

# Author's response to Editor

<p>Dear Authors,</p> <p>We have now received the reports from two reviewers regarding your manuscript. While both reviewers recognize the potential of your novel empirical methodology and the clarity of your writing, they have raised significant concerns regarding the statistical assumptions, the geological context, and the structure of the manuscript.</p> <p>Therefore, I am recommending Major Revisions. Before the manuscript can be considered for publication, please address the following critical points in detail:</p>	<p>Dear Editor</p> <p>we thank you for the very constructive revision.</p> <p>We significantly modified the manuscript, implementing all the suggestions. We deeply revised the writing, we added additional analyses and figures, and we restructured the manuscript, as suggested. All these modifications are detailed below.</p> <p>We think that the manuscript has overall improved significantly.</p>
<p>1. Statistical Assumptions (Radial and Azimuthal Independence): A central concern is the assumption that the probability density function can be split into independent radial and azimuthal components. As noted by Reviewer #2, this is a strong mathematical assumption that requires justification. In your revision, you must address this by doing one of the following:</p> <p>a) Prove they are independent (e.g., via bivariate plots);</p> <p>b) Discuss rigorously why this assumption is valid for this context despite potential correlations; OR c) Explicitly acknowledge this as a limitation of the model and discuss the implications.</p>	<p>As now discussed in Section 2 (lines 87 and following), the independence between radial and azimuth components was initially assumed, as in Rivalta et al. (2019) they are controlled by two different features of the caldera. However, this should have left also a track in past data.</p> <p>Consequently, we also tested the independence in existing data (Section 3.2, Supplementary Figure 5), confirming the independence that was earlier assumed only based on physical considerations.</p> <p>In particular, in Section 3.2 (line 159 and followings), we now added the description of the statistical test that we implemented to test the independence, commenting the results. In Supplementary Figure 5, we show both the bivariate plot and the results of the statistical test.</p>
<p>2. Manuscript Structure: The current format mixes methods with results, making it difficult to assess the technical validity. I suggest to restructure the paper to:</p> <p>Separate Methods from Results: Create a dedicated "Methodology" section. This must include a clear explanation of the</p>	<p>We now separated methods and results, and we introduced sections Discussion and Conclusions. Specifically, methods are described in Section 2. The results are presented in Section 3 and discussed in Section 4. Key findings and implications are reported in Section 5 (Conclusions).</p> <p>In Methods, all equations and assumptions</p>

<p>equations used and the details of the Kolmogorov-Smirnov (K-S) test, as requested by Reviewer #2.</p> <p>Add Conclusions: Both reviewers noted the absence of a formal "Conclusions" section. Please add this to summarize the key findings and implications.</p>	<p>are specifically discussed. The Kolmogorov-Smirnov two sample test is presented and discussed at its first use (Section 3.1, line 119), and a reference is also provided.</p>
<p>3. The discussion comparing your vent opening maps with recent seismicity seems currently too qualitative. Phrases such as "striking correspondence" should be supported by data or revised. You should either provide a quantitative comparison or adopt a more nuanced tone that accounts for the spatial variability of deep seismicity across the caldera.</p>	<p>We completely revised this part. The comparison with past model is made more quantitative by plotting the differences between all maps (Fig. 3A-F).</p> <p>The figure comparing our maps with seismicity and the caldera structure have been edited to better highlight the main points, and the discussion has been completely reviewed, using more appropriate terms (Section 5, line 341).</p>
<p>4. Please carefully review the comments from Reviewer #1 regarding the geological timeline, citation updates, and tectonic assumptions (e.g., dyke source models).</p>	<p>We implemented all the suggestions of Reviewer #1.</p>
<p>Minor suggestions: Section 2, page 3, line 6: It seems that the values of LON and LAT are inverted, whereas in Section 3, they are written in the correct order. Please check.</p>	<p>Corrected, thanks.</p>
<p>Cited localities, such as Pozzuoli, Agnano-Astroni, Gauro, Nisida, Solfataro, etc. should be reported in the maps.</p>	<p>We added a new figure, Fig. 1, reporting all the cited localities. We also added the most important ones in Fig. 2.</p>
<p>Page 7, line 2: Campo Miseno -&gt; Capo Miseno</p> <p>Please submit a point-by-point response to all comments raised by both reviewers alongside your revised manuscript.</p> <p>Sincerely,</p> <p>Giovanni Macedonio</p>	<p>Corrected, thanks.</p>

# Author's response to Reviewer #1

Dear Editor,

I have completed a thorough review of the manuscript entitled "Brief Communication – Vent Opening at Campi Flegrei: Clues from Dyke Propagation Patterns" by Selva and Mangone.

