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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Thematic vent opening probability maps and hazard assessment of small-scale pyroclastic density currents in the San Salvador Volcanic Complex (El Salvador) and Nejapa-Chiltepe Volcanic Complex (Nicaragua)" *by* Andrea Bevilacqua et al.

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General comments

The manuscript presents a collection of 'thematic' maps for the probability of vent opening, given eruption, at San Salvador (El Salvador) and Nejapa-Chiltepe (Nicaragua) volcanic complexes. By 'thematic', the authors interpret that vent-opening-probability maps are built using data points (or areas, including epistemic uncertainty) of past

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Discussion paper

vent locations, which are partitioned according to the occurrence of different hazardous phenomena during the associated eruptions. Additionally, the authors present maps of probability of invasion from pyroclastic density currents (PDCs), computed adopting the thematic description of the vent-opening probabilities, and compare them with those computed using 'non-thematic' (i.e. independent of hazardous phenomena) vent-opening models. They use the novel tree-branching energy cone model, recently developed and presented by some of the authors, to simulate PDC invasion.

I honestly think that the manuscript is a valuable contribution, as it proposes an interesting approach to explore spatial patterns in eruptive style, and could be complementary to previous and future studies tackling this complex problem, which is highly relevant to volcanic hazard assessment. Moreover, the initial probabilistic volcanic hazard assessment (PVHA), carried out at two volcanic systems with such high density of population on and around them, should represent vital information to manage volcanic risk in the area.

My main reservations concerning the content and presentation of the manuscript are the following:

1. The main methodology of the manuscript, currently on Appendices A.1 and A.2 should be moved to the main text of the manuscript, and be better explained (perhaps, step by step), including an improved visual description of the methods in Figure 2 (or another figure). In relation to the methodology, further clarification should also be provided as regards: (1) whether the ballistics thematic maps are mathematically equivalent to the non-thematic maps; (2) why the thematic maps for lava emission appear to be quite similar to the ballistics/non-thematic maps, considering that the datasets for the two phenomena seem to be relatively different (Tables 1 and 2).

2. The justification of joining the Nejapa-Miraflores fissure-vents complex with the Apoyeque caldera, under a common, underlying data-generation process in terms of vent locations (as well as eruptive phenomena) should be, at least, expanded and

made more clear, considering the significant differences in terms of geochemistry, eruption size and style at the two volcanic systems.

3. Discussion could be enriched around the differences between the approach of calculating P(vent location | hazardous phenomena), presented here; and that of calculating P(hazardous phenomena | vent location), which is more common in volcanic hazard assessments using event tree models (e.g. Tonini et al., 2014; Thompson et al., 2015; Sandri et al., 2018; Tierz et al., 2020).

4. The justification of using the tree-branching energy cone model should be also expanded, given the expected radial dispersal of PDCs related to phreatomagmatic activity, and the apparent limited channelization that the flows may be subject to (judging, very preliminarily, by the topography of the two volcanic systems). If possible, comparison of results obtained with the 'classical' (non-branching) energy cone could be an interesting addition to the manuscript.

The rest of my comments mostly relate to more specific points and suggestions, which I believe could help improve the manuscript. In summary, I would support the acceptance of this contribution to Natural Hazards and Earth System Sciences, after major revisions have been made.

Receive my best regards,

Pablo Tierz

Specific comments

L16 – Change to "Densely inhabited cities are built on them and their surroundings"?

L18-19 – Perhaps explain here that the novelty is on the 'thematic' representation of the vent-opening maps.

L26-27 – Considering that PDC channelization does not appear to be crucial at the two volcanic systems presented (judging, preliminarily, by the topography in the DEMs

shown in Figure 1), the manuscript should better justify the use of the branching-version of the energy cone, instead of the 'classical' one. If possible, performing an analysis with the classical energy cone at San Salvador Volcanic Complex, to compare the results from the two versions, would be an interesting addition to the manuscript, even if this analysis had to be included in the Supplementary Information (SI). Many thanks.

L26-28 – OK, but one could also argue that the aleatory uncertainty in PDC generation (i.e. source conditions, including vent location) could be explored using a non-thematic vent-opening map coupled with spatially-varying probability density functions (PDFs) for the energy cone model parameters. This could describe how 'external factors' (e.g. groundwater) might influence the eruptive style (e.g. phreatomagmatism) and, therefore, the types of PDCs generated (modeled via different model parameter values, cf. Tierz et al., 2016a). While I understand that this is beyond the scope of this manuscript, I think a richer discussion on these aspects should be incorporated. In the end, the authors are proposing to calculate the probability of vent opening conditional on eruptive style (i.e. P(vent | style) while most of the previous approaches in the literature (e.g. event-tree models) have devised the opposite conditional probabilities: that is, probability of eruptive style given vent location: P(style | vent). In my opinion, this aspect merits further discussion in the manuscript. Many thanks.

