

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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Thank for your useful comments and suggestions. We followed most of your comments, as described in detail in the following pages (blue texts). We preferred not to follow two of your suggestions because we do not agree with them. Nevertheless, we thoroughly explained the reason behind that. In particular, we prefer to maintain the current structure of the manuscript with Appendixes rather than moving Appendixes to

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the main text, as the main novelties of this work are not there. We also think that presenting a comparison of the results we obtained with the branching energy cone model respect to those that could be derived from the traditional formulation is completely beyond the scope of this paper, where this model is used just to illustrate the importance of adopting thematic maps. A comparison of these results has been already presented in detail in Aravena et al. 2020, clearly referenced in the text.

Moreover, you suggested to include thirty-one additional papers in the Bibliography, many of them repeated multiple times in the body text. We appreciate the effort to improve a literature review on PVHA, that however was not the purpose of this study. We included those that we found to be really significant additions in the context of the present paper.

We remark that all the modifications described here are already implemented in the text and figures (we did not include the file because it is not asked by the journal at this stage).

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 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 interest-

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ing 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.

[Thank you. We appreciate that.](#)

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).

[We believe that moving the Appendix into the main text would decrease the paper readability. In fact, the other reviewer appreciates the current organization of the paper, that we decided after a thoughtful discussion between all the coauthors. Furthermore, despite the methods described in the Appendix are important to the description of our approach, the main novelties of this work are not related to that part of the paper. The methods are already fully described in several previous papers and the Appendix is mostly a summary, and highlights a few minor changes in some technicalities. However, we slightly expanded the explanation of the methods in the main text, also including more citations.](#)

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).

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Yes, ballistics thematic maps are completely equivalent to the non-thematic maps because all the eruption observed at both volcanic systems are able to produce this phenomenon, and thus their weights in the thematic maps are the same (and equal to 1.0).

Regarding to the similarity of lava emission maps and ballistics/non-thematic maps, we have that the peaks of vent opening probability are located in the same zones, but the maps are not completely equivalent. The maximum vent opening probability is naturally located in the zones of high density of past vents (northern portion of fault A and the southern portion of NML, respectively). In the specific case of SSVC, this zone coincides with the zones where lava flows are concentrated, so the peaks coincide but the lava flows thematic maps present higher values in this peak. In the case of NCVC, a similar situation is observed, with most of the lava flows along NML, at south of Xolotlan Lake.

Accordingly, peaks are located in the same zone but maximum values differ. In other words, we agree, they are similar, but this is a consequence of the specific characteristics of these volcanic systems and it is not related to methodological, extrapolable considerations. We also want to repeat that the main message of the paper is related to the possibility and the importance to build "thematic maps of vent opening probability" aimed at improving our capability of mapping any specific volcanic hazard in complex volcanic fields. Keeping this in mind, it is clear that the examples given in the manuscript are also used to show the potential of the method, despite the specific results of a single.

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.

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Thank you. This is an interesting point, similar to what we already discussed in the section about how to weight the sequences of lava flows that constructed the Boqueron volcano in SSSC. We believe that this should not be considered a methodological limitation in our approach, and we better clarified that. However, since we do not use an event-counting based approach, the effects of including the Apoyeque volcano are extremely restricted, as observed in the resulting vent opening maps (i.e. Apoyeque volcano does not present high vent opening probabilities in the different thematic maps). In fact, this is only 1 of 31 vents and the weight assigned is low (or zero) in 3 of 4 thematic maps. We added the following text: "We choose to consider the volcanic activity in Apoyeque volcano and in other zones of this volcanic system in a common framework to assess volcanic hazard. It is worth noting that the influence of this assumption in the analysis of small-scale events is limited because of the restricted influence of a single vent within the entire volcanic system, and because the weight assigned to the different volcanic phenomena at Apoyeque caldera is null or small in most of the cases (for three of the four considered volcanic phenomena)". From a volcanological point of view, although volcanoes of the Nejapa-Miraflores lineament and those of the Chiltepe peninsula have different style and magma composition, it is undoubtful that their activity was strongly interfingering in the recent past (Kutterolf et al., JVGR 2007), and that the tectonic structures controlling these volcanoes are strictly interrelated (the Xiloa maar being placed just at the tip of the two structures). Dealing with a map aimed at defining probability of vent opening for volcanoes in the area of Managua, to be used for volcanic hazard assessment in that area, we are confident that the approach used is the most efficient. This is particularly true in the light of our suggestion of tracing thematic maps of vent opening probability for different hazards and/or styles of activity.