This work provides a concise overview of the current understanding of vent-opening maps at Campi Flegrei. It introduces a novel empirical methodology that incorporates the spatial distribution of past vent locations along with the influence of topographic relief on dyke trajectories. The results, presented as vent opening probability maps, suggest two concentric annular maxima at approximately 2 km and 4 km radial distances, with a general peak toward the northeast and additional local maxima corresponding to topographic highs. The authors also note a qualitative correspondence between their results and the epicentral distribution of recent seismicity. While this point is briefly mentioned in both the abstract and the discussion, it is not explored in depth, particularly regarding the significant variation in the depth distribution of seismicity across different sectors of the caldera.

Overall, the manuscript is clearly written

We thank the reviewer for the very constructive revision. We implemented all the suggestions, and we think that the manuscript has overall improved significantly.

In particular, we thoroughly revised all citations, also thanks to the important suggestions given by the reviewer. We improved the presentation of the method, and we now better specify the effective assumptions behind the model, by specifying both the assumptions of Rivalta et al. (2019) and those assumptions that are inherited in our study. We also significantly updated the discussion, making more quantitative and explicit the comparison with both previous models and the ongoing seismicity.

Regarding the assumptions, as now better discussed at the beginning of Section 2, we note that they are somehow looser than the one adopted in Rivalta et al. (2019). Indeed, we empirically track potential propagation of dykes from a central area of the caldera toward the position of past vents, but we do not need any assumption about depth and shape of the source (like in Rivalta et al. 2019), as we do not model dyke propagation, but we simply empirically analyze past vent positions with respect to caldera centre. In other words, our model is inspired by the physical process described in Rivalta et al. (2019), which stands for an independence between distance from the center and direction of the propagation of dykes, but we do not adopt any physical modelling here. This very important point is now better described in the manuscript.

Overall, we think that the manuscript significantly improved thanks to this revision. All these improvements are specifically discussed below.

<p>and logically structured. Nonetheless, there are several areas in which the presentation could be improved, particularly concerning the manuscript's structure, the accuracy and placement of literature citations, the clarity of the figures, and certain aspects of the wording.</p>	
<p>The proposed approach is both interesting and promising; however, the manuscript would benefit from a more explicit articulation of the underlying assumptions. For instance, the authors adopt a similar assumption to that of Rivalta et al. (2019) regarding the dyke source, While this assumption is consistent with certain modeling efforts, it simplifies the broader volcanological literature, which often supports the existence of a sill-like source. Furthermore, the presumption of a fixed source depth of 3 km for all post-NYT eruptions may lack robust petrological justification. The model also assumes predominantly lateral dyke propagation, a simplification that may not hold uniformly throughout the post-NYT eruptive period. It would be beneficial for the authors to state these assumptions more explicitly at the outset.</p>	<p>Thank you for this comment. In Section 2, we now better specify the effective assumptions behind the model, by specifying both the assumptions of Rivalta et al. (2019) and those assumptions that are inherited in our study.</p> <p>To this end, we now specify that</p> <p><i>“Rivalta et al. (2019) found that the path of the dykes feeding eruptions is mainly controlled by the geometry of the caldera, which determines the distance from the centre of the caldera, and by the topographical peaks surrounding the caldera, which establishes preferential directions for propagation. To this end, Rivalta et al. (2019) assumed that the origin at depth of the magma is located at the centre of the caldera, 3 km below the location of the maximum observed uplift (Amoruso et al. 2014, Rivalta et al. 2019, Buono et al. 2025), and producing mostly lateral propagation with trajectories controlled by the structure of the caldera and the consequent stress field. Assuming a magma origin located around the centre of the caldera, independently of its specific depth and geometry, the empirical track of these features may be retrieved by studying the distribution of past vents around the caldera centre...”</i></p>
<p>The referencing of literature requires refinement, as several citations appear either outdated or incorrectly positioned—particularly those</p>	<p>The number of references is limited 30 in “Brief Communications”, and this influenced us a bit in the writing. However, we agree with the reviewer and we added several more, as suggested.</p>

