Answers point-to-point

September 2, 2025

1 Report of Reviewer A

The authors proposed integrating the widely applied temporal ETAS Model with measurements of strain rates to develop a short-term seismicity model. They presented an approach in which the background seismicity rate is modulated by an empirically-derived dynamic kernel. They demonstrated the effectiveness of their approach through their analysis of the seismicity in Campi Flegrei caldera. Integration of the temporal ETAS Model with strain rates is an excellent idea. However, the manuscript lacks a clear explanation of method, for instance the formulation of the empirical kernel estimation: what would be the scientific rationale for the scheme? How does it exactly support forecasting based on past measurements of strain rates? Additionally, some description about the data used by the authors would be also helpful. I believe that it would not be difficult for the authors to address these concerns. Below, I list a few comments and hope that they would be useful to the authors. For the notations in the comments, I am writing names of the symbols (e.g., tau) and am using "-" to indicate subscript.

Answer: We sincerely thank the reviewer for their constructive remarks and for recognizing the value of integrating strain rate information into short-term seismicity models. We answer point to point below.

Line 13: Although ETAS is well known to statistical seismologists, it would still be useful to provide the full form.

Answer: Done.

Line 20: Perhaps, "This finding . . . "

Answer: Done.

Line 29: Perhaps, "... generate strain that can . . . ". Seismicity modulated by deformation signals seems awkward. Authors can de-

cide.

Answer: Done.

Line 35: " . . . the deformation and the seismicity . . . ." .. perhaps, as this observation is for the Campi Flegrei seismicity.

Answer: We thank the reviewer for this helpful remark. We have revised the sentence to make it clearer that the observation refers specifically to Campi Flegrei and now it reads: "A notable feature of this unrest is the increase in both deformation and seismicity rates in Campi Flegrei during the last decade...".

Line 65: A quick explanation of the Md scale would be useful.

Answer: We added in the text the following statement: "The Md magnitude, or duration magnitude, is a scale used to measure the size of smaller, local earthquakes; it is the standard in the Campi Flegrei seismic catalog "

Line 67-68: (Lilliefors test, Herrmann and Marzocchi, 2021).

Answer: Done.

Line 69: Zhuang et al. (2017)'s method.

Answer: Done.

Line 72: "arrival times" - would it be "occurrence time"?

Answer: We thank the reviewer for pointing this out. We agree with the suggestion and have replaced "arrival times" with "occurrence time" in the revised manuscript.

Figure 1B: There is a need to significantly improve the readability of this figure. Additionally, please confirm that the strain rate is in m/year. In the caption, instead of "Panel A" and "Panel B", one may just write "(A)" and "(B)".

Answer: We thank the reviewer for this helpful suggestion. We have revised Figure 1 to improve its readability: earthquake magnitudes are now more clearly displayed and the overall layout has been redesigned. In the caption we have also adopted the notation "(A), (B), ..." instead of "Panel A, Panel B, ..." for consistency. Regarding the deformation data, we clarify that Figure 1D reports cumulative vertical displacement (in meters) measured at the RITE GNSS station.

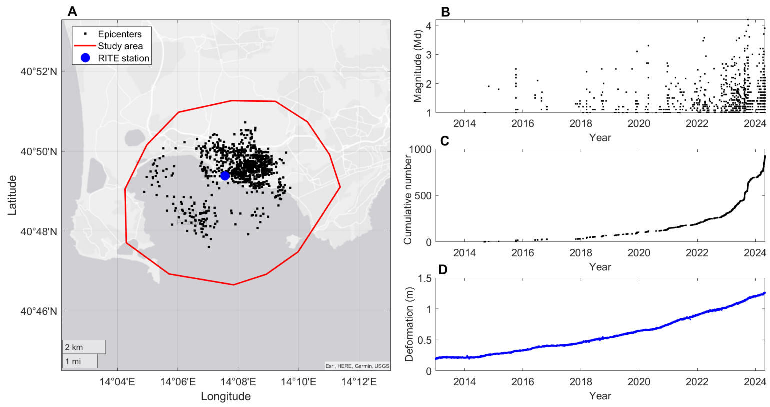

Figure 1: **Seismicity and deformation data at Campi Flegrei caldera (2013–2024). (A) Map of the study area (red polygon) showing the epicenters of earthquakes (black dots) and the GNSS RITE station used in this study (blue circle). (B) Time series of earthquake magnitudes ($M_d \geq 1$) within the study area. (C) Cumulative number of events over the same period, highlighting the acceleration of seismicity after 2020. (D) Vertical ground deformation (in meters) recorded at the RITE GNSS station, expressed as cumulative uplift relative to 2013, showing a continuous increase.**

Figure 1B: The time here is in years - not exactly clear whether the measurements are given for each month or day? Do the authors have measurements for each day? If not, how did they manage to compute the measurements for each day?

Answer: Yes, our GNSS measurements are one per day; we added "daily" in the text to clarify this point.

How did the authors account for (or take care of) the spatial (not just temporal) variation of these measurements?

Answer: All the previous studies on the Campi Flegrei deformation vs seismicity used only the RITE station, since it is placed in the center of the caldera, and it shows the maximum deformation. At the other stations, the deformation is smaller, although it remains coherent (correlated) with the RITE station. Thus, in this work, as in the other previous studies, we only used data from RITE station. See Fig.(2) of this document, from De Martino et al. 2021;

Line 80: "inset" or "intercept"?