L30-31 – Please rephrase. Volcanic hazard assessment also includes the assessment of the probability of eruption or the probability of vent location. Many thanks.

L32-33 – Please add Newhall and Hoblitt (2002); Newhall and Pallister (2015); Sandri et al. (2014). Many thanks.

L35-36 – I would diversify the references here to include other research groups and/or volcanic hazardous phenomena. You could include, for example: Bonadonna et al. (2005); Costa et al. (2009); Del Negro et al. (2013); Mead and Magill (2017); Strehlow et al. (2017); Gallant et al. (2018); Tierz et al. (2018). Many thanks.

L40 – Similarly to the previous comment, please expand/diversify the list of references.

You could consider removing Alberico et al. (2002), as Bevilacqua et al. (2015) also analyze vent opening at Campi Flegrei. Additionally, you could include the following references: Connor et al. (2012); Thompson et al. (2015); Clarke et al. (2020). Many thanks.

L47 – Please add Clarke et al. (2020) for Aluto volcano, in the Main Ethiopian Rift. Many thanks.

L50-51 – I agree with this statement, but I think it would be important to briefly comment here, and add a proper argumentation in the Discussion section, about the significant difficulties associated with interpreting and modeling the dependence of eruptive style on vent location (see e.g. Thompson et al., 2015; Tonini et al., 2015; Sandri et al., 2018; Tierz et al., 2020). Many thanks.

L52 – Please use "hazard" instead of "risk" here. Many thanks.

L57 – "is clearly influenced by their position **(e.g. Tonini et al., 2015; Sandri et al., 2018)**"

L66 – Please see my previous comments about the 'dichotomy' between quantifying P(vent | style) vs P(style | vent) (e.g. in event trees), and add some more details in the Discussion section. Many thanks.

L70-71 – Is the sentence about the funding needed here within the body text of the manuscript? The statement is already included in the Acknowledgements section.

L88 – Maybe a brief description of how exactly the polygons were defined or calculated would be a good addition to the manuscript, even if it appears in the SI. Many thanks.

L90-91 – What is the source of this uncertainty? Poorly-preserved deposits? Buried crater structures? Other? Please briefly complement. Many thanks.

L91-94 – Whether here and/or in the caption of Figure 1c-d, I think it would be important to add some explanation about the 'bifurcations' and the arrows in the 'stratigraph-

ical/temporal trees' presented. Do the bifurcations represent events (units) identified on different stratigraphical sections? If they do: can (some of) the units be correlated across sections, beyond their stratigraphical position with respect to the regional markers? In the context of Stage III of volcanic activity at San Salvador, I think it could be beneficial to indicate which unit(s) is/are interpreted to represent the beginning of this most recent stage of activity. Many thanks.

L100-101 – Are these 'probability levels' used as the 'weighting' to count a particular vent location as belonging to the datasets used to compute the different thematic maps? Many thanks for the clarification.

L111 – "E-W"

L112 – Change to something like: "is formed by **a summit cone** called Boqueron Volcano (BV), **which is enclosed by the remnants of the** volcanic edifice of San Salvador Volcano (SSV), **which experienced a vertical collapse around 36 ka (Sofield, 1998)**. At least 25 monogenetic vents [...]"? Many thanks.

L135-137 – Are you implying that this type of activity is unlikely to occur in the future?

L150-151 – What is the recurrence period estimated for summit eruptions? Please report it, if it is available. Many thanks.

L154-155 – I think the merging of eruptive vent locations from the three stages of activity, considering the possibility of significant changes in the magmatic system after the end of each stage (as observed in changes in the eruptive dynamics and spatial location of vents), deserves an expanded justification. This choice implies the assumption of a common, underlying data-generation process for vent locations at SSVC over the last 70 ka, and it represents the basis for the statistical modeling adopted henceforth. In my opinion, it would be beneficial to present two different hypotheses/assumptions: one that considers all vent locations; another that considers only vent locations during the last stage of volcanic activity. You should clearly describe, for any future reader

of the manuscript, which implications each of the hypotheses/assumptions has (e.g. stationarity of vent locations in time). Many thanks.

