not see the large variation in January at all at Krakatau. I see it for Guntur or Papandayan, for example. Please clarify.

- Figure 11: In panel for VEI5 and tephra fall, there are names of volcanoes that are not referred to in the caption. I guess they are the volcanoes scoring the largest value for that combination, i.e., the ones giving the largest grey circle. However, it should be mentioned in the caption, if so. And the names should be given in all panels (I guess they change from case to case).

- Lines 670-671: "With this study we have evaluated multiple categories of exposure (n=5) to a range of volcanic hazards (n=4) and VEI scenarios (n=3) to give probabilistic outputs for 40 high-threat volcanoes" --> "With this study we have evaluated five multiple categories of exposure to a range of four volcanic hazards and three VEI scenarios to give probabilistic outputs for 40 high-threat volcanoes."

- Line 673: The "long-term" or "absolute" assessment does not "accounts for the probability of occurrence of a given eruption scenario at each volcano", but for the probability of occurrence of the different eruption scenarios considered, at each volcano.

- Line 708: in the list of areas to widen the assessment, I would reiterate the extension to other relevant hazardous events, such as lahars and tsunamigenic eruptions.

Typos:

- Add a ")" after PHIVOLCS at line 105