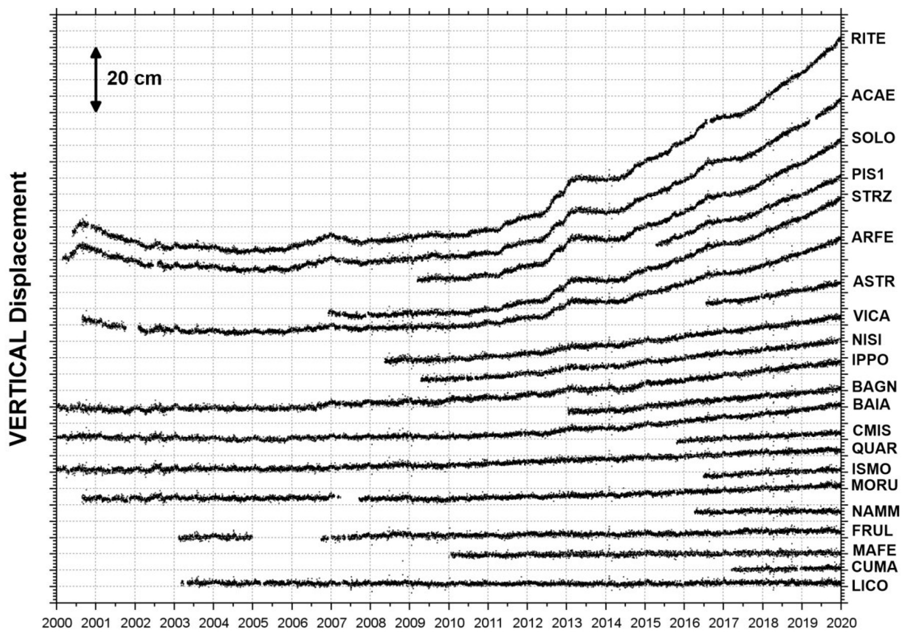

Figure 2: Deformation at different GNSS stations in the Campi Flegrei caldera

Answer: We thank the reviewer for noticing the issue, we meant ”onset”. We fixed the typo in the revised version of the manuscript.

Line 93: Perhaps, “speculate” instead of “infer”

Answer: We thank the reviewer for the comment. We changed ”infer” to ”speculate” in the revised version of the manuscript.

Equation 2: Would s be a time variable (units in days)?

Answer: We thank the reviewer for this clarification request. Yes, s represents a time variable, expressed in units of days. We have clarified this point in the revised manuscript by explicitly stating that s is a time variable (in days).

Equations 2 and 4: Would not it be dt in Equation 2? Also, would it not be important to have a baseline of the background rate (in Equation 1) and fluctuations introduced in these equations?

Answer: We thank the reviewer for this comment. In Eq.(2), the integration is indeed carried out over the lag variable s, which represents the time delay between deformation and its effect on seismicity. Therefore, the correct notation is ds, not dt. To avoid confusion, we have clarified in the revised manuscript that s is the integration variable (in days). Regarding the baseline of the background rate, this is provided by the scaling parameter μ_0. In our formulation, μ_0 plays the role of the baseline stationary rate, while the convolution with the kernel introduces fluctuations around this baseline as a function of the strain-rate history. We have emphasized this point in the revised text, noting that $\mu_{\text{eff}}(t) = \mu_0 \cdot \mu(t)$ naturally recovers a constant baseline when $\mu(t)$ is flat. Changes introduced at lines 127-128.

Equation 3: Would τ_{max} and τ_{min} be more appropriate notations? In any case, there is a need to explain what these variables ($\min_\tau$ and $\max_\tau$) are.

Answer: We thank the reviewer for pointing out this notational ambiguity. In Eq.(3), $\min_\tau \rho(\tau)$ and $\max_\tau \rho(\tau)$ denote, respectively, the minimum and maximum values of the correlation function $\rho(\tau)$ across the entire range of time lags considered (0-40 days). To make this clearer, we have revised the notation and explanation in the text. Specifically, we now write:

$$G(\tau) = \frac{\rho(\tau) - \rho_{\min}}{\rho_{\max} - \rho_{\min}},$$

where $\rho_{\min} = \min_\tau \rho(\tau)$ and $\rho_{\max} = \max_\tau \rho(\tau)$. We have also updated Eq.(4) and the text to explicitly explain these quantities.

Line 102: Are the authors considering magnitudes in the Md scale as a proxy for seismic energy? If yes, how reliable and/or viable is that?

Answer: We thank the reviewer for this important observation. Yes, our analysis is based on the duration magnitude (M_d) reported in the INGV catalog. We acknowledge that M_d is not directly related to seismic moment in the same way as M_w, and that duration magnitudes may present biases with respect to other magnitude scales.
However, we employ them as a proxy for seismic energy by adopting the standard energy–magnitude scaling introduced by Hanks and Kanamori (1979), which is widely used in seismology. The reliability of using M_d in this framework is supported by previous studies. In particular, M_d was originally calibrated to be consistent with the local magnitude M_L (Lee et al., 1972), and it has been shown that M_L provides a good approximation of M_w in the small to moderate magnitude range ($M < 4.5$). Since our dataset only contains small events ($M < 4$), the use of M_d is therefore appropriate as a relative proxy for seismic energy release. We reformulate the text and changes can be found at lines 153-156.

Line 105, Equation 3: Is there a minimum number of events or data pairs for the correlation analysis? The estimation of the correlation

coefficient may not be well constrained (or statistically significant) if the number of events is not adequate.

