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Natural Hazards and Earth System Sciences
Discussions

NHESSD

Interactive comment

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

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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 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.

I would like to sincerely thank the authors for the effort put on their first round of reviews. I am generally satisfied with most of their responses and modifications. However, I have a few additional comments that I think are important before the article can be accepted for publication. These are the following:

1. I think it is extremely important that the authors provide any future reader of the manuscript with a list of references that is as comprehensive and unbiased as possible. This list must reflect, and acknowledge, the research work developed by diverse groups of colleagues on the different topics covered by the article. In particular, I find that the omission of references to previous research on the first attempts to apply the (classical) energy cone model to PVHA of PDCs (i.e. Tierz et al., 2016a, b; Sandri et al., 2018), prevents the authors as well as any future readers of the manuscript from recognising that previous work, which I think is quite relevant in the presented manuscript. Many thanks.

2. I also think that it is necessary that the authors clearly state and discuss, not only the advantages of the methods, models and results presented, but also their potential limitations. This will help the future readers of the manuscript, especially those who come from slightly different backgrounds, to obtain a better picture of the problems presented, and partially tackled, in the submitted manuscript. Many thanks.

3. Finally, I keep thinking that including Appendices A.1 and A.2 in the main text of the manuscript (leaving A.3 as the only Appendix) would improve its readability. They are only 1.5-pages long and, in my opinion, they include the main novelty of the work, which is incorporating the eruptive style (hazardous phenomena) into the calculation of vent-opening probabilities. Many thanks.

The rest of my comments mostly relate to more specific points and suggestions, which I believe could help improve the manuscript. Hereinafter, responses to authors' comments in the file (https://doi.org/10.5194/nhess-2020-382-AC2) are given in italic, red text, and introduced as, e.g. 'C1', for reference to the pdf page in the aforementioned file. 'LR1' refers to the Line 1 in the revised manuscript (https://nhess.copernicus.org/preprints/nhess-2020-382/nhess-2020-382-AC3-supplement.pdf). If there was anything unclear, please do not hesitate to let me know. Many thanks.

In summary, I would support the acceptance of this contribution to Natural Hazards and Earth System Sciences, after minor revisions have been implemented on its first reviewed version.

Receive my best regards,
Pablo Tierz

Specific comments

C1 - 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 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).

Many thanks for your explanations. I agree with the point of not including a comparison with the results obtained using the classical energy cone. This was more a suggestion or curiosity, but I understand that it may be beyond the purpose of the manuscript. Nevertheless, I would like to stress that further comparisons between the branching and classical versions of the energy cone model would be desirable in the future, as PDC inundation drastically changes from one volcanic system to another, both in terms of PDC source conditions as well as due to the topographic surface. Hence, I would suggest that the results in Aravena et al. (2020) were a useful starting point, but not the final and conclusive point as regards comparisons between the branching and classical energy cone models. Many thanks.

Concerning the references, I thought that it was convenient that the authors acknowledged, more clearly, the research work done by a variety of groups of scientific colleagues, in the different topics covered in the manuscript, e.g. vent-opening probability models, PVHA of different hazardous phenomena, and PVHA of PDCs. I would say that it is not a matter of the number of references, but their relevance in the context of the manuscript. In my opinion, the most important omissions are the references to previous work on PVHA of PDCs using the energy cone model (Tierz et al., 2016a, b; Sandri et al., 2018), which at the time were the first attempts to use the energy cone model for PVHA of PDCs. I would also like to note that Sandri et al. (2018), additionally, presented the application of an approach to model the effect of partially-submerged vents. In this respect, I find extremely surprising the deliberate omission of this reference in LR60-61 ("This is evident for example in partially submerged calderas, in which the style of activity of vents in the submerged zones is clearly influenced by their position"), as I had already suggested its inclusion during my first round of reviews. I sincerely think that the future readers of the manuscript should be aware of this previous research work. Many thanks.

Finally, in terms of Appendices A.1, A.2, please see my general comment above. I really think it would not be detrimental in terms of the length of the presented manuscript (they only represent 1.5 pages, in peer-review format); and it will give the future readers a more immediate hold to the main methodological novelty of the presented manuscript (i.e. including the hazardous-phenomena data in the vent-opening model, e.g. L533-541). Many thanks.

C3 - 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

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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.