<p>pertaining to the structural evolution of the caldera and its eruptive history. More recent studies have revised the temporal framework of Campi Flegrei's activity, extending it beyond 200 ka, and have updated interpretations of the caldera structure. For instance, the structural rims are now understood to be primarily associated with the CI eruption and subsequently reactivated during the NYT phase (e.g., Natale et al., 2022, Journal of Structural Geology). Additionally, recent investigations into the physical properties of dykes within the Campi Flegrei system should be acknowledged (e.g., Buono et al., 2025, AGU Advances; Natale and Vitale, 2025, Nature Communications).</p>	
<p>In summary, I find the manuscript to be of overall high quality and recommend it for publication pending moderate revisions.</p> <p>Please find below line-by-line minor suggestions and comments.</p> <p>Best regards</p>	<p>We carefully reviewed and also implemented all line-by-line suggestions. Thank you for all of them.</p>
<p>[...] Line 9: Consider adding the term "azimuth" to clarify the reference to the orientation of the dykes.</p>	<p>Added</p>
<p>Line 10: The two principal peaks should be more clearly emphasized in Figure 2, which is currently difficult to interpret.</p>	<p>We modified Figure 2 (now Fig. 3), and now the peaks are more evident. They appear very clearly in the comparison with previous models, reported in Fig. 4.</p>
<p>[...] Line 14: The earliest volcanic activity</p>	<p>Thank you for this comment. We corrected the text accordingly, and we added the</p>

<p>observed in outcrops within and beyond the caldera is estimated at approximately 80 kyr, based on Pappalardo et al. (1999) and Scarpati et al. (2013). However, recent studies of widespread tephra layers in Italy and the Mediterranean extend the chronology of CF activity to nearly 200 kyr (e.g., Monaco et al., 2022 - GPC; Fernandez et al., 2024 - QSR). This includes a recently identified large-magnitude eruption at 109 ka (e.g., Fernandez et al., 2025 – Communications Earth and Environment). Tephra deposits of comparable age and composition have also been retrieved from boreholes (e.g., Sparice et al., 2024 – JVGR). These more recent contributions should be acknowledged.</p>	<p>references.</p>
<p>Line 15: Please include a reference to the Campanian Ignimbrite eruption/age.</p>	<p>Added</p>
<p>Line 15: The statement that all eruptions occurred within the CI caldera rim is not accurate. For example, eruptions took place at Procida Island (De Astis et al., 2004) and at CFc, such as the Torregaveta eruption.</p>	<p>We removed the sentence, as not necessary.</p>
<p>Line 17: The citation of Sbrana et al. (2021) is inaccurate, as these authors attribute the caldera structure solely to the CI eruption.</p>	<p>We removed the sentence, as not necessary.</p>
<p>Line 18: The term “inner caldera” has a specific structural meaning, yet many post-NYT vents are located beyond this boundary. Orsi et al. (2004) is incorrectly referenced here, as it does</p>	<p>We removed the word ‘inner’, as not necessary.</p>

not address volcano-tectonic structures.	
Line 19: A citation to Di Vito et al. (1999) would strengthen this statement.	Added, thanks
Line 19: Consider modifying the sentence to read "comprises at least 33 eruptions."	Corrected, thanks
Line 19: Correct "spaning" to "spanning."	Corrected, thanks
Line 20: Adjust 15,000 to 14,000 for consistency with line 16.	Corrected, thanks
Line 22: Revise 9.200 to 9.100.	Corrected, thanks
Line 23: Revise 28 to 26.	Corrected, thanks
Lines 23–25: Consider dividing the sentence after "3,800 years ago." A more precise description could be: "Its activity was predominantly concentrated in the northeastern part of the caldera (Agnano area), secondarily in the northwestern sector (Averno area), and concluded with peripheral distal eruptions (Nisida, Capo Miseno, and Fossa Lupara)." Recent works provide additional context (Natale et al., 2025 – GSA Bulletin).	Corrected and integrated, as suggested
Line 27: Update the reference to Di Vito et al. (1987 – Bulletin of Volcanology).	To avoid too many additions, we added directly Di Vito et al. 2016 (Scientific Report)
Line 28: It would be useful to cite recent studies on caldera resurgence at La Starza cliff (Isaia et al., 2019 – JVGR; Natale et al., 2022 – Basin Research).	We added the suggested citations, thanks