Answer: We agree with the reviewer that the reliability of the correlation analysis depends on the number of available data pairs. In our implementation, we impose a minimum threshold of 10 pairs: if the number of valid samples in a given window is below this threshold, the correlation is not computed and the value is set to zero for consistency in the normalization. In addition, for each lag we report both the p-value of the Pearson correlation and 95% confidence intervals estimated using the Fisher r-to-z transformation, in order to assess the statistical significance of the results. This ensures that only correlations based on an adequate sample size are considered meaningful. We replaced the old figure 2 with a new version containing the related information.

Line 107: It'd be useful to mention that G(tau) is a distribution. Also, since G changes dynamically for a reference time, it may be a function of two parameters - tau and t_j

Answer: We thank the reviewer for this observation. In our study, $G(\tau)$ should be understood as an empirical response kernel, normalized between 0 and 1, rather than a probability distribution. It quantifies the average delayed effect of strain rate on seismicity. Also, we recognize that, in principle, one could define a time-dependent kernel $G(\tau, t_j)$, recalculated locally at each reference time t_j, in order to capture possible non-stationary changes in the deformation seismicity relationship. However, in this work we deliberately opted for a global kernel $G(\tau)$ estimated from the full dataset. This choice has two main advantages: (i) statistical robustness: by pooling the entire 11 year dataset, the kernel estimation is based on a larger number of pairs, which ensures stable and well constrained correlations. (ii) forecasting simplicity: a single stationary kernel allows us to directly integrate deformation into the ETAS framework without introducing additional indices or parameters, thereby avoiding potential overfitting and keeping the model comparable to the standard ETAS formulation. We agree that extending the analysis to time varying kernels $G(\tau, t_j)$ would be a natural and interesting future development, particularly to explore how the deformation seismicity coupling may evolve during different phases of unrest. We improved this point to explain better the kernel G is the revised version of the manuscript at lines 118-123.

Lines 120-121: Ambiguity: previously, G is a function of tau and here it is a function of t.

Answer: We thank the reviewer for pointing this out. This was a typographical error: throughout the manuscript G is consistently defined as a function of the lag τ, i.e. $G(\tau)$. We have corrected the notation in the revised version to avoid ambiguity.

Line 133: How did the authors obtain the empirical relation? This equation would need a reference. Is this relation valid for the Md scale? Also, the empirical relation needs numbering. In fact, several equations/relations in the manuscript need numbering.

Answer: We thank the reviewer for raising this important point. The relation between seismic energy and magnitude is the standard scaling introduced by Hanks and Kanamori (1979) in the definition of the moment magnitude scale. In their formulation, M_w is linearly related to the logarithm of the seismic moment, which in turn leads to the well known energy-magnitude scaling. In our case, the catalog reports magnitudes in the M_d scale. We note that M_d was originally calibrated to be consistent with M_L in the Southern California network (Lee et al., 1972), and that M_L is known to be a good proxy for M_w in the small to moderate magnitude range, with differences typically below 0.2 units. Since our dataset is limited to small events ($M < 4$), using M_d in the energy scaling law is therefore appropriate as a relative proxy for seismic energy release. In the revised manuscript we now number this equation, provide the appropriate references and explain better the relation utilized. Changes at lines 152-156.

Lines 135 - 137: I'm trying to understand. Now, t_j is in days. The deformation time series is in days; this is not apparent in Figure 1B. There is a need to explain why the range of [0, 40] days is used. I meant, is the maximum of 40 days taken arbitrarily? Likewise, why should the sliding window have a length of 30 days?

Answer: We thank the reviewer for pointing this out. We have revised Figure 1B to make explicit that both the seismic catalog and the deformation series since the beginning of the observation period improve clarity. The choice of the lag range [0,40] days was not arbitrary. From a physical perspective, in volcanic calderas the relevant processes that can link deformation and seismicity, such as pore pressure diffusion, magma and fluid migration, or viscoelastic stress redistribution, typically act over timescales of several days to a few weeks. A maximum lag of 40 days was therefore selected to cover this expected range of delayed response. From an empirical perspective, our correlation analysis shows that the deformation-seismicity correlation decays to negligible values beyond $\approx$ 40 days (see Figure 2), indicating that longer lags do not carry additional information and would only introduce noise. The 30-day sliding window was chosen to ensure statistical stability of the correlation estimates. Because our catalog contains only small magnitude earthquakes, shorter windows would often contain very few events, resulting in unstable correlation estimates. A 30 day window represents a compromise: it smooths short-term fluctuations while retaining the variability of the strain–seismicity coupling on a monthly scale. We have revised text to clarify these choices. Change at lines 165-180.

Line 138: The window to estimate the cumulative seismic energy, for tau = 0, would be [t_j-30, t_j], so this window is a lag of 30 days

from t_j. However, the lag time varies with increasing tau, and for $tau >= 30$, there is no lag time. This scheme seems odd. Authors are requested to explain why they adopted this windowing scheme.

Answer: We thank the reviewer for pointing this out. The adopted scheme indeed implies that for $\tau = 0$ the cumulative energy is evaluated in the interval $[t_j - 30, t_j]$, while for increasing values of τ the window progressively shifts forward and, for $\tau \geq 30$, it lies entirely after t_j. This was an intentional choice: our aim was to perform a cross-correlation analysis broad enough to capture possible delayed, synchronous, or even slightly anticipatory relationships between deformation and seismicity. In volcanic systems, strain changes and seismicity do not necessarily follow a strict causal sequence; instead, they may occur nearly simultaneously or with variable delay depending on the underlying physical mechanism (e.g., fluid migration, viscoelastic adjustment). By using a sliding 30 day window, we ensured that enough events were included for a stable correlation estimate, while allowing the lag parameter τ to explore the relative timing between the two signals. We now clarify this point in the manuscript. Changes at lines 173-180.