I will let the Associate Editor to decide on this aspect, but I hardly see how including Appendices A.1, A.2 inside Section 3.1 of the manuscript would decrease its readability. Those appendices are referred to twice inside the text of that section, which seems to suggest that this is important information for the reader to know at that point inside the manuscript. Personally, as one of the first 'external readers' of the manuscript, I would have obtained a much better picture of the methodology presented if those Appendices had been included in Section 3.1. Many thanks.

C4 - 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 openinig 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.

Many thanks. I may have overlooked the differences in the case of SSVC. In the case of NCVC, I still see the maps as strikingly similar. I wonder if adding a few more isolines to the maps (perhaps at the cost of reducing the text font size for the isoline values?) could help the reader appreciate the differences between the non-thematic/ballistics and the lava-flows maps more clearly.

There is also the case of the low-intensity tephra-fallout maps, which to me, look hard to differentiate from the non-thematic/ballistics thematic maps. I suppose that my point was that, out of the four thematic maps presented (ballistics, lava flows, tephra fallout and PDCs), one is mathematically equivalent to the non-thematic map (ballistics) and another two (lava flows and tephra fallout) are very similar to the former. Accordingly, I think it would be informative for any future reader that these observations were clearly stated in the main text of the manuscript. Of course, this result is volcano-specific and may be completely different at another volcanic system under study. However, in the presented cases, I think it is important to stress the high relevance of considering thematic vent-opening maps for PDCs (i.e. an advantage of the method in the presented cases), but also to acknowledge the limited impact of considering the thematic vent-opening maps for lava flows and tephra fallout (i.e. somewhat a 'limitation' of the method for the presented cases). Many thanks.

C5 - 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 SSVC. 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 amd magma composition, it us undoubtful that their activity was strongly intefingered 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.

Many thanks. I was not suggesting that this was a methodological limitation of the ‘thematic-vent-opening’ approach but an interesting point about the volcanic hazard assessment presented in the manuscript. I still think it deserves a sentence or two in the main text of the manuscript, probably citing the relevant Kutterolf et al. (2007) reference. In my view, even if the mapped tectonic structures are closely related in space, the evident differences in terms of magma chemistry and eruption size/style would point to differences in the magmatic plumbing systems. One could expect that this has implications for vent opening and, therefore, it should be briefly discussed in the manuscript. Hence, in the text introduced in the reviewed version of the manuscript, a volcanological explanation is still lacking:

“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. ***[Volcanological explanation about the implications of this choice, including the possible limitations, e.g. decoupling in terms of magma geochemistry and eruption sizes/styles]. Nonetheless,*** *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)."*

C6-C7 - 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 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).

Please see my previous comments about how it is important that the presented manuscript cites previous research on PVHA of PDCs using the classical energy cone model, particularly if PDC channelization at the volcanoes under study is expected to be low and, hence, the behaviour of the branching energy cone model is expected to approximate that of the classical energy cone.

As I said in a previous comment, I understand that comparing the two formulations of the model is not the goal of the presented study. However, as I also said in a *previous comment, I find the assertion: “This is widely discussed already in Aravena et al. (2020) and another comparison would be a repetition of those results”, very difficult to justify. Considering the strong variability in PDC source conditions and topographical surface among different volcanic systems, one may strongly argue that future comparisons between the branching and classical energy cone models will be anything but the repetition of the results presented in Aravena et al. (2020). Many thanks.*

C9 - 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).”

LR55-56 - I still think that, in this context, Tierz et al. (2020) would be an informative reference for the future readers of your manuscript (please see Section 5.3 and Supplementary Material of that article, https://doi.org/10.1029/2020GC009219). Many thanks.

C10 - OK, we added (Tonini et al., 2015 ; Paris et al., 2019).

LR60-61 - I genuinely do not understand the deliberate omission of Sandri et al. (2018) in these lines.

C12 - 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.

LR158-167 – OK. In that case, please clearly specify this temporal (and spatial) constraint on the analysis in lines LR158-167. In my view, this is not straightforward to understand upon the first read of the main text of the manuscript alone. Many thanks.

C14 - 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.

My comment here wanted to address the fact that the vent positions have 'double' influence on your final vent-opening probability maps, as computed following the schematic shown in Figure 2. In other words, the dataset of vent locations is used 'twice' (in Models 1 and 2), while the dataset of fault locations is used only once (in Model 1). Is this correct? If it is, perhaps some comments on this choice could be added to the main text of the manuscript. Many thanks.

