Answers point-to-point - R2

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- Reviewer comments are in **bold**.
- Authors answers are in red.

Report of Reviewer A

1. I appreciate the authors for making substantial improvements on the manuscript with regards to the comments by both reviewers. I do believe that the manuscript is now suitable for publication in current shape. In particular, the overall figure quality has improved and I think new Figure 3 that compares the swarm-informed model with the other two reference models clearly and intuitively demonstrates the ability to better capture the temporal dynamics of seismicity. I thank the authors for conducting this additional analysis.

Answer: We thank the reviewer again for the constructive feedback provided throughout the review process, which has helped us improve the clarity and overall quality of the manuscript.

2. I have one minor suggestion which the authors may consider further: The authors added a short description of non-stationary ETAS model fitting procedure (lines 281 – 292). For the sake of reproducibility, I hope the authors could add a little more details about this procedure, regarding how they selected the smoothing strength and what is the ABIC value.

Answer: We thank the reviewer for this helpful suggestion. In this study, we implemented a non-stationary ETAS model within the Ogata-type framework, in which both the background rate and the productivity are allowed to vary smoothly in time. Specifically, we parameterized $\mu(t) = \mu_{\mathrm{ref}}\, q_\mu(t), \qquad K_0(t) = K_{0,\mathrm{ref}}\, q_{K_0}(t)$, with $q_\mu(t) = \exp[s_\mu(t)]$ and $q_{K_0}(t) = \exp[s_{K_0}(t)]$, where $s_\mu(t)$ and $s_{K_0}(t)$ are represented by cubic splines. This log-spline formulation ensures positivity of both functions while allowing flexible temporal variability. The spline functions were defined using uniformly spaced knots over the observation window (8 knots for each function in the implementation used here). No change-point was introduced in the analysis shown in the manuscript. Parameter estimation was performed by maximizing a penalized log-likelihood function. The penalty term controls the smoothness of the temporal variations and is defined as the sum of squared first differences of the spline coefficients, following a standard roughness regularization approach. The smoothing strength w was selected by minimizing the Akaike Bayesian Information Criterion (ABIC). In practice, we explored a discrete set of candidate values $w \in \{0.0, 0.01, 0.05, 0.1, 0.3, 1.0\}$, and for each value we performed a full likelihood optimization. The ABIC was computed as $\mathrm{ABIC} = 2\,\mathcal{L}_{\mathrm{pen}} + 2\,n_{\mathrm{hyper}}$, where $\mathcal{L}_{\mathrm{pen}}$ is the minimized penalized negative log-likelihood and $n_{\mathrm{hyper}} = 1$ is the number of hyperparameters (i.e., the smoothing weight w). The optimal model corresponds to the value of w that minimizes the ABIC. In our case, the optimal ABIC value is 520.30. The nonstationary ETAS model remains a highly flexible time-dependent formulation characterized by a large number of spline coefficients. Therefore, its effective model complexity is not directly comparable to that of the stationary or swarm-informed ETAS models, and a standard AIC-based comparison is not appropriate. We have added a concise description of this procedure in the revised manuscript to improve clarity and reproducibility.

Report of Reviewer B

1. Thank you for this revised manuscript, which effectively addresses the comments raised on the initial draft. The revisions to the overall context and the descriptions of the figures have been considered and improve the clarity and readability of the paper. I also appreciate the modifications made in response to the second reviewer's comments on the ETAS methodology section; these additions enhance clarity and better highlight the value of the swarm-informed kernel. With this additional information comes some new questions on the way results have been interpreted in terms of swarm-like behavior and underlying physical processes. In particular, it would help the reader to clarify how swarm-like sequences are defined in their dataset. From the figures presented in the paper I am also not entirely convinced on some interpretations on the relationship between swarm-like occurrence rate and b-value variations and thus on the physical interpretations made from these results even though these interpretations are coherent with the overall context of volcanic activity. I would thus argue on another round of minor revisions upon acceptance of the paper. I find the proposed methodology for analyzing the area very interesting and innovative. I hope these new comments will help highlight the results obtained, making it easier to convince readers of your interpretations.