3. Discussion could be enriched around the differences between the approach of calculating $P(\text{vent location} \mid \text{hazardous phenomena})$, presented here; and that of calculating $P(\text{hazardous phenomena} \mid \text{vent location})$, 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).

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Thank you. This is a very interesting suggestion. We added this comment in the discussion including some of the citations suggested: "It is worth highlighting that the approach adopted here, where the resulting vent opening maps are conditioned on the occurrence of a specific volcanic phenomenon, is opposite from the strategy adopted in a series of studies of hazard assessment (e.g. Neri et al., 2015; Thompson et al., 2015; Bevilacqua et al., 2017a; Tierz et al., 2020), where the probability of having a specific hazardous phenomenon or a specific eruption size is conditioned on vent location. Although both alternatives can be useful for hazard assessment, their suitability is controlled by the expected application, and the two approaches are linked through the application of the Bayes Theorem. Our approach seems to be more appropriate for the construction of thematic hazard maps for a specific volcanic phenomenon and eruption size, while the opposite strategy, possibly coupled with event trees, seems to suit better in the formulation of hazard maps that combine multiple phenomena and/or multiple eruption sizes."

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.

Although the topography may imply limited channelization processes, that is not a point against the use of the branching energy cone. In fact, in absence of channelization processes, the branching formulations tends to coincide with the traditional formulation and thus this is not a relevant element to discuss its applicability. Moreover, the focus of this study is not on the analysis of the properties of the branching energy cone model versus those of the traditional formulation. This is widely discussed already in Aravena et al. (2020) and another comparison would be a repetition of those results. The main target of this study is the illustration of the use of thematic maps, which is the reason for

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using a model to describe an example of hazard maps. To address this point we clarify that channelization is not expected to be large and that under these conditions, the branching formulation tends to be similar to the traditional model, but not completely equal (in particular, in section 4).

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

[Most of the specific comments were introduced in the new version of the manuscript, as described below.](#)

Specific comments

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

[Modified.](#)

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

[Modified. We moved the sentence associated with the thematic nature of 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.

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[Please see general answer.](#)

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. $P(\text{vent} \mid \text{style})$) 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: $P(\text{style} \mid \text{vent})$. In my opinion, this aspect merits further discussion in the manuscript. Many thanks.

[Thank you. In fact, the two approaches are linked through the application of the Bayes Theorem, but we believe that both alternatives can be useful for hazard assessment, and their suitability is controlled by the expected application. This discussion was included, as described in the general answer.](#)

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

[We clarified the current formulation of the sentence by using "influenced" instead of "controlled" to emphasize that these are not the only parameters affecting volcanic hazard assessment.](#)

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

[We added some of these citations.](#)

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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.

[We added Bonadonna et al. \(2005\), Del Negro et al. \(2013\), Mead and Magill \(2017\) and Strehlow et al. \(2017\).](#)

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.

[We added Connor et al. \(2012\) and Thompson et al. \(2015\).](#)

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

[OK, added.](#)

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.

[We added the following sentence to clarify : "However, there are significant difficulties associated with interpreting and modelling the dependence between eruption style and vent position \(e.g. Thompson et al., 2015\)."](#)

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

[Modified.](#)

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

OK, we added (Tonini et al., 2015 ; Paris et al., 2019).

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

Please see our general answer.

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.

Yes, we believe it is was worth including it within the main text.

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.

We think that this is a methodological consideration (already explained in section Methods, 3.1), while section in L88 is geological framework. We prefer to avoid redundance.

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

OK. We added this information within parenthesis.

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 ‘stratigraphical/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.

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In the caption, we indicate what bifurcations mean. We also indicated the eruptions that separate the different stages of SSV, including the most recent stage of activity. We remark that the spider-diagram mainly has a descriptive purpose and is not part of the vent opening model.

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.

[Yes, now it is explained.](#)

L111 – “E-W”

[Modified.](#)

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.

[Modified.](#)

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

[Not necessarily, we are not saying so, for we do not have enough information to indicate that.](#)

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

[Of the order of one event every 1000 yr \(clearly much higher than one event every 85 yr\). However, the statistics is not robust so we prefer to avoid these numbers, which are poorly-constrained.](#)

L154-155 – I think the merging of eruptive vent locations from the three stages of activ-

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ity, 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.

