We thank the reviewer for this revision and the interesting suggestions provided. We essentially 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 3), 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 (new section 4). We significantly extended the discussion, 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 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 2.2, 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 2), 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 highlighted in the discussion, our model "... *may provide the possibility to better constrain the epistemic uncertainty about vent opening probability. Indeed, several case studies recently demonstrated that the effective epistemic uncertainty on a target physical process (here vent opening) is better estimated by combining alternative approaches than by exploring the epistemic uncertainty inherent to individual models (Selva et al. 2015; Marzocchi et al. 2021; Meletti et al. 2021, among the others), defining weighted ensembles of existing models (SSHAC 1997, Marzocchi et al. 2017). This quantification may be the topic for future works, and the approach presented here may be a significant added value to this end, by providing an effectively alternative approach to vent opening probability quantification." (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 future epistemic uncertainty quantification. 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, as the last point of the manuscript.

Overall, we thank the reviewer for the important comments that gave us the opportunity to significantly improve the manuscript. All these improvements are specifically discussed in the attached file.

The manuscript describes an interesting approach to define a vent opening map at Campi Flegrei caldera	Thank you for this revision and the interesting suggestions provided. We essentially implemented all the suggested analyses, significantly improving the manuscript. We significantly extended the

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.	text to describe the new analyses, updating all figures and also adding 3 supplementary figures. We also significantly extended the discussion, as suggested. Regarding the independence between radial and azimuthal components, we tested the independence in existing data (Section 2.2, Supplementary Figure 5), confirming the independence that was earlier assumed only based on physical considerations (see comments below for more details). We now added the description of the test and an additional supplementary figure to show the results.
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	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. 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. 3), by reporting

between past vent opening positions and geophysical unrest patterns, in the discussion. 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.	the suggested analysis of the difference with the previous methods (we also added the comparison with Alberti et al., as suggested by reviewer 1), and by producing specific maps to compare earthquakes and structural features.
In summary, the results are incomplete and the discussion section is 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.	We significantly improved the discussion section, adding most of the reviewer's suggestions. We added new 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.3). 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, our model "... *may provide the possibility to better constrain the epistemic uncertainty about vent opening probability. Indeed, several case studies recently demonstrated that the effective epistemic uncertainty on a target physical process (here vent opening) is better estimated by combining alternative approaches than by exploring the epistemic uncertainty inherent to individual models (Selva et al. 2015; Marzocchi et al. 2021; Meletti et al. 2021, among the others), defining weighted ensembles of existing models (SSHAC 1997, Marzocchi et al. 2017). This quantification may be the topic for future works, and the approach presented here may be a significant added*

	value to this end, by providing an effectively alternative approach to vent opening probability quantification.”. 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 future epistemic uncertainty quantification. However, in this manuscript we prefer avoiding the additional complication of quantifying an epistemic uncertainty on the 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, as the last point of the manuscript.
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 started from the caldera center.	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.
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.	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". Thanks.
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.	We modified accordingly
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.	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). However, we did not test it explicitly, and we

	now added this test (Section 2.2). The results (discussed in Section 2.2 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.
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.	We modified the figure accordingly, 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.
L142 – The result section is unfortunately incomplete. Some examples of required additions are listed below.	Thank you for the suggestions. We tried to improve this section, also addressing all the following points.
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.	We now report the bivariate plot, along with the results of the independence test discussed above, in supplementary figure 5.

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.	We discharged half of the events simply because we demonstrate that Epochs 1 and 2 are significantly different from Epoch 3. Thus, their introduction may introduce a bias. This is now explicitly commented on in Section 3, just after equation 2. 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 at the beginning of section 4.
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.	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.
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?	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, Fig. 1, and in Supplementary Figure 6.
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?	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 just above Fig 3.
6. Could you try quantifying the uncertainty affecting all this? You may do that as you wish, but you	The uncertainty on vent position is treated in all tests. Regarding the epistemic uncertainty on the vent opening probability, as we now comment on the paper (last part

should not totally oversee that step.	of the discussion): *"Overall, the maps proposed here are based on assumptions radically different from the ones discussed in literature. This may provide the possibility to better constrain the epistemic uncertainty about vent opening probability by producing an ensemble of alternative models, in which all models consistent with data are combined to provide an ensemble model (SSHAC 1997, Marzocchi et al. 2017). Several case studies recently demonstrated that the effective epistemic uncertainty on the target physical process (here vent opening) is better estimated by combining alternative approaches than by exploring the epistemic uncertainty inherent to individual models (Selva et al. 2015; Marzocchi et al. 2021; Meletti et al. 2021, among the others). Thus, uncertainty on vent opening probability in Campi Flegrei may be may estimated by defining an ensemble of all available models, including the ones produced here as well as all previous models available in literature, along with a proper quantification of potential credibility weights (SSHAC 1997; Marzocchi et al. 2015): this may be the topic for future works. This paper may be a significant added value in this sense by providing an effectively alternative approach to vent opening probability quantification."*
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).	Thank you for this comment. We added these plots in Figure 3, and we updated the discussion accordingly (Section 4).
L158 – I can't see the rings very well in Figure 2, because of the earthquakes plotted on top of it. Please delete them.	We modified Figure 2, 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, when the model developed here is compared with previous studies.
L164 – Figure 3: I find this Figure a bit confusing. These are all Figures	We modified this figure. The first figures are now about the differences between alternative maps, as suggested above,

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?	extending to Alberico et al. 2002. 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.
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 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?	Here, with striking we mean "unexpected". We now reformulated this sentence, along with all this discussion, to make it more clear. We also modified Fig. 3H,I, in order to facilitate a quantitative comparison. 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.