L156-158 – Please clarify how these probabilities of occurrence are exactly used in the thematic vent-location datasets, to run Model 2 shown in Figure 2. As specified in my general comments, moving Appendices A.1 and A.2 to the main text of the manuscript may be necessary. Many thanks.

L173-174 – Given the substantial differences in magma geochemistry, eruption sizes and styles, and even spatial distribution of eruptive vents between the Nejapa-Miraflores fissure-vents system and the Apoyeque caldera, some extra explanation would be needed about why they are treated as having a common, underlying data-generation process in terms of vent locations. This should be carefully explained and justified in the manuscript. Many thanks.

L186-187 – Please rephrase slightly: the difference between 28 past vent locations and 31 does not seem critical. Could you also expand on which vents were not included by Connor et al. (2019) and why you included them? Many thanks.

L187 – Uncertainty in vent locations: a few extra details on how you defined the polygons that define this source of uncertainty would be appreciated. Many thanks.

L192 – It is not clear how the current presence of water relates to the eruptive phenomenology at the time of each eruption. Please expand. Many thanks.

L195-199 – Please see my previous comment on how the justification of mixing the Nejapa-Miraflores and Apoyeque volcanic systems under a common underlying data-generation process deserves substantial further explanation and justification. Many thanks.

L202-205 – Please add a few references for previous similar approaches, from which the ones that are cited built upon: e.g. Connor and Hill (1995); Martin et al. (2004); Connor and Connor (2009); Bebbington and Cronin (2011); Connor et al. (2012);

Cappello et al. (2012). I saw that many of this references are included in Appendices A.1 and A.2. This could be another reason to move these Appendices to the main text of the manuscript. Many thanks.

L206-207 – If Model 1 considers the past vent locations: are these vent locations used twice (Model 1 and 2) in generating the final vent-opening-probability maps? Please clarify. Many thanks.

L210 – Please report the spatial resolution of the vent-opening computational grid. Many thanks.

L212-214 – As a key novelty, it would be very beneficial if Figure 2 could incorporate some explanation about the new methodology you present here for the thematic maps. Many thanks.

L214 – Change “leads” to “lead”

L215-216 – Specify that the continuous PDF is bivariate (Easting-Northing)?

L216-218 – What is the area of each grid point in the computational grid? I wonder if providing the values of vent-opening probability by grid point (instead of per km^2) could be easier to interpret by the readers. Also, could you specify whether the thematic vent-opening probabilities integrate to 1, by hazardous phenomenon? That is: given, say, a lava flow (emission) to occur in one of the volcanic systems: is the (mean) probability of a vent opening, summed across the computational grid, considered equal to 1? I interpret from the Appendices that this is the case, but some clarification could be welcome around these lines. Many thanks.

L218-219 – I think this is a very important point and perhaps it deserves a little extra explanation or justification here. Many thanks.

L221 – Redundant text: “Jaquet et al.”?

L233 – How much the results could be expected to change if, say, p1 = Unif(0.6, 1)?

Many thanks.

L236 – Would it be possible that the "unknown" fault structures in the region were not distributed uniformly, for instance, due to regional tectonics? Perhaps adding a brief comment about this aspect could be beneficial. Many thanks.

L238-240 – "[. . .] or likelihood based techniques (Bevilacqua et al., 2017a; Bevilacqua et al., 2018), **and/or by carrying out further geophysical surveys in the area/region (e.g. Martin et al., 2004; Jaquet et al., 2012; Runge et al., 2016; Deng et al., 2017)**".

L244 – Could you provide some more details on how many sub-regions and/or which ones? It is also interesting to see that the two volcanic systems, despite expected differences in terms of their local structural and tectonic conditions, have the same distribution for d_1. Could you expand on this a bit? Many thanks.

L253 – But $f(x)$ is independent on fault structures, right? Please clarify. In addition, the fact that the past vent locations seem to be used in Model 1 should be made more evident. Many thanks.

L258-264 – Maybe it is better to present the non-thematic vent-opening maps first (as Figure 3) and then all the thematic maps for the two volcanic systems under study. If the ballistics thematic vent-opening map is equivalent to the non-thematic map, I would suggest that the 5*th*- and 95*th*-percentile maps are included in the current Figure 5 and Figure S1 was not included in the manuscript. Many thanks.

L266-267 – "N65W" and "N40W", right?

L270-273 – It would be convenient to explain these calculations better: why 4 km? Where exactly is the "northern portion of fault A"? Which pixels are excluded (i.e. what "closer" means in this context)? Etc. Many thanks.