[This is wrong. We are not considering the last 70 ka, but the last 36 ka for SSVC – our analysis starts with the end of Stage I, characterized by the collapse of SSV. In fact, we believe that this event significantly changed the volcanic structure. The last stage is only 3 kyrs long, not enough to be representative of the long-term eruption statistics. Indeed the recent activity did not leave the zones that were active in the second stage - the Boqueron volcano is currently active.](#)

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.

[Please see the general answer \(in particular, in section 3\).](#)

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 datageneration process in terms of vent locations. This should be carefully explained and justified in the manuscript. Many thanks.

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[Thank you, now it is discussed. Please see the general answer.](#)

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.

[We deleted "solely" to not to suggest that the number is the most critical difference. Unfortunately the paper Connor et al., \(2019\) lacks of detailed volcanological information and does not include the datasets, so it is rather difficult to detail which vents they missed.](#)

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.

[Please see section 3.](#)

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.

[OK. We extended the sentence.](#)

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.

[Please see general answer.](#)

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.

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We added Martin et al., 2004; Jaquet et al., 2012 and Tadini et al. 2017b. Other explanations were preserved in the Appendix (see general answer).

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.

Yes, the vent locations are used in both models. This is not a methodological issue. The differences between the methods are not in the dataset, but in the mathematical processing of it. Please see Figure 2.

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

100 x 100 m, now it is indicated.

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.

This information is already included in Tables 1 and 2. The weighting is linear and hence mathematically trivial. We think that the objective of Figure 2 is different, that is, explaining the nontrivial differences between the two models 1 and 2.

L214 – Change “leads” to “lead”

Modified.

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

No, the kernel is not bivariate. We are not assuming the same preferential alignment direction is valid everywhere in the volcanic field. The vent alignments spatially change, especially in SSV. We remark that in Model 1 the orientation of faults breaks central symmetry around the past vents uncertainty areas. Please see Figure 2.

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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 km²) could be easier to interpret by the readers. Also, could you specify whether the thematic ventopening 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.

We think that the best way to present this information is density distribution of probability, as adopted here. Computational grid resolution is 100 x 100 m, and hence the sum of the computational grid is 100 and not 1.

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

We extended the sentence.

L221 – Redundant text: "Jaquet et al."?

Modified.

L233 – How much the results could be expected to change if, say, $p1 = \text{Unif}(0.6, 1)$? Many thanks.

The boundary effects are insignificant and the results do not significantly change under small modifications of the uncertainty ranges.

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.

We used "assumed to be" to say expressly that this is an assumption. The not-uniform features due to mapped faults are well represented in the corresponding layer, while as-

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signing new preferential directions based on regional tectonics not represented in past activity and mapped faults, would be arbitrary. We remark that Model 2 is symmetrical around past vent locations and so we are not excluding other possible alignments.

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)”.

Additional knowledge is always useful but the relationship between geophysical surveys and the definition of these parameters is not immediately evident.

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.

Uncertainty affecting d_1 is large – from 5 km to 10 km. It is the average distance between sub-parallel regional faults, which we found to be consistent between the two systems, but locally varying. We considered a small number of subregions, about 3-4 in each case, but their exact number is not significant, and 5 to 10 km is their envelope.

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.

We separated the dependence of f and g in two groups. The dependence of Model 1 on vent locations is clearly explained at the onset of section 3. We prefer not to be redundant.

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

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suggest that the 5th- and 95th-percentile maps are included in the current Figure 5 and Figure S1 was not included in the manuscript. Many thanks.

[Please see answer in Figure S1 \(below in this document\).](#)

L266-267 – “N65W” and “N40W”, right?

[Modified.](#)

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.

[We do not have a reason to prefer 4 km rather than 5 km, it is only to fix a common criterion in the order of magnitude of the value used for d1 in our model. This parameter has only a descriptive purpose and does not require sensitivity analysis. The pixels excluded are indicated. Closer means that the distance to other faults is lower.](#)

L273 – “Sequel”?

[We used "in the analysis of the other thematic maps".](#)

L275 – “N65W”?

[Modified.](#)

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.

[Please see general answer.](#)

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’

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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.

In fact, we present in the main manuscript small scale PDCs and lava flows because they present the main differences in the resulting maps, so we are already paying special attention to small scale PDCs. We prefer not to focus in PDCs because our main message is that the methodology is applicable also to other eruption phenomena and limiting the paper to PDCs is not consistent with that. From the point of view of the methods, we want to remain as general as possible.