Line 139: It is not clear how the strain data was extracted. If it is just the strain data at each time point spanning 40 days, how will the author explain correlating the cumulative seismicity energy (for a time window) to the strain data (at a point in time)?

Answer: We thank the reviewer for raising this important point. In our analysis, the strain rate at a given day X is not taken as an instantaneous measurement, but rather as a monthly average. Specifically, we compute it as

$$\dot{\varepsilon}(X) = \frac{D(X) - D(X - \Delta t)}{\Delta t}, \tag{1}$$

which corresponds to the mean strain rate over the previous $\Delta t = 31$ days, subsequently expressed in units of m/year. This procedure ensures consistency when correlating with the cumulative seismic energy, which is also evaluated over a finite time window. We have clarified this definition explicitly in the revised manuscript at lines 101-105.

Line 141: The time shift from 0 to 40 days is understood. But, it is unclear how the entire scheme provides a 41-days discrete lag grid, as the applied window has a maximum of 30 days lag (see previous comment on Line 138). Also, would the sentence read "... kernel for a 41-day discrete lag grid."? Perhaps I am missing something here.

Answer: We thank the reviewer for raising this point. The 41 day discrete lag grid arises because we evaluate the correlation for integer lag values $\tau \in [0, 40]$, yielding 41 distinct correlation estimates. This is independent of the sliding window length 30 days, which is used to stabilize the estimate of cumulative seismic energy. In other words, the lag grid defines where the window is positioned relative to the deformation time series (from 0 to 40 days), while the 30 day window defines how much data is integrated at each lag. We agree that our wording was ambiguous and have revised the manuscript to clarify that the kernel is estimated on a "41-point discrete lag grid" spanning 0-40 days, with each correlation computed over a 30 day window of seismic energy.

Line 142: "... interpolated strain rates". How are they interpolated?

Answer: We thank the reviewer for raising this point. In this revised version, we replaced the word "interpolated" with "averaged" to better reflect our procedure. The strain rates are computed using a monthly (31-day) moving window to reduce noise in the daily GNSS measurements. Specifically,

$$\dot{\varepsilon}(t) = \frac{D(t) - D(t - \Delta t)}{\Delta t}, \tag{2}$$

where $D(t)$ is the deformation at time t, and $\Delta t = 31$ days in our case. This ensures consistency when correlating with the cumulative seismic energy, which is also estimated over a finite time window. We have clarified this explicitly in Section 3.1 of the revised manuscript.

Equation 6: I'm curious as to why lag=0 day was considered. How would that help in forecasting seismicity based on immediate but past deformation (observed strain rates)? To achieve one-day-ahead forecasting, would it not be necessary to have data input considering a lag of one day?

Answer: We agree with the reviewer. A lag of $\tau = 0$ cannot be used for one day ahead forecasting because it would require deformation data from the future. We included $\tau = 0$ only as a diagnostic reference to assess whether any instantaneous coupling exists between strain rate and seismicity. In the revised manuscript we clarify this point and provide the explicit forecasting form. Changes at lines 185-187.

Line 152: It would be useful to mention that μ_0 is a constant.

Answer: Done.

Line 155: Discrete values of what?

Answer: We thank the reviewer for pointing out this ambiguity. The "discrete values" refer to the background modulation $\mu(t)$, which we initially compute only at the discrete times of the seismic events. Since the likelihood evaluation requires $\mu(t)$ at arbitrary times, these discrete values are then interpolated with a spline function to obtain a continuous representation. We have clarified this in the revised manuscript.

Equation 8: Would the second term also be a logarithm of lambda(t)?

Answer: We thank the reviewer for the observation. The second term in (old) Equation (8) is not a logarithm, but the time integral of the conditional intensity $\lambda(t)$ over the observation window. This follows the standard log-likelihood formulation for point processes (Ogata, 1988).

Line 163: What is being referred to by "The last"?

Answer: We agree that the wording was unclear. We rewrite the sentence to make it more clear.

Line 174: Would not different initial guesses and/or multiple runs produce different results?

Answer: We thank the reviewer for the observation. Indeed, as the optimization relies on a local solver, different initial guesses could in principle affect the results. To test this, we repeated the estimation using multiple initial parameter sets and obtained consistent results across runs. This confirms that our estimates are robust with respect to the choice of initial conditions. We have clarified this point in the revised manuscript. Changes in lines 219-221.

Line 179: What would be the general range of the estimated correlation coefficient before normalization?

Answer: Before normalization, the empirical kernel is based on the Pearson correlation coefficient $\rho(\tau)$, whose values lie in the standard range $[-1, 1]$. To make this explicit, we computed $\rho(\tau)$ for $\tau \in [0, 40]$ days using 30-day backward windows and show the results with 95% Fisher confidence intervals and significance markers (see figure 3).
We also clarify this point in the main text by explicitly stating that the maximum correlation coefficient prior to normalization is about 0.6.