L273 – "Sequel"?

L275 – "N65W"?

L279-280 – Why these two maps are quite similar if the datasets for ballistics and lava flows appear to be relatively different in the two volcanic systems (Tables 1, 2)? Is it because the fault structures (known or unknown) have a very strong influence on the final vent-opening probabilities? If that is the case, the discourse about the thematic vent-opening maps, based on past vent locations, loses a bit of strength, in my opinion.

L283-284 – At some point, one may start to wonder if the stress on the thematic maps in the manuscript should be put almost exclusively on the 'small-PDCs-conditioned' vent-opening maps, given that the ballistics thematic map is mathematically equal to the non-thematic map, and both the lava-emission and tephra-fallout thematic maps seem similar to the former. In other words, if the data-generation process for the vent locations of (small-)PDC-forming eruptions is the only one that seems to be significantly different from the non-thematic vent-location variability (the former perhaps linked to local conditions, such as groundwater availability or fluid pathways, influencing the eruptive style?): why not to focus on these aspects more clearly in the manuscript? Many thanks.

L288-289 – Figure S3 may not be needed, if it is equivalent to Figure 5b. Many thanks.

L292 – Please see my previous comment.

L295 – Is the Miraflores scoria cone identified in Figure 1b?

L303-310 – This part could be moved to the Discussion section. If the visual comparison of the results from Connor et al. (2019) and those presented in this manuscript is important, perhaps a figure could be added to the SI, where the two vent-opening probability maps are displayed. Many thanks.

L306-307 – Please rephrase. A vent-opening probability map is a PDF, even if it only captures the aleatory uncertainty (i.e. single value of probability at each grid point) in the eruptive vent location. Many thanks.

L312-320 – If I understood correctly, you extracted 100 realizations from the array of

vent-opening model parameters (p_1, p_2, d_1, d_2), which generated 100 different values of vent-opening probability at each of the vent locations (1024). Is this correct? Then, you used these sets of vent-opening probabilities to weight the hazard footprints generated from running the branching-energy-cone model (cf. Sandri et al., 2018; Clarke et al., 2020, for the 'classical' energy cone), with fixed parameters independently of vent location. Is this correct? If it is, I do not understand why the total number of simulations is: 100 x no. of vents (1024), and not just 1 x 1024, as I picture vent-opening probabilities to be independent from PDC propagation. If any of the above is not correct, please clarify here and in the main text of the manuscript as well. Many thanks.

L319 – Please state the Digital Elevation Model (DEM) resolution over which the branching energy cone was run. Many thanks.

L320 – Please express ϕ in degrees, additionally. Many thanks.

L321 – Please expand on the links between the model parameters and the volcanological observations. What are the runout distances mentioned? From how many eruptions were they calculated? Were the associated eruptions large or small? Many thanks.

L322-323 – "We decided not to use variable input conditions for initial PDC characteristics **(e.g. Tierz et al., 2016a, b; Sandri et al., 2018; Aravena et al., 2020a; Clarke et al., 2020)**"

L325 - "would require additional information to properly calibrate these input parameters (e.g. Cioni et al., 2020) **and/or complementary data coming from analogue volcanoes (e.g. Sandri et al., 2012; Tierz et al., 2016a; Clarke et al., 2020)**"

L327-328 - "(e.g., Spiller et al., 2014; Ogburn et al., 2016; Ogburn and Calder, 2017; **Tierz et al., 2016b, 2018**; Rutarindwa et al., 2019; Patra et al., 2020)."

L331-332 – What is the spatial resolution of the hazard domain? Many thanks.

L335 – Consider removing "small-scale". Many thanks.

L337 – “Ciudad” or “Sitio”? Many thanks.

L336-338 – In my view, it is not only the topographic barrier of Cerro El Picacho which is hindering PDC inundation over (the western areas of) San Salvador city. Very importantly, given that the highest thematic vent-opening probabilities (for small-scale PDCs) are located on the NW flank of the stratocone (outside the caldera rim of the SSV edifice) and beyond, onto the surrounding plain (Figure 4); PDCs generated from some of these vents would need to overcome the whole 14km-basal-width, 1.2-1.4km-height volcanic edifice! (NB. Morphology data from Grosse et al., 2014).