<p>Line 31: The history of ground deformation since 35 BCE has been reconstructed by Di Vito et al. (2016 – Scientific Reports). Vitale and Natale (2023 – Earth, Planets, and Space) describe the long-term deformation pattern.</p>	<p>We added the suggested citations</p>
<p>Line 33: Correct the date to 1982–84.</p>	<p>Corrected, thanks</p>
<p>Lines 34–35: Suggested rephrasing: “which fully recovered the subsidence in 2021, and now exceeds the uplift peaks observed in the last century.” If this statement refers exclusively to deformation, cite Bevilacqua et al. (2024). If it encompasses seismicity and degassing, please include additional references accordingly.</p>	<p>We modified the text accordingly.</p>
<p>Line 40: Suggested revision: “[...] parameters, focusing on the tectonic structures recognized at that time, to track [...].”</p>	<p>Corrected, thanks</p>
<p>Line 45: Update to Charlton et al. (2020), not 2018.</p>	<p>Corrected, thanks</p>
<p>Line 46: Replace with “corresponding to the caldera rim.”</p>	<p>Corrected, thanks</p>
<p>Line 52: Suggested rephrasing: “Rivalta et al. (2019) analyzed the effects of caldera unloading, as well as those of topographic peaks, [...].”</p>	<p>Corrected, thanks</p>
<p>Line 55: Insert “may”: “[...] creates a stress field that may favor magma trajectories [...].”</p>	<p>Corrected, thanks</p>

Line 56: Add "(unloading effect)": "[...] of the caldera (unloading effect) significantly [...]."	Corrected, thanks
Line 57: Clarify the intended wording: "Geometric centre at a given distance"?	We mean 'geometric centre of the caldera'. Corrected, thanks.
Line 60: Replace with "left a trace."	Corrected, thanks
Line 66: Correct citation to Amoruso et al. (2014).	Corrected, thanks
Line 67: Refer also to the general comment above regarding assumptions on the dyke source.	As suggested, we added the assumptions, as discussed above.
Line 68: Verify coordinates, as longitude (Easting) appears to contain one digit too many, while latitude (Northing) appears to be missing one.	Corrected, thanks
Line 76: Please ensure consistent use of terminology ("dyke" vs. "dike") throughout the manuscript.	Corrected, thanks
Line 85: Among the cited works, only Rivalta et al. (2019) appear to state this explicitly.	Corrected, thanks
Line 88: The assertion regarding topographic control seems to be treated as a fact, whereas it should be presented as a working hypothesis.	Now the assumption is better stated in the previous section, and it is recalled here as "Under the assumption of propagation from the caldera center, this distance represents the length of the horizontal propagation of the dykes that alimented such eruptions."
Line 101: Consider expanding the statement: "[...] the stress field is mainly controlled by unloading."	Added
Line 102: Please clarify whether Figure	They are the maxima in radial swath profiles

<p>1c represents simple topographic profiles or averaged swath profiles. The latter would provide a more representative average topography.</p>	<p>with specific length. Now we added this in the caption and in the main text.</p>
<p>Line 107: Use "La Starza marine terrace."</p>	<p>Corrected, thanks</p>
<p>Line 107: Clarify orientation: If La Starza terrace is located NW of the center, it should correspond to ~340° azimuth, not 50°.</p>	<p>Correct, La Starza is at NE of the caldera center, thus it is at around 50°.</p>
<p>Line 108: Correct to "Camaldoli Hill."</p> <p>General: These locations should be shown on a map, as readers unfamiliar with Campi Flegrei may otherwise be confused.</p>	<p>Corrected, thanks</p>
<p>Line 112: This observation indicates an anticorrelation, or at least underscores the need to state at the outset that the spatial distribution of vents is hypothesized to be controlled by topography.</p>	<p>The peak toward NNW is present only for small radii, up to 5 km, while it is almost negligible for larger radii. This is in agreement with the results of the test that we describe in the following lines, that found that radii below 7 are not in agreement with the data. Now we made this point clear in the text (line 134-150).</p>
<p>Line 120: Invert the order of this sentence and the next, so that it follows the order presented in the formula.</p>	<p>Done</p>
<p>General comment on Figure 2: Figures could be made more legible by using a lighter, more uniform background (e.g., shaded relief). The seismic hypocentre dots currently obscure the NE peak within the inner circle.</p>	<p>We followed this suggestion, and indeed Figure 2 (now Fig. 3) improved significantly.</p>
<p>Line 142: Consider renaming this section "Discussion and Conclusions," as</p>	<p>Done</p>