Figure 2: Would it be possible to illustrate the two cases of fast and slow decaying kernels (as described in Lines 121-122)? Also, it would be useful to know what exactly was the estimated maximum correlation coefficient

Answer: We thank the reviewer for this comment. The mention of fast and slow decaying kernels in Lines 121–122 was intended only as a general overview of the different functional forms that are commonly considered in the literature. In our study, however, we do not assume a specific functional form but instead adopt an empirical kernel derived from the correlation between strain rate and

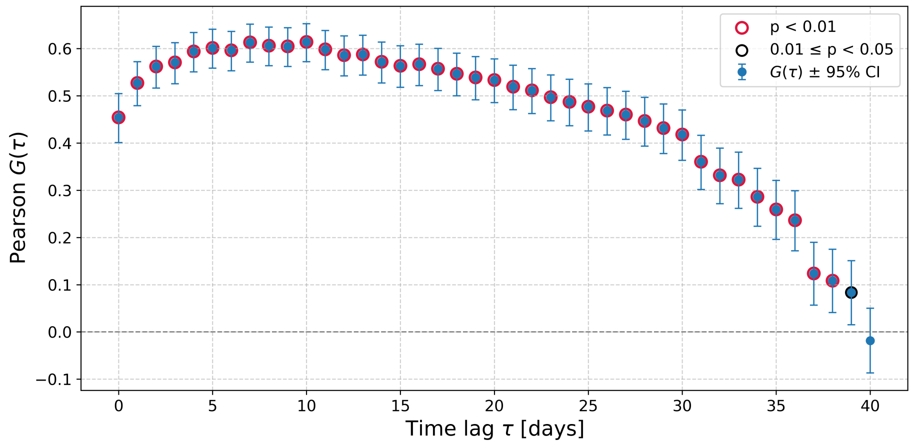

Figure 3: Pearson correlation coefficient $\rho(\tau)$ between monthly strain rate and cumulative seismic energy computed over 30-day backward windows, for integer lags $\tau \in [0, 40]$ days.

seismic energy release. For this reason, we did not illustrate the two hypothetical cases, as we cannot a priori discriminate which decay would be physically appropriate for Campi Flegrei.

Line 193/Table 1: AIC difference is not very significant and does not support the author's claim of significantly improving forecasting ability.

Answer: We agree with the reviewer that the difference in AIC is not extremely large. However, following the guidelines by Burnham and Anderson (2002), a ΔAIC of about 4 is already considered to provide considerable support for the better model. For this reason, we interpret the deformation dependent ETAS as better supported than the stationary version. We have revised the wording in the manuscript to avoid overstating the result, now referring to an "improved model fit" rather than a "significant improvement in forecasting ability."

Table 1 needs a discussion about the differences in the ETAS parameters between the stationary and deformation driven model.

Answer: We thank the reviewer for this suggestion. We agree that Table 1 would benefit from a brief discussion of the parameter differences. In the revised version we have added such a discussion, emphasizing that: (i) the stationary background rate μ_0 is constant, while in the deformation-driven model the effective background rate $\mu(t)$ varies according to the strain rate. The fitted μ_0 is accordingly smaller in the deformation driven case, since part of the background variability is absorbed by the modulation. The triggering parameters (K_0, α, c, p) remain of the same order of magnitude in both models, indicating that the short-term clustering properties are robust with respect to the background formulation. Small shifts in K_0 and p reflect the redistribution of seismicity between background and triggered components when the deformation term is introduced. This discussion has been included in the revised manuscript to clarify the implications of Table 1.

2 Report of Reviewer B

The submitted manuscript, authored by Giuseppe Petrillo and Matteo Taroni, presents the development of a non-stationary Epidemic-Type Aftershock-Sequence (ETAS) model informed by strain rate. The authors utilise the seismic catalog and Global Navigation Satellite System (GNSS) data between 2013-01-01 and 2024-04-30, associated with the volcanic activity around the Campi Flegrei caldera in southern Italy. These datasets are incorporated into the time-variable background rate ($\mu(t)$) of their non-stationary ETAS model. The performance of the proposed model is evaluated against the stationary ETAS model with a constant background rate, and the authors conclude that their non-stationary model exhibits improved forecasting ability. The authors' approach is valuable for investigating and assessing seismicity in an active volcanic region, where seismic activity can be modulated by or sensitive to volumetric changes possibly linked to processes such as fluid migration or other aseismic phenomena. Below, I outline some minor points that I believe would help improve the manuscript. In particular, I may encourage the authors to: (1) provide a clearer explanation and/or visualisation of the datasets (seismic catalog and GNSS data), and (2) discuss the potential impact of relying on a single GNSS station, especially regarding how the distance between that station and the evolving seismicity clusters may affect the estimation of the non-stationary background rate.

Answer: We thank the reviewer for the careful reading of our manuscript and for the constructive comments and suggestions. We appreciate the positive assessment of the relevance of our approach for understanding seismicity in active volcanic regions, as well as the encouragement to further clarify and expand some parts of the work. We address each of these points in detail below, and we have revised the manuscript accordingly.

 # Explanation of datasets - I believe that both the seismic catalog and the GNSS data could be described and visualized more rigorously, if possible: The location of the RITE station should be indicated on the map in Fig. 1.

Answer: In the revised version of the Figure 1, we show both seismic and deformation data in a better way. In particular, we also added the location of the RITE station.