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

[See answer in Figure S1.](#)

L292 – Please see my previous comment.

[See previous answer.](#)

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

[Yes, it is vent 5.](#)

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.

[Connor et al., 2019 did not include enough information to precisely reproduce their](#)

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map, which is not expressed in term of probability values, but in terms of "vent per km²". The main purpose of that paper was describing general methods with a "how to" approach, including pieces of pseudo-code, rather than preparing a robust hazard assessment. Despite this, it remains the only map presently available for the area, and we think that it deserves to be cited. We believe that our method includes several improvements, first of all the use of faults, and the uncertainty quantification efforts, and the new thematic approach. We already included a first comparison in the main text - a further visual comparison is made possible by collecting the original reference.

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.

Connor et al., 2019 did not present a probability density function, but instead a spatial density function, that lacks of any normalization step. Thus, they did not provide probability values. We could obtain some probability values by dividing their density function values by their total number of vents, 28, but we could not do this processing for their entire map, as they did not include the data in their publication, only a Figure with colors and contour lines. Please see Connor et al., 2019.

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: $100 \times \text{no. of vents}$ (1024), and not just 1×1024 , 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.

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We performed 100x1024 simulations in order to investigate how the uncertainty in vent opening maps is propagated in the derived uncertainty in PDC inundation probability. In fact, we performed 100 Latin Hypercube samplings of 1024 spatial points, thus repeating the PDC simulations from 1024 points of origin, different for each realization of the vent opening map. We added : "The reason for running different sets of simulations for each volcanic system is to investigate the propagation of the uncertainty associated with our vent opening maps in the resulting probability maps of PDC inundation."

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

[See answer in L331-332.](#)

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

[Modified.](#)

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.

[Now we indicated the typical runout distance, and we think that we already indicate the scale and the type of these eruptions.](#)

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)”

[We do not understand the references, in all those works, variable input conditions were adopted.](#)

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)”

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Modified.

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)."

We think Tierz et al. 2016b does not use a dataset of dependent input parameters. The other citations are already included.

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

30 m, now it is indicated.

L335 – Consider removing “small-scale”. Many thanks

Removed.

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

Modified.

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).

We agree. We are not saying that this is the only effect, but it is significant. We include a new phrase to include your comment: " and also by the low vent opening probability computed at the flank E of the volcano ".

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

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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

We agree, the maximum is not in Santa Tecla, we modified the description of results (also we modified the conclusions). We think that to conditionate vent opening to a specific threshold is highly arbitrary (we would not know how to justify such choice).

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

Removed.

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

Modified.

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.

Modified.

L356 – “at the NNE flank”?

Modified.

L357 – Change “parameter” to “probability”?

Modified.

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

Modified.

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

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suggested below. Many thanks.

[We believe that the original references are more appropriate as general reviews of PVHA and not just examples of vent opening probability maps.](#)

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

[Modified.](#)

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.

[We added Del Negro et al. \(2013\) and Thompson et al. \(2015\).](#)

L370 - “plugging-in”?

[Modified.](#)

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.

[We added Selva et al. \(2014\).](#)

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).”

[Modified.](#)

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.,

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2020)”

[We added some of these references.](#)

L383 – “N65W-trending fault B” instead?

[Yes, modified.](#)

L384 – “N40W-trending fault A” instead?

[Yes, modified.](#)

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.

[This would be extremely speculative. This is only based on the geological record.](#)

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

[It is Xolotlan Lake. It has two names. Now it is indicated in the text.](#)

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.

[We use "some" to clarify that differences are not evident always. For the reasons of the similarity in some of the maps, please see the general answer.](#)

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.”

[Modified.](#)

L409 - “sequel”?

[We used " below ".](#)

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 SSV. Is this correct?

We do not agree. Because the system is currently in a mostly monogenetic stage, if a past vent was polygenetic we are counting it once anyway. So we did not assume stationarity.

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

Because in this study there was not a migration of the main site of activity tens of km in the direction of a dike intrusion, but rather a change from a polygenetic stage in the central edifice, to a monogenetic stage in a volcanic field surrounding it. Please see response to L154-155.

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.

We are saying that an event counting approach would be more significantly affected by underrecording than a vent counting approach. When an improved dataset will be available, repeating the analysis could provide more accurate results in terms of event counting, but we believe that we are already capturing the vent opening spatial distribution well enough. In fact, we are not expecting to have many buried vents in zones far from the mapped vents.