<p>the final sentences are conclusive in nature and a separate Conclusions section is absent.</p>	
<p>Line 149: The reference to “E and SE” is unclear. Does it pertain to Baia and Bacoli, or to Bagnoli and Nisida? Please clarify and specify locations.</p>	<p>The direction is W and SW referred to Baia and Bacoli.</p>
<p>Line 151: It would be appropriate to include the map from Alberico et al. (2002) to illustrate similarities and differences, rather than requiring the reader to consult that paper independently.</p>	<p>We now removed all the original maps and we report the map of the differences, including Albertico et al. (2002). In particular, we added the comparison in Fig. 4A,D and the original maps in Supplementary Figure 7.</p>
<p>Line 170: The difference in the Solfatara peak is not particularly pronounced; it is already visible in Selva et al. (2012) and Bevilacqua et al. (2016).</p>	<p>We now made a specific and more quantitative comparison with both models (new Fig. 4), making the comparison much more explicit. This comparison highlights that in the Solfatara area, the peaks of the models developed here are more pronounced than in all previous studies, especially for model M1 (empirical without topography)</p>
<p>Comment on Figure 3F: According to the caption in Tizzani et al. (2024) and cited references, the rims shown in Tizzani et al. (2024) correspond to the CFc caldera rims, not the CI and NYT rims. Revising accordingly would improve consistency throughout the text.</p>	<p>Corrected, thanks</p>
<p>Line 172: The directions are unclear. By “E and SE,” do you, in fact, mean “W and SW,” referring to the topographic highs at Baia and Capo Miseno?</p>	<p>Yes, W and SW. Corrected, thanks.</p>
<p>Line 178: Natale et al. (2025) is not included in the reference list.</p>	<p>Added, thanks.</p>

<p>Final comment: A dedicated "Concluding Remarks" section is missing and should be provided if the name of the section is not updated as suggested above.</p>	<p>We updated the section's name as suggested.</p>
<p>I trust that these comments will assist the authors in enhancing the clarity, accuracy, and overall quality of their manuscript.</p>	<p>They helped improve the text a lot indeed. Thank you for this very constructive revision</p>

## Author's response to Reviewer #2

The manuscript describes an interesting approach to define a vent opening map at Campi Flegrei caldera by splitting the density function in polar coordinates as the product of radial and azimuthal components. While this formulation is promising in studying the vent opening patterns, the manuscript does not provide any convincing argument for assuming independence between the radial and the azimuthal patterns, nor an uncertainty quantification of the output. In fact, splitting the density function in two factors is equivalent to assume they are independent, and that seems a pretty strong assumption, to me, although it may provide an interesting point of view on the problem.

The manuscript, after a general introduction, follows with a description of the empirical distributions of direction and radial distances of past vents from the caldera center. Then, it describes the split formulation of vent opening probability, before a Discussion section in which the new vent opening maps are briefly described and compared to some pre-existing vent opening maps and to the current geophysical unrest pattern.

We thank the reviewer for this revision and the interesting suggestions provided. We implemented all the suggested analyses as specified below, significantly improving the manuscript.

In particular, we extended the text to better describe the methods and related assumptions (Section 2), adding several new analyses and leading to an update of all figures and to the addition of 3 supplementary figures. We now split methods and results in two sections (Sections 2 and 3). The discussion (now reported in a separate section, section 4) has been significantly extended, by adding all the suggested additional points. We added a more quantitative comparison between our results and all the previous models existing in literature, as well as with the other geophysical data. We also added a concise conclusion session (section 5), which includes a specific discussion about epistemic uncertainty on vent opening, as detailed below. These changes address all reviewer's suggestions.

Among the added analyses, we now specifically study the correlation between the radial and azimuthal components in past data, as well as we implemented the suggested kernel for the analysis of empirical distributions. Regarding specifically the independence between radial and azimuthal components, we formally tested the hypothesis of independence through standard hypothesis testing (Section 3.2, Supplementary Figure 5). The results stand for the independence that was earlier (in the first version of the manuscript) assumed only based on physical considerations (see comments below for more details). This test is now described in the main text (end of Section 3), and an additional supplementary figure was produced to show the test results in detail (supplementary figure 5).