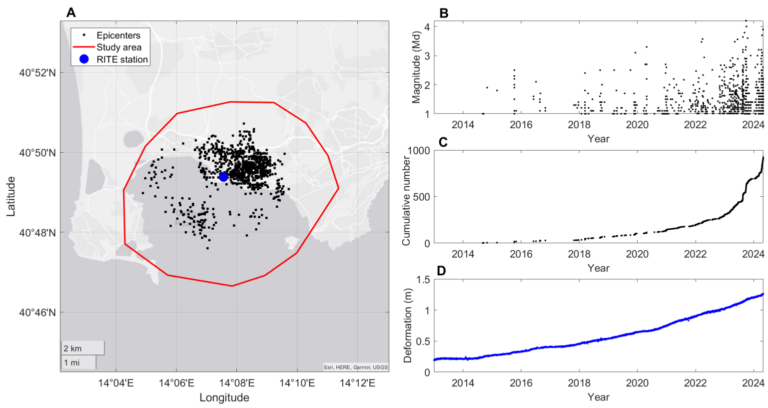

Figure 4: **Seismicity and deformation data at Campi Flegrei caldera (2013–2024). (A) Map of the study area (red polygon) showing the epicenters of earthquakes (black dots) and the GNSS RITE station used in this study (blue circle). (B) Time series of earthquake magnitudes ($M_d \geq 1$) within the study area. (C) Cumulative number of events over the same period, highlighting the acceleration of seismicity after 2020. (D) Vertical ground deformation (in meters) recorded at the RITE GNSS station, expressed as cumulative uplift relative to 2013, showing a continuous increase.**

- Overlaying the strain rate scatter plots (blue) on the Magnitude-Time plots of seismicity (red) is somewhat difficult to interpret for me. I would suggest separating the datasets into different panels; for example, panel B for seismicity and panel C for strain rate.

Answer: In the revised version of the Figure 1, we show both seismic and deformation data in a better way. In particular, we also separated the seismic and the deformation data.

- It would be helpful for readers if some additional information associated with the caldera and the study region were annotated (e.g., the outer rim of the caldera and an inset map showing regional context, similar to the examples in https://www.nature.com/articles/s41467-025-56723-y/figures/1).

Answer: In the revised version of the Figure 1, we show both seismic and deformation data in a better way. In particular, we also added the polygon of the study area.

- For the clearer visualisation of the seismic catalog (and also for the better comparison with the background rate), I would think that the figures like Time vs Magnitude, Time vs cumulative number of events, and/or Time vs background rate (intensity) can be informative for the readers. Please see an example Figure 2 in Kumazawa and Ogata (SRL, 2024, https://doi.org/10.1785/0220240166).

Answer: In the revised version of the Figure 1, we show both seismic and deformation data in a better way. In particular, we also added Time vs Magnitude and Time vs cumulative number of events plots.

- Could the authors provide a more rigorous explanation of how the strain rate is calculated from the GNSS velocities?

Answer: We thank the reviewer for this comment. The strain rate used in our analysis is derived directly from the GNSS displacement time series. In the revised manuscript (Section 3.1), we now provide a formal definition:

$$\dot{\varepsilon}(t) = \frac{D(t) - D(t - \Delta t)}{\Delta t}, \tag{3}$$

where $D(t)$ is the cumulative deformation measured at time t, and $\Delta t = 31$ days. This corresponds to a backward moving average over a monthly window, which reduces the short term noise in the daily GNSS measurements and yields a smoother strain-rate time series. The resulting values are expressed in meters per year.

- I appreciate the authors that the datasets have been made available via the Zenodo archive (I ran the Python script and confirmed that Figs. 2 and 3 were reproduced, which is truly beneficial for all readers). However, where can we access the seismic catalog itself (e.g., including longitude, latitude, and depth information) so that Fig. 1 can also be reproduced? Please include the information in the Data and Code availability section.

Answer: We added both seismic and deformation datasets to a publicly available repository.

- I would appreciate it if the authors could explain how we access the seismic catalog and GNSS data in the Data and Code availability section. (perhaps via De Martino et al., 2021 at https://www.mdpi.com/article/10.3390/rs13142725/s1 ?)

Answer: As explained in the previous reply, we added both seismic and deformation datasets to a publicly available repository.

 # Potential bias of using a single GNSS station - I understand that the RITE station is likely the nearest GNSS station available in this region (or in the centre of the caldera). However, I wonder whether using the strain rate from a single station can robustly represent the bulk crustal deformation around the caldera and the resultant background rates of seismicity. As shown in Fig. 1A, seismicity occurs in several clusters that are not continuously distributed along the caldera. This may imply that the distance between the GNSS station and a given seismicity cluster may vary over time? Also given the potential heterogeneity of the region (e.g., the caldera structure), could there be a bias in estimating the background rate when relying on a single GNSS station? (e.g., Kumazawa et al., 2016, EPSL, https://doi.org/10.1016/j.epsl.2016.02.049). I would appreciate it if the authors could elaborate on a potential bias or uncertainty regarding the formulation of relying on a single GNSS station.

Answer: We thank the reviewer for raising this important point. We agree that relying on a single GNSS station may in general introduce a bias, especially in regions where deformation is spatially heterogeneous or where seismicity clusters occur at varying distances from the station. In the case of Campi Flegrei, however, several geodetic and geophysical studies (e.g., De Natale et al., 2006) have shown that the deformation field during recent unrest episodes is largely dominated by an almost radially symmetric uplift centered beneath the caldera. The RITE station, being located close to the centre of deformation, has therefore been widely used in the literature as a representative proxy of the bulk deformation signal. See Fig.(5) of this document, from De Martino et al. 2021; We acknowledge, however, that spatial heterogeneity in the seismic clusters (as correctly noted by the reviewer) may affect the degree to which RITE alone captures localized deformation processes. We have now added a discussion in the manuscript to clarify that while our choice of RITE is justified by the radially symmetric deformation pattern, a multi-station or spatially distributed approach would be preferable to better account for possible local variations. This is an important perspective for future extensions of our work.

 # L30: the Campi Flegrei caldera ... has emerged as a key case study -¿ the Campi Flegrei caldera ... has emerged as a key case?