C16 - Additional knowledge is always useful but the relationship between geophysical surveys and the definition of these parameters is not immediately evident.

LR250-253 - Still, I think the future readers of your manuscript would benefit from knowing about approaches that try to link geophysical information with vent-opening probabilities in this point of the main text. Otherwise, it appears as if the future solutions to this complex problem could only be provided by the approaches adopted by the authors of the presented manuscript. Many thanks.

C16 - 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.

OK. But some clarification was still needed in LR265-266, which I think now reads *more clearly. Many thanks.*

C17 - 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.

*LR282-290 – Perhaps you just need to clarify the latest point in the main text, stating: "excluding the pixels that are closer to fault B **than to fault A**". This may apply to other occurrences of "closer", "near". Many thanks.*

C18 - 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.

Please see my previous comment about how it is important to stress the main advantages of the presented methods, and results, but also the potential limitations. The fact that 3 out of 4 thematic vent-opening maps presented are either identical or quite similar to the non-thematic vent-opening maps does not necessarily have to stand as a limitation of the methodology presented but, at least, it has to be discussed as a potential limitation of the presented results. Many thanks.

C20 - 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.”

OK. Many thanks for the clarification. Thus, if I understood correctly, your set of 1024 initiation points (i.e. vent locations) for the energy cone model change for each of the 100 LHS designs explored. As a consequence, the number of initiation points per grid cell across the vent-opening spatial domain may change from one design to another, and some grid cells may lack initiation points for a given LHS design. Is this correct? The approach presented here is more similar to that of, e.g., Rutarindwa et al. (2019) [for TITAN2D] and different from, e.g., Sandri et al. (2018) or Clarke et al. (2020), where initiation points for the energy cone model were fixed at the center of each grid cell across the vent-opening spatial domain. I think you should add a brief comment on this. Many thanks.

C20 - Now we indicated the typical runout distance, and we think that we already indicate the scale and the type of these eruptions.

Please note how phreatomagmatic PDC events with average runout distances of 2 km may not be optimal examples to model PDC channelization. I think the future readers of the manuscript should be clearly informed about these aspects. Many thanks.

C20 - We do not understand the references, in all those works, variable input conditions were adopted.

LR340-344 - Maybe I did not express myself properly in this comment. What I meant was acknowledging previous studies that had used variable input conditions for the energy cone model: “We decided not to use variable input conditions for initial PDC *characteristics, so our hazard assessment is only valid in this specific scenario of PDC size and friction angle* ***(see Tierz et al., 2016a, b; Sandri et al., 2018; Aravena et al., 2020a; Clarke et al., 2020; for hazard assessments that incorporated this variability into the energy cone modeling)****". Many thanks.*

C21 - Modified

LR342-344 - Here, the authors deliberately choose to omit Tierz et al. (2016a) and Clarke et al. (2020), both of which use analogue-volcanoes data to parameterize the model used in the presented study (the energy cone), to perform "more complete PDC hazard assessment considering variable size and friction properties". On the contrary, they choose to cite Sandri et al. (2012), which I agree represents an interesting use of analogue volcanoes for PDC hazard assessment, but it does not use the model presented in the current manuscript. Please also note that Sandri et al. (2012) is omitted later on in LR390-392, in a context where I believe it is a very relevant reference to cite. I struggle to understand all this reasoning on the choice of references.

C21 - We think Tierz et al. 2016b does not use a dataset of dependent input parameters. The other citations are already included.

LR344-347 - Tierz et al. (2016b) did use a dataset of dependent input parameters to explore the importance of theoretical uncertainty (a source of epistemic uncertainty) on the outputs of the energy cone model (please see, for instance, Sections 9.2.6, 9.3.4 and Figures 9.4, 9.8 of the article, https://doi.org/10.1002/9781119028116.ch9). Additionally, Tierz et al. (2018) adopted an inter-eruption-size dependence between the PDC volume and the bed friction angle at Somma-Vesuvius (Italy), and discussed the role of theoretical uncertainty for TITAN2D (please see Section 4.2 of the article, https://doi.org/10.1029/2017JB015383). Many thanks.

C21 - 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 ".