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

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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).

We are interested in the next event and, because we are not currently during a sequence of activity at a specific location (either a monogenetic center or the stratovolcano), we assume that the number of repeated occurrences of PDCs and lava flows at the same vent, potentially during the same eruption, is not significant. An event counting approach, like the one we are showing as an example, would assume the opposite.

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.

We did not include the maps associated with small-scale PDCs because in this case the number of events of interest from the central event is limited and thus differences between the two approaches are small.

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

We agree. We moved the previous sentence to remove this problem by linking "conversely" with the general argument.

L435-436 – Change "fields" to "systems"?

Modified.

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 rewording the sentence slightly. Many thanks.

We indicate expressly that this is a common feature in many volcanoes. Thank you.

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L443 – Change “lavas” to “lava”

[Modified.](#)

L448 – “N65W-trending fault B” instead?

[Modified.](#)

L449 – “N40W-trending fault A” instead?

[Modified.](#)

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

[Modified.](#)

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.

[We do not think that this is key in this context. This is an illustrative application of the branching energy cone model, with the novelty that this is the first systematic application. We could use the traditional energy cone model or depth-averaged models, but these tests are beyond the scope of this paper.](#)

L461 – Please see my previous comment on clarifying where these 105 simulations exactly come from. Many thanks.

[See answer in L312-320.](#)

L470 – “Ciudad” or “Sitio”? [Modified. It is Sitio.](#)

L477 – “decreased”

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Modified.

L482 – Change “hazards” to “risk”?

Modified.

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.

[Please see response to L206-207.](#)

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

Modified.

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.

The prior pdfs are shortly described: "Namely, $z=z_1$ is the distribution of mapped faults, and $z=z_2$ is the distribution of unknown faults". In L244-245 they were already described as : "One layer, z_1 , is related to the mapped faults (i.e. fault outcrops), and the other layer, z_2 , is a uniform PDF representing the unknown (i.e. buried) faults.". We added a short sentence in the Appendix: "In particular, z_1 is uniform inside a 100 m buffer along the fault outcrop, which is not sensibly affecting the results because it is an order of magnitude lower than d_1 ."

The symbol ξ is used as the integral variable for spatial location x . However, there was some ambiguity in the use of the symbols in formula (3), that we corrected it to improve clarity. Thanks for spotting this.

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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.

OK. It was already shown, but after the symbol change it is more clear.

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.

See general answer.

L514 - “assume that, on average, about 95

Modified.

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

Modified.

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.

See general answer.

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

The bandwidth assume to estimate the bivariate distance so the Rayleigh distribution is more appropriate than a Gaussian. This is just a technical detail without the only consequence to be more mathematically rigorous.

Figures and Tables

Figure 1:

- It may be good to show where approximately within the ‘timeline’ of SSVc does the

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time 3ka sit. Many thanks.

Ages are indicated in the figure and we prefer not to add a temporal indication because it would order eruptions whose stratigraphic relationships are not known (this is the reason of bifurcations). However, we added information about the three periods in the caption and about bifurcations meaning.

- 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.

Thank you. We used yellow instead of blue.

- 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.

See general answer.

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.

Please see the response to L212-214.

- 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.

We do not understand this suggestion. The example isolines are quite different.

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

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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.

The analysis of the channelization properties of the model is not the scope of this paper, which was largely discussed in Aravena et al. 2020. We think that this type of analysis is not meaningful in absence of calibration procedures to set input parameters and thus any conclusions would be strongly associated with the set of input parameters considered here. So we could say things that are only true in these specific conditions.

Figure 10:

- L850 – “that would be derived”?

[Modified.](#)

- 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.

[Modified.](#)

Table 2:

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

[Thank you, it was solved.](#)

Figure S1:

- If this figure is mathematically equivalent to Figure 5a, I would move the 5th- and

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95th-percentile maps to the main text of the manuscript and I would then not include Figure S1 in the manuscript. Many thanks.

We prefer not to follow this suggestion because it would increase the figure number (and we preferred to prioritize other figures) and also it can produce confusion between thematic and non thematic maps, with a figure that is both of them at the same time. We prefer to present only thematic maps with the same format and the non-thematic map with other format, which is only used to compare.

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.

[Please see previous answer.](#)

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Please see the individual answer, where we indicate the included references in the new version of the manuscript.

Interactive comment on *Nat. Hazards Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/nhess-2020-382>, 2020.

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