Answer: Done.

 # L51: GPS or InSAR − > Global Positioning System (GPS) or Interferometric Synthetic Aperture Radar (InSAR)

Answer: Done.

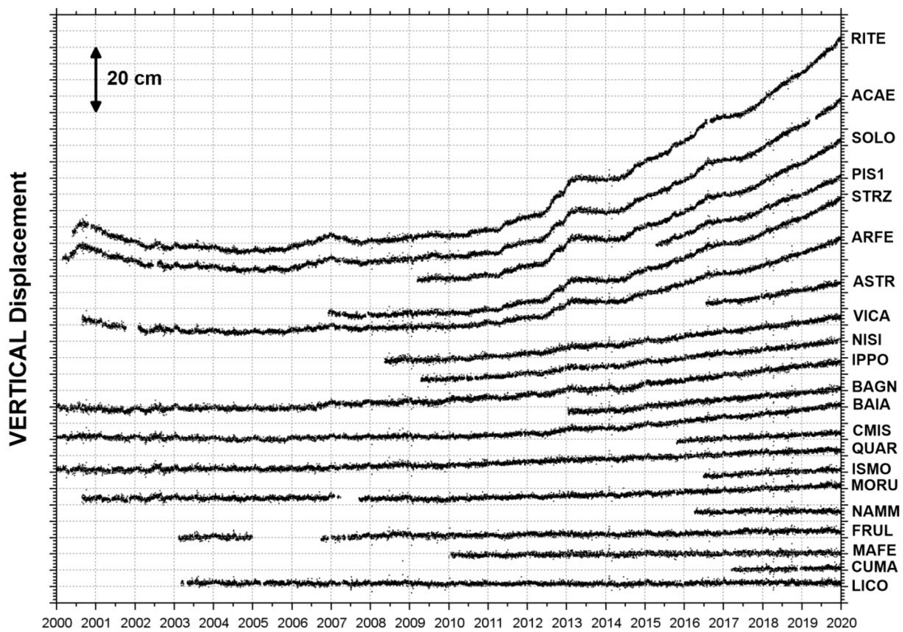

Figure 5: Deformation at different GNSS stations in the Campi Flegrei caldera

 # L62: Perhaps OEF can be explained briefly or at least be referred to some relevant literature, for example: Mizrahi, L. (2024), Operational Earthquake Forecasting – what is it and how is it done?, Eos, 105, https://doi.org/10.1029/2024EO245024.

Answer: We thank the reviewer for this useful suggestion. In the revised manuscript, we now briefly explain the concept of Operational Earthquake Forecasting and cite Mizrahi (2024).

 # L64: Please give a citation for the seismic catalog.

Answer: We added the quotation:
Ricciolino, P. e Lo Bascio, D. Cataloghi sismici dei vulcani campani. Stazione STH Campi Flegrei dal 2000 al 2021 (Cat-STH_2000_2021) (1.0) Data set. Istituto Nazionale di Geofisica e Vulcanologia. https://doi.org/10.13127/ovcatalogsth_2000_2021 (2021).

 # L66: maximum depth of 4 km - Is it possible to plot the seismicity coloured by depth in Fig. 1A?

Answer: As shown in a previous reply, Figure 1 has now changed, to better

show both seismic and deformation data.
Since in our work the depth of events is not important (it is a temporal ETAS model, without considering spatial or depth variations), we prefer not to show too many things in the same figure.

 # L74: Epidemic Type Aftershock Sequence (ETAS) -¿ ETAS (*as ETAS is already defined in the Introduction section.)

Answer: Done.

 # L105: The way of calculation of E(t) can be described here. (or at least the equation of L134 should be referred to here.)

Answer: We thank the reviewer for the suggestion. In the revised manuscript, we now explicitly mention Equation for the energy when introducing E(t), so that the definition is immediately clear. However, we prefer to keep the detailed derivation in the later section, where we present the algorithm in a unified and systematic way.

 # L118–126: I would feel that the contents in these two paragraphs can be moved in the Discussion section by citing the necessary references.

Answer: We thank the reviewer for this helpful suggestion. We agree that the content in (old) Lines 118–126 is more interpretative than methodological, and thus better suited for the Discussion section. In the revised manuscript, we have moved this paragraph to the Discussion and Conclusion section, immediately after the description of the model limitations.

 # L132: How is this empirical relation derived?

Answer: We thank the reviewer for pointing this out. The empirical relation used to compute the seismic energy, $E \propto 10^{1.5M}$, is the standard relation introduced by Hanks and Kanamori (1979). This is widely adopted in seismology to convert magnitude into radiated seismic energy. In our case, we use M_d as a proxy for magnitude, which is appropriate given the low magnitude range of Campi Flegrei seismicity ($M < 4$). As discussed in Lee (1972), M_d and M_L are highly consistent for small magnitudes, and M_L in turn is a good proxy for M_w in this range. We have clarified this in the revised manuscript and added the appropriate references.

 # Please assign the equation number to the following: L134 L165 L167

Answer: Done.

L168: Aj corresponds to L165?**

Answer: Yes, we have replaced the expression "above" with the equation number just introduced.

 # L181: τ^* seems not defined.

Answer: We thank the reviewer for pointing this out. The symbol τ^* was unintentionally introduced without a proper definition and did not add any additional meaning beyond τ. We have therefore removed the asterisk throughout the manuscript to avoid confusion.