LR356-358 - Many thanks. Still, I would say that something like the following could be more appropriate:

"Modelled invasion probability at the SE flank of San Salvador volcano and in the city of San Salvador tends to be low, where a significant effect is exerted by the topographic barrier of Cerro El Picacho, ***as well as of the entire volcanic edifice of SSVC*** *(Fig. 8),* ***given that vent-opening probabilities are highest on the NW flank of the volcano and on the surrounding plain to the N-NW of the edifice; and low on the E*** *flank of the volcano* ***(Fig. 8)****."*

C22 - 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).

L358-361 – It is not clear to me how the description of the results has been modified around these lines. My suggestion of conditioning the vent-opening to the summit of SSVC, its N-NW flanks, and the surrounding plain in these directions was to address the plausibility of this assertion: "This is favoured by the high slope of the main edifice and the absence of significant topographic barriers in this direction".

If the high values of P(PDC) between Santa Tecla and San Salvador shown in Figure 8 decreased significantly when vent-opening was conditioned to the aforementioned areas, then the assertion would be difficult to support.

I think that, at least, you should reword the sentence in L358-361 following something similar to the text below. Many thanks.

"The highest PDC invasion probability calculated at the metropolitan area of San

Salvador, 2-3%, is located in its western sector, i.e. between the western sector of San Salvador and Santa Tecla. ***These values might be explained by the peak in vent-opening probability shown in Figure 4 and/or by*** *the high slope of the main edifice and the absence of significant topographic barriers* ***on the southern sector of the SSVC edifice"***

C23 - We believe that the original references are more appropriate as general reviews of PVHA and not just examples of vent opening probability maps.

LR383-384 - In my personal view, I still struggle with Poland and Anderson (2020) being the most appropriate reference to accompany the sentence: "Several probabilistic assessments of vent opening at different volcanoes have been presented during the last decade". As I suggested in my first review, I think there are other references which would fit better in this sentence. Many thanks.

C23 - We added Del Negro et al. (2013) and Thompson et al. (2015).

LR388-390 - In my opinion, omitting Sandri et al. (2014) from this list is unfortunate, as that article presents a variety of examples of "models aimed at describing the dispersal of volcanic products". Many thanks.

C23 - We added Selva et al. (2014).

LR390-392 - In my opinion, omitting Sandri et al. (2012) here is also quite unfortunate. The article is cited earlier in the text but it is in these lines where the reference is highly relevant. The article by Sandri et al. (2012) shows how vent-opening probabilities, and as a result PDC invasion probabilities, change in time as a result of changes in the recorded volcano monitoring data (please see Figures 3, and 6-8 of the article, https://doi.org/10.1007/s00445-011-0556-y). I think this is strongly relevant for the *content of the presented manuscript. Moreover, I think adding one of the other two references (either Marzocchi et al., 2008 or Tonini et al., 2016) could still be beneficial for the future readers of your manuscript. Many thanks.*

C25 - We are saying that an event counting approach would be more significantly affected by underrecording than a vent counting approach. When an improved datased 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.

OK. Many thanks for the clarification.

C26 - 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.

OK. I understand that the thematic vent-opening approach could be implemented using different PDC models (and models for other volcanic hazardous phenomena as well). Nevertheless, given that you present a first systematic application of the branching energy cone model for PVHA, I sincerely believe that it is important that any future reader of your manuscript can be referred (earlier in the main text of the presented manuscript) to previous research that investigated the first-ever applications of the (classical) energy cone to PVHA of PDCs (Tierz et al., 2016a, b; Sandri et al., 2018). This was an interesting matter of scientific debate back in those years and, judging by the recent developments in the field, it will continue to be relevant over the years to come. Many thanks.

C30 - We do not understand this suggestion. The example isolines are quite different.

*What I meant is that the example for Model 1 in Figure 2b seems to show well the influence of the presence of the fault on the isolines of vent-opening. However, it does not seem to show a clear influence of the presence of the past vent, which should also modify the isolines of vent-opening (compared to the case if the past vent was not there). Is this correct? I was just wondering if the isolines shown for Model 1 in Figure 2b could demonstrate the influence of **both** the fault line and the past vent on the vent-opening probabilities computed from Model 1. Many thanks.*

Suggested additional references *(please see the list given here, https:// doi.org/ 10. 5194/ nhess-2020-382-RC2, for a few relevant references that are still missing from the presented manuscript. Many thanks)*

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