L339-341 – I beg to disagree with this interpretation. In my opinion, the peak in thematic vent-opening probability for small-scale PDCs located ‘between’ Santa Tecla and San Salvador (and associated with vent 16, right? Figures 1 and 4) should be a more important factor conditioning the probabilities of PDC invasion in this area. I wonder how the map displayed in Figure 8 would change if you conditioned the vent opening, spatially, to vent locations with a (mean) vent-opening probability of (say) 0.5%/km^2 or greater. How much the probability of PDC invasion would decrease around the area of Santa Tecla, in this scenario? Perhaps, including this figure in the SI could be a very valuable addition to the manuscript. Many thanks.

L339 – Consider removing “small-scale”. Many thanks.

L349 – Please change to “would be derived”. Many thanks.

L349-351 – I would rephrase the sentence as follows: “These results show clearly the relevance of using thematic vent opening maps in the assessment of hazard **at** volcanoes **where eruptive style may significantly change with vent location**”. Many thanks.

L356 – “at the **NNE flank**”?

L357 – Change “parameter” to “probability”?

L361 – Change “small” to “smaller”? Many thanks.

L364 – Please remove “Poland and Anderson (2020)” from this list and use examples previously cited instead (e.g. L44-50), and/or some of the additional references suggested below. Many thanks.

L365 – Please change “fields” by “systems”. Many thanks.

L369 – This list of references should be more generic in terms of volcanic hazardous phenomena included, and wider in terms of research groups represented. Please add references like: Del Negro et al. (2013), Thompson et al. (2015), Sandri et al. (2014, 2018). Many thanks.

L370 - “plugging-in”?

L371-372 – Please add some references to previous relevant research in the topic: e.g. Marzocchi et al. (2008); Sandri et al. (2012); Selva et al. (2014); Tonini et al. (2016). Many thanks.

L376-378 - “We remark that previous studies already produced vent opening maps devoted to specific types of eruptions (e.g. Plinian and sub-Plinian eruptions in Tadini et al., 2017b; **or pumice-cone-forming eruptions in Clarke et al., 2020)**, to eruptions inside selected sub-regions (Bevilacqua et al., 2017b) or to a suite of pre-imposed eruptive scenarios (Ang et al., 2020).”

L382 – “which has been **reported for** different volcanic systems (e.g. Andronico et al., 2005; Coppola et al., 2009; **Sigmundsson et al., 2010; Sandri et al., 2012; Tierz et al., 2020)**”

L383 – “**N65W**-trending fault B” instead?

L384 – “**N40W**-trending fault A” instead?

L385-386 – This is interesting. Could you briefly expand on the possible reasons behind this lack of small-scale PDC activity from the central crater of Boqueron? Many thanks.

L388 – I think the exact location of Managua Lake may not be indicated in Figure 1?

L389-391 – This is mostly true for small-scale PDCs but it is more difficult to justify for any of the other three phenomena presented. Please try to expand your argumentation around this point here, and maybe in other parts of the manuscript. Many thanks.

L394 – Consider changing as follows: “The design and implementation of mitigation strategies **for** volcanic risk **are profoundly conditioned by** the characteristics of the volcanic **processes** under consideration and **the** vent position.”

L409 - “sequel”?

L410-414 – If I am not wrong, you are adopting this very same assumption by using the past vent locations across different eruptive stages, for instance, at SSVC. Is this correct?

L414-416 – Please explain why did you not implemented a similar approach in the presented study. And/or excluded pre-3ka vents from your analysis. Many thanks.

L419-420 – One could also argue, then, that the spatial dataset used in the presented analysis may suffer from the same or very similar issues. How is the analysis influenced by ‘buried vents’, for instance? Many thanks.

L420-423 – I am not sure if I follow the reasoning presented here. If the stratocone has been the source of **many** eruptions, those may have produced **many** lava flows but also **many** PDC events, potentially. Moreover, monogenetic vents are interpreted to be the product of a single eruption, but that does not necessarily mean that they generated 1 lava flow or 1 PDC. In the end, all these considerations also depend on the degree of (temporal) detail on which the eruptions, and their associated hazardous phenomena, are analyzed (e.g. Jenkins et al., 2007; Wolpert et al., 2018; Bebbington Jenkins, 2019).

L426-434 – I wonder why not including the thematic vent-opening maps for small-scale PDCs in Figure 11, as the former are one of the main focuses of the manuscript. Many

thanks.

L431-434 – These sentences feel slightly disconnected and/or vague as they are now. Please improve the argumentation here. Many thanks.

L435-436 – Change “fields” to “systems”?

L436-440 – Please note that what are presented as unique features of the presented volcanic systems could be said of many other volcanoes in the world. Consider re-wording the sentence slightly. Many thanks.