 # L190: I understand the section 3.2 is dedicated for the numerical implementation and the equations there are no necessarily the same as those in the application here, but I would appreciate it if the expression can be a bit more consistently used, for example, $\tilde{\mu}(t)$ at L190 is Eq.(6)? $\mu(t)$ at L190 is μ_{eff}?

Answer: We thank the reviewer for this helpful remark. We acknowledge that the notation was inconsistent in the original submission. To avoid unnecessary symbols, we have simplified the notation throughout the manuscript: we now use $\mu(t)$ consistently to denote the normalized background rate derived from the strain-kernel convolution, and the effective background term entering the conditional intensity is $\mu_0\mu(t)$. This resolves the ambiguity between $\tilde{\mu}(t)$ and $\mu_{\text{eff}}(t)$.

 # L183: How can this 10-day be supported by any evidence? (I would like to note that I am okay if the authors explicitly mention that this 10-day delay is just an empirical value and avoid possible over-interpretation about it.)

Answer: We thank the reviewer for this useful remark. The 10 day delay is not imposed a priori but rather emerges as an empirical result of the Campi Flegrei dataset, corresponding to the maximum of the estimated correlation function $G(\tau)$. We have clarified this point in the revised manuscript to avoid possible misunderstanding. In the Discussion, we also note that while this empirical lag could be qualitatively consistent with processes such as viscoelastic relaxation or fluid migration often invoked in volcanic environments, this interpretation should be considered only as a working hypothesis rather than direct evidence of a specific physical mechanism.

 # L198–200: These sentences look like duplicates of the corresponding figure caption.

Answer: We thank the reviewer for this observation. We agree that the original sentences overlapped with the figure caption. In the revised manuscript, we have rephrased this part to avoid duplication.

 # L199: Blue curve, not shown?

Answer: We thank the reviewer for pointing this out. The reference to the ”blue curve” was a leftover from an earlier version of the figure. In the revised manuscript, we have removed this sentence to maintain consistency between text and figure.

Answers point-to-point

September 8, 2025

1 Report of Editor

I have evaluated the revised manuscript as well as the authors' responses to the reviewers. Prior to making a final decision, I would appreciate it if the authors could address the following points.

Answer: We thank the editor for the careful re-evaluation of our revised manuscript. Below we address each of the additional points raised.

strain signals that can $->$ strain that can (I assume this is what Reviewer A has meant.)

Answer: Done.

The authors answered as "We added both seismic and deformation datasets to a publicly available repository." (page 14 of 'answer point-to-point.pdf'), but it seems the zenodo repository (https://doi.org/10.5281/zenodo.16667421) has not been updated? I would rather suggest that the GNSS data and the seismic catalog can be cited with DOIs in the Data and Code availability section if these are publicly available. For example, "GNSS dataset is available at XXX. The seismic catalog is from Ricciolino et al. (2021). "

Answer: We apologize for the confusion created by our previous response. The seismic and deformation datasets were already included in the Zenodo repository at the time of our first revision, and no further update was required. To avoid any ambiguity, in the revised manuscript we have clarified this point in the Data and Code Availability section and explicitly cited the corresponding sources: The seismic catalog is from Ricciolino et al. (2021).
For clarity, please see Fig.(1) (screenshot provided) which shows the link as it appears in the Data and Code Availability section, pointing to the Zenodo repository https://doi.org/10.5281/zenodo.16667421, where both seismic and deformation datasets are archived.

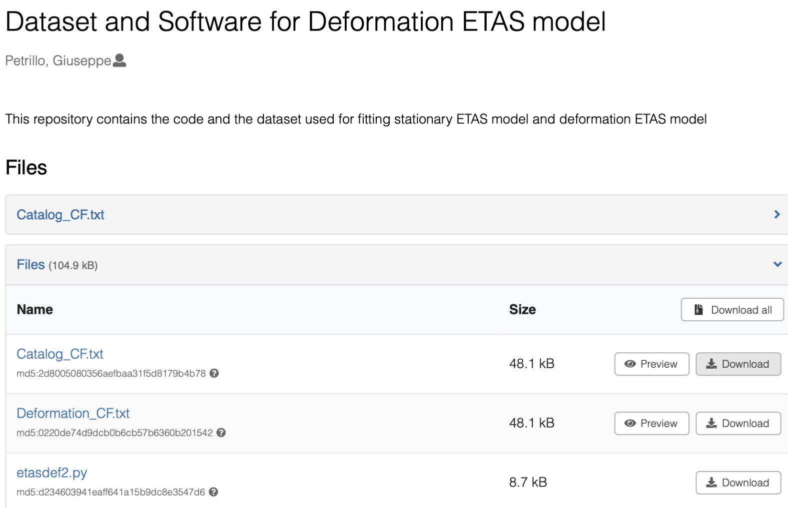

Figure 1: Caption

The authors answered as "We added the quotation" (page 16 of 'answer point-to-point.pdf'), but I am missing the quotation in the revised manuscript. Please add this citation; Ricciolino et al. (2021) in the revised manuscript.

Answer: We have added the correct quotation on the Data and Code Availability.

In the decision letter sent to the authors, the editor was asking; "I would like to ask [...], so that the authors could clearly convey that the present study is preliminary, which should be strengthened in the Introduction and Conclusion sections as well." May I possibly ask the authors how or where they have addressed these comments in the revised manuscript? I would appreciate it if the authors could revise the manuscript accordingly if they would deem it appropriate.

Answer: We thank the Editor for this important remark. In the revised manuscript, we have clarified more explicitly that our study represents a preliminary exploration of deformation-driven ETAS models in volcanic environments. Changes implemented at the end Introduction and Conclusions.