Answer: We sincerely thank the reviewer for the careful reading of the revised manuscript and for the positive assessment of the substantial improvements made with respect to the initial version. We are grateful for the reviewer's appreciation of both the revised contextual framing and the clarifications added to the ETAS methodology section. We also appreciate the reviewer's thoughtful new comments regarding the interpretation of swarm-like behavior, the relationship between occurrence rate and b-value variations, and the associated physical implications. These are important points, and we agree that further clarification can help strengthen the manuscript and make our interpretations more convincing to the reader. Below, we respond point by point to each comment and explain how we have revised the manuscript accordingly.

2. ETAS: In the description of the swarm-informed ETAS kernel it is not entirely clear to me whether replacing the Omori-type kernel with a swarm-informed kernel truly enables the isolation and characterization of a "seismic swarm occurrence rate" (name of the third section). I guess this formulation still captures a composite seismicity rate that includes contributions from the background rate, mainshock–aftershock sequences, and swarm-related activity. This should be clarified in the description of the formula (5). In addition, the parameters introduced in the swarm-informed kernel would benefit from further clarification, particularly for readers who are not familiar with the approach proposed by Godano (2023). Providing plots of the corresponding functions, or illustrative examples, would greatly help the reader understand Equation (4) and better appreciate how the parameters of the classical ETAS kernel differ from those of the swarm-informed kernel in practice. Even though this could be explained in the methodology, I appreciate that it is now discussed in the results section and in the description of Figure 2. However, I still think the interpretation in terms of swarm-rate occurrence is not enough supported by the results of Figure 2. How is a swarm defined within this framework ?

Answer: We thank the reviewer for this important comment. We agree that the previous wording could give the impression that the proposed ETAS formulation directly isolates a "seismic swarm occurrence rate" from the catalog, whereas this is not the precise meaning of the model. Our intention was not to claim that Eq. (5) separates swarm activity from background seismicity in a strict classification sense. Rather, the proposed formulation defines the conditional seismicity

rate for the analyzed catalog using a triggering kernel specifically designed to reproduce temporal patterns that are characteristic of swarm-like volcanic sequences, namely rapid activation, finite-duration memory, and a tapered decay (exactly same way how Omori kernel acts for standard ETAS model: the Omori kernel is not a declustering procedure). Probably a section title "swarm-informed ETAS" is more accurate than "swarm occurrence rate" in a strict sense, and we have revised the text accordingly.
The motivation for the kernel comes from Godano et al. (2023), where an average swarm occurrence rate was obtained after identifying swarm episodes through space-time clustering criteria and then stacking the selected sequences. That study showed that the average swarm rate is well described by a function of the form $\nu(t) \propto (t^{-p} + \mu)e^{-t/\tau}$, where the short-time decay is controlled by p, the finite duration by τ, and the additional μ term accounts for an approximately stationary component internal to the swarm sequence. In our work we use this empirically and physically motivated temporal form as the triggering kernel of a point-process model, because the TVZ catalog is dominated by repeated swarm-like sequences rather than by classical mainshock-aftershock behavior.
We agree that this distinction should be made clearer in the manuscript. We have therefore revised the title and opening paragraphs of Section 3 to clarify that the model describes the conditional seismicity rate of the catalog through a swarm-informed kernel, rather than isolating a pure swarm-only rate. We have also expanded the explanation of the kernel parameters to better distinguish them from those of the classical Omori-type ETAS kernel: p controls the short-time clustering, τ introduces a finite memory timescale absent in classical ETAS, and μ is part of the triggering kernel itself and should not be interpreted as an independent background rate. Finally, we agree that the definition of "swarm" should be stated more explicitly in our framework. In the revised manuscript we now clarify that "swarm-like" refers here to temporal clustering patterns characterized by the absence of a dominant mainshock, repeated short-lived rate transients, and finite-duration activation, consistent with the phenomenology of volcanic swarms described in the literature. We avoid implying that individual swarm episodes are explicitly identified by the ETAS inversion itself.

3. Figure 1: I appreciate that the authors followed my earlier suggestion by replacing the raw earthquake data with an earthquake density map in Figure 1, which was intended to improve readability and better highlight spatial patterns. However, I am not fully convinced that this modification achieves its intended goal. In particular, I had expected this approach to make regions of higher seismicity more apparent and thereby help identify potential swarm activity. Instead, the current representation appears to be overly smoothed, which tends to obscure local variations in seismicity and makes the identification of swarms difficult. The presence of several swarms related to specific volcanic activities remain unclear in Figure 1, which raises further questions about how swarm activity is identified and subsequently interpreted in the analysis.