L443 – Change “lavas” to “lava”

L448 – “**N65W**-trending fault B” instead?

L449 – “**N40W**-trending fault A” instead?

L455-456 - “which implies major challenges **in managing the volcanic risk** associated with NCVC” may be better. Many thanks.

L461-462 – You should introduce, and justify, more clearly early in the manuscript, why the use of the branching-energy-cone model is key in the context of the two volcanic systems analyzed, where PDC channelization does not appear to be a critical conditioning factor to PDC propagation (preliminarily judging by the DEMs of the areas). Many thanks.

L461 – Please see my previous comment on clarifying where these 10^5 simulations exactly come from. Many thanks.

L470 – “Ciudad” or “Sitio”?

L477 – “decreased”

L482 – Change “hazards” to “risk”?

L491 – Therefore, the past vent-location data are used twice in the final vent opening map explained in Figure 2: once in Model 1 and another in Model 2. Is this correct?

Could you briefly expand on the reasoning behind this choice? Many thanks.

L496 – Please remove: “the outcrop of”, and/or change it by “the surface projection of”. Many thanks.

L496-497 – How are these prior PDFs calculated? $z(x)$ in formula (3) is the prior probability of ζ (or ξ? Symbols in the text and in formula (3) seem different. Are they referring to two different variables? Many thanks) at the vent location $x = (x_i, y_i)$. Is this correct? If it is, please explain better how this is calculated. Also: is z_2 a Uniform PDF over the whole vent-opening spatial domain, right? Many thanks.

L500, 520 – Perhaps it would be useful to report the formula for the posterior PDFs: $z_j(\zeta|D_i)$. It is not clear to me that it is shown. Many thanks.

L505 – Given the key importance of the weights w_i in the paper, I suggest that (parts of) Appendix A.1 are actually moved into the main text of the manuscript. Many thanks.

L514 - “assume that, on average, about 95% of the vents [...]”?

L525-526 – Please rephrase slightly: e.g. “the expected distance **between past and future eruptive vents**”. Many thanks.

L530 – Similarly to my previous comment, the critical relevance of the weights w_i in the results shown in the manuscript could justify moving (parts of) Appendix A.2 to the main text of the manuscript. Many thanks.

L531-533 – Could you please expand a bit on the meaning, reasoning or implications of your choice in the presented manuscript? Many thanks.

Figures and Tables

Figure 1:

- It may be good to show where approximately within the ‘timeline’ of SSVC does the time 3ka sit. Many thanks.

- I would consider changing, at least one of, the symbol colors (dark blue and black) as, at the moment, they are very difficult to differentiate, visually. Many thanks.

- Please see my previous comments on further justifying the use of a common dataset to model vent-opening at the Nejapa-Miraflores fissure-vents system and the Apoyeque caldera. Many thanks.

Figure 2:

- I would recommend the inclusion here of some details on the approach to compute the 'thematic' vent-opening maps, not just the 'non-thematic' maps. Many thanks.

- If I understood correctly, the presence of the past vent in Figure 2b-Model 1 should modify the final vent-opening probability calculated using that model. Is this correct? If it is: would it be worthy to try show this more clearly with the 'example isolines' of vent-opening probability displayed in the figure? Many thanks.

Figures 8-9:

- Given the apparent low degree of PDC channelization that the topography of the area(s) suggests, combined with where the sectors of high vent-opening probability are located (e.g. on plain areas), I would suggest a brief justification for the use of the branching energy cone, instead of the 'classical' version. If it was feasible, running the analysis using this 'classical' version (also available in the code presented by Aravena et al., 2020a, right?), and compare the results with those of Figure 8 could be an interesting addition to the manuscript (even if it was included in the SI only). Many thanks.

Figure 10:

- L850 – "that would be derived"?

- L850 – I would change the last sentence of the caption to something like this: "in the assessment of volcanic hazard at volcanoes where eruptive style may significantly

change with vent location". Many thanks.

Table 2:

- I think there is an erratum in the title of the table (it should be "Table 2", right?). Many thanks.

Figure S1:

- If this figure is mathematically equivalent to Figure 5a, I would move the 5th- and 95th-percentile maps to the main text of the manuscript and I would then not include Figure S1 in the manuscript. Many thanks.

Figure S3:

- If this figure is mathematically equivalent to Figure 5b, I would move the 5th- and 95th-percentile maps to the main text of the manuscript and I would then not include Figure S3 in the manuscript. Many thanks.

Suggested additional references

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