Regarding uncertainty quantification, we agree that it is an important topic. As now

	<p>highlighted in the conclusions, here we concentrate on describing one approach that is effectively an alternative to the ones available in literature. The very existence of a completely different approach is an added value for future epistemic uncertainty quantifications. However, in this manuscript we prefer avoiding the additional complication of quantifying an epistemic uncertainty on the specific approach that surely represents an underestimation of the effective epistemic uncertainty on the process (vent opening). This very important discussion is now added in the conclusion (line 253 and following), as very last point of the manuscript.</p> <p>We thank the reviewer for the important comments that gave us the opportunity to significantly improve the manuscript. All these improvements are specifically discussed below.</p>
<p>The manuscript showcases interesting ideas, but it is incomplete and unconvincing in providing a new vent opening map. Also, the methodology section is missing and the methods are mixed with the results section, and insufficiently detailed. Another problem is the unclear comparison between past vent opening positions and geophysical unrest patterns, in the discussion.</p> <p>The text is generally well written, but I am not a native speaker so I may have overseen minor language issues. The key novelty of this research stays in the split formulation, but that would deserve a more extensive analysis and discussion.</p>	<p>We significantly edited the manuscript, also thanks to reviewers' comments. In particular, we better clarified the main assumptions, and we improved the presentation of the method.</p> <p>We separated methods from results and discussion (now sections 3 and 4), and we added a concise conclusion section (section 5). In section 4, we improved the comparison with previous methods and with geophysical data (new Fig. 4), by reporting the suggested analysis of the difference with the previous methods (we also added the comparison with Alberico et al., as suggested by reviewer 1), and by producing specific maps to compare earthquakes and structural features.</p>
<p>In summary, the results are incomplete and the discussion section is</p>	<p>We significantly improved the discussion and conclusion sections, adding most of the reviewer's suggestions. We added new</p>

disappointing. I strongly suggest re-shaping the manuscript by adding new analyses, separate the methods from results, and rewrite the Discussion. Probably, a plot of the differences between the pre-existing vent opening maps and the results of the manuscript would help to highlight how the previously published maps differ from the manuscript's results. Very importantly, if the authors aim at introducing a new vent opening model, they should provide an uncertainty quantification of the probability density values. Also, having a clear and concise Conclusion section would be really appreciated. Finally, please consider if a "plain" research article would not be a better format for this manuscript.

analyses like the correlation analysis and test, we introduced a kernel for the analysis of empirical distributions, we made a quantitative comparison between this model with all models existing in literature, and we improved the comparison with the other geophysical data (Fig.4).

We separated methods and results, and we significantly improved the discussion section, addressing all reviewer's suggestions. All these improvements are specifically discussed below.

As now discussed in the Conclusions (Section 5), regarding the epistemic uncertainty quantification, we agree that it is an important topic. As now highlighted in the discussion, "... we prefer not to quantify the epistemic uncertainty on the proposed approach, differently from what was done in previous studies like Selva et al. (2012) and Bevilacqua et al (2015). The reason is that several case studies recently demonstrated that the effective epistemic uncertainty on a target physical process (here vent opening) is better estimated by combining radically alternative approaches (e.g., defining weighted ensembles of alternative models), rather than by exploring the epistemic uncertainty inherent to one specific approach (Selva et al. 2015; Marzocchi et al. 2021; Meletti et al. 2021, among the others). Consequently, while the epistemic uncertainty on a given approach may be of relative interest, the very development of an alternative approach like the one presented here may be a significant added value to future quantification of epistemic uncertainty in the process of vent opening at Campi Flegrei, via multi-model ensembles or equivalent approaches that combine all the scientifically grounded approaches available in literature (e.g. SSHAC 1997, Marzocchi et al. 2017, 2021)." (Section 5). In other words, here we concentrate on describing one approach that is effectively an alternative to the ones available in literature. The very existence of a completely different approach is an added value for epistemic uncertainty quantification, but this may be the topic for future studies. This very important discussion is now added in the conclusion,

	as the last point of the manuscript (line 253 and following).
L21 – please add a reference for the 12300 BP age, in fact the 2022 book by Orsi et al. reports 11900 to 12200 BP.	We added the reference to Bevilacqua et al. 2016, thanks. Here this detail is not used in the paper, so we treat this as a general indication, without discussing details about it. For this reason, we just rounded all numbers indicating averages.
L24 – when you say that the third epoch is predominantly focused in the NE part of the caldera and secondarily in the NW sectors, you should also mention the Nisida and Capo Miseno exceptions.	Added, thanks.
L25 – is the 4550 BP age an average of 4500 to 4600 BP? Add reference please.	Yes, we averaged. We now reported 4500 and we added the reference to Bevilacqua et al. 2016. Again, this detail is not used in the paper, so we treat this as a general indication, without discussing details about it.
L29 – “geometrical center” is unclear.	We reformulated in order to make more clear this sentence
L33 – why 1984-1986? Should not be 1983-1985?	Yes, there was a mistake. Usually this unrest is referred to as 1982-84, as in the cited Del Gaudio et al. 2010. Now, we corrected the text.
L40-44 – please mention that both Selva et al. 2012 and Bevilacqua et al. 2015 are vent opening maps with uncertainty quantification, i.e., the probability density values have an uncertainty distribution. This is a key point.	Added, thanks.
L62 – Why you speak of past dykes? I would find much clearer if you spoke of past vents distances. In fact, we don't really know if all (or any) dykes actually	We updated the title, as suggested. In any case, we now better specify the assumptions of our study, which are similar to the ones of Rivalta et al 2019, who assumed that the dyke propagation starts approximately at the center of the caldera.

<p>started from the caldera center.</p>	
<p>L74-75 - Is 1000 samples the most appropriate sample size? Did you try with more, or less, samples? What changes? Also, by looking at the Supporting File I see that you probably used a uniform distribution inside the elliptical shapes of Bevilacqua et al. 2015; I can't find the uniform distribution specified anywhere, tough.</p>	<p>The samples are used to check the stability of all statistical analysis and tests. All cumulative distributions shown in Appendix indicate that this number is sufficiently large to this aim. Regarding the distribution's shape, yes, you are right, they are uniformly distributed. This is what we meant by "randomly selected within bounds". Now we made it more explicit in the text, saying that they are "uniformly distributed" (line 107). Thanks.</p>
<p>L76, L87 - again, please do not speak about dykes. It is unrequired to assume that all dykes propagated from the caldera center if you just speak about vent opening patterns. It is ok to speak about dykes in the discussion, but it is not necessary here.</p>	<p>We modified accordingly</p>
<p>L83 - you used the Kolmogorov-Smirnov test many times. For a statistician it is a classical test, but I would strongly suggest adding an explanation of how it works. This would fit very well in the new methods section</p>	<p>The Kolmogorov-Smirnov two sample test is now presented and discussed at its first use (Section 3.1, line 119 and following), and a reference is provided.</p>
<p>L119 - This formulation is not valid for every 2D density function. You can do that if (and only if) the two components (radial and azimuthal) are independent. This is really a key point that you missed to discuss.</p>	<p>Thank you for this comment. The need for independence was already specified in line 121 of the original text, and it was assumed because of the independence of the physical processes leading to different directions and distances (being the distance fundamentally controlled by the nearly circular shape of the caldera and the direction predominantly controlled by local topographic features). Thus, the independence between radial and azimuth components was initially assumed, as in Rivalta et al. (2019) they are controlled by two different features of the caldera. However, this should have left also a track in past data. This is now explicitly discussed in Section 2 (method).</p>

	<p>In Section 3.2, we now added also this test. The results (discussed in Section 3.2, lines 159-164, and reported in Supplementary Figure 5) show that the hypothesis of equal distribution cannot be rejected for all combinations of sectors, standing for the independence of the two parameters.</p>
<p>L140 – Please delete the earthquakes from Figure 2. They do not belong to the vent opening pattern analysis that you are describing here and they hide the probability density function values. Please add contour lines. The comparison of vent opening map and seismic patterns could be done in the discussion section, once it is clarified its meaning.</p>	<p>We modified the figure accordingly (now Fig. 3), removing the background to better highlight the probability values. We prefer to avoid contourlines, as the computations are made over a grid and we prefer that this appears in the figures.</p>
<p>L142 – The result section is unfortunately incomplete. Some examples of required additions are listed below.</p>	<p>Thank you for the suggestions. We tried to improve this section, also addressing all the following points.</p>
<p>1. You assumed that azimuthal and radial distributions are independent. But how the bivariate plots of distance and direction of past vents looked like? If they looked much correlated, it should be a concern for the validity of this formulation in producing a new vent opening map. However, this analysis could be useful anyway to understand that correlations in polar coordinates may play a role in shaping the vent opening patterns. This should be discussed.</p>	<p>We now report the bivariate plot, along with the results of the independence test discussed above, in supplementary figure 5.</p>

<p>2. Why you did not try to produce maps based on the three epochs? I can't see why you just tested the third epoch after having analyzed the marginal distributions of the three epochs – by doing that, you discarded half of the vent opening dataset.</p>	<p>We discharged half of the events simply because we demonstrate that Epoch 3 is significantly different from Epoch 1. Thus, the introduction of Epoch 1 (and Epoch 2, which is very close in time to it) may introduce a bias. This is now explicitly commented on both in Section 2 (line 97) and in Section 3.2 (line 174-176).</p> <p>Notably, this is in agreement with Orsi et al. (2004) that concluded that the last change in stress regime occurred prior to onset of the Epoch 3, suggesting that only the past 5 ka should be considered as reference for the present state of the caldera. For this reason, Epoch 3 was taken as reference also in Orsi et al. (2009) and Selva et al. (2012). Also this is now discussed in section 5 (line 247).</p>
<p>3. How the differences between E and W sectors could find integration in this analysis? I believe this distinction was important in shaping some of the pre-existing vent opening maps. Please give a look over 10.3389/feart.2017.00072.</p>	<p>The difference between E and W sectors discussed in that paper does not pertain to vent opening, but more in the size of eruptions (and specifically in PDC areal distributions). The size may be influenced by multiple factors, and to discuss this is out of the scope of this manuscript.</p>
<p>4. Why you used empirical distributions and not kernel functions? Is this changing much if you assume a simple Gaussian kernel with an appropriate bandwidth?</p>	<p>Thanks for this suggestion. We now implemented kernel functions to smooth the distributions, as suggested. The results are practically the same. The implementation and the results are discussed in Section 3.2 (line 171), Fig. 1, and in Supplementary Figure 6.</p>
<p>5. How the new formulation deals with a uniformly distributed layer that both Selva et al. 2012 and Bevilacqua et al. 2015 assumed inside the caldera?</p>	<p>This assumption is not made here. The size here is empirically controlled by the maximum distance recorded in the past, which is about 8 km from the caldera center. Here, as the distribution of distances never go beyond caldera borders. We now specifically comment on this in Section 3.2 (line 176) and in the comparison with previous studies in Section 4 (line 219).</p>
<p>6. Could you try quantifying the</p>	<p>The uncertainty on vent position is treated</p>

<p>uncertainty affecting all this? You may do that as you wish, but you should not totally oversee that step.</p>	<p>in all tests. Regarding the epistemic uncertainty on the vent opening probability, it is now discussed in Section 5 (see above for more details).</p>
<p>L150-L155 – I would have liked to see a plot of the differences between the new map(s) and the pre-existing maps (e.g., by considering their mean values).</p>	<p>Thank you for this comment. We added these plots in Figure 4, and we updated the discussion accordingly (Section 4).</p>
<p>L158 – I can't see the rings very well in Figure 2, because of the earthquakes plotted on top of it. Please delete them.</p>	<p>We modified Figure 3, removing earthquakes and changing the background, in order to enhance the vision of the probability values. The rings are also now very evident in Fig. 3 and 4, when the model developed here is compared with previous studies.</p>
<p>L164 – Figure 3: I find this Figure a bit confusing. These are all Figures published in other papers, and I can't do a quantitative comparison of these heterogeneous data to the new maps (which are also plotted elsewhere, in Figure 2). Also, the data in plots d-e-f are not about vent opening information. Why mix apples with pears?</p>	<p>We modified this figure. The first figures are now about the differences between alternative maps, as suggested above, extending to Alberico et al. 2002 (following the request of reviewer 1).</p> <p>We then added two selected maps that are useful for the discussion, and enable a more quantitative comparison between our vent opening models and the distribution of the seismicity and the caldera ring faults.</p>
<p>L174-175 – “striking correspondence” is a qualitative claim. Please make it more quantitative. In particular, please explain how a comparison between your vent opening map and seismic pattern should be read. In general, main active faults are certainly correlated to seismicity, and main active faults are also hypothesized to be correlated to vent locations in several of the pre-existing vent opening maps. What is really the point of this sentence? Is it pointing at</p>	<p>With striking we were meaning “unexpected”, but we agree that was misleading. Now, we reformulated this part (line 241), to make it more clear. We also modified Fig. 3H,I, in order to facilitate a quantitative comparison.</p> <p>As for the interpretation, we think that whatever interpretation is very speculative at this point. The interesting thing is this correspondence, that may be better understood with future studies, as now specified (line 245).</p>

the possibility of new vent opening models also integrating seismicity? Or is it somewhat trying validate the current vent opening maps by using geophysical data?