Answer: We thank the reviewer for this comment. We agree that the current density representation in Figure 1 may be overly smoothed, which can obscure local variations in seismicity and make the visual identification of localized clusters less clear than intended. We would like to clarify that the primary purpose of Figure 1 is to provide a regional overview of the spatial distribution of seismicity within the Taupō Volcanic Zone, rather than to explicitly identify individual swarm sequences. In our analysis, swarm-like behavior is not defined based on visual inspection of spatial density maps. We tried different smoothing levels and color palettes, but the swarm distribution is not visible by eyes, but can be easily recognizable in Fig. 2.

4. Figure 2: The current x-axis is now well suited for comparing the results with Figure 7. However, it would be helpful to also indicate the corresponding calendar dates, particularly during the period of volcanic unrest. Including real dates (e.g., as a secondary axis or annotations) would provide additional temporal context and help the reader better relate the results to the broader sequence of events. I would also suggest duplicating the variations of seismic rate with the variations of b-value to help the reader really capture their correlation

Answer: We thank the reviewer for this constructive suggestion. In the revised version, we have modified the time axis to be expressed in decimal years, which provides a direct correspondence with calendar time and allows the reader to more easily relate the temporal evolution of the seismicity to the sequence of volcanic unrest. This representation avoids introducing an additional axis while still preserving a clear link to real dates. Following the reviewer's suggestion, we have also revised Figure 7 to better highlight the relationship between seismic rate and magnitude statistics. In particular, we now show the temporal evolution of the b-value together with the estimated seismic occurrence rate $\lambda(t)$ on the same time axis (Fig. 7a). This allows a direct visual comparison between the two quantities. We note, however, that the purpose of this representation is to illustrate a temporal co-variation rather than to imply a strict or systematic correlation. The revised figure aims to provide a clearer qualitative assessment of how changes in seismic rate relate to fluctuations in the b-value during periods of volcanic unrest.

5. l.266: "Overall, the temporal evolution of the seismic rate provides a coherent framework to interpret the spatial variations of the b value discussed in the following sections." I don't understand this sentence, this could be clarified for the reader. I guess this is because of the relation between seismic rate to specific known volcanic activities that you want to highlight in space and time with b-value analysis.

Answer: We thank the reviewer for this comment. Our intention was not to imply a direct or causal relationship between seismic rate and the b-value, but rather to use the temporal evolution of the seismic rate as a way to identify distinct phases of seismic activity (e.g., periods of enhanced clustering or swarm-like behavior). These phases can then be associated with specific regions within the Taupō Volcanic Zone, allowing for a more structured interpretation of the spatial variations of the b-value discussed in the following sections. We have revised the sentence to clarify that the seismic rate is used as a temporal reference to distinguish different activity regimes, which are then analyzed spatially in terms of their magnitude statistics.

6. l.273 : The year for the Tongariro eruption is missing.

Answer: Done.

7. l.277: Could you please prefer real dates and maybe add the number of days in parenthesis? I would also prefer as a reader having the real dates of eruptions written in Figure2 above the names of eruptions.

Answer: Done

8. l.364: "These b value variations are temporally coherent with the seismic occurrence rate" I think this is not very clear with the comparison between Figure7 and Figure2 even though the axes are now similar. You should maybe provide a figure combining both informations or show the difference

Answer: As already addressed above, we have revised the figures to explicitly combine the temporal evolution of the b-value and the seismic occurrence rate $\lambda(t)$ within the same panel (Fig. 7a). This modification allows for a direct visual comparison between the two quantities and resolves the difficulty noted in the previous version, where they were presented separately. In addition, we have clarified the corresponding statement in the manuscript. Rather than implying a strict correspondence, we now refer to a temporal co-variation between the two quantities, which reflects their concurrent variability during periods of changing seismic activity, without assuming a direct or systematic relationship. Change in the text: "the temporal evolution of the b-value shows a co-variation with the seismic occurrence rate, with periods of increased seismic activity often accompanied by noticeable changes in the magnitude distribution."

9. Figure 6.a and Figure 8.b have the same caption but don't represent the same

thing (temporal/spatial), can you add the information on the figure caption.

Answer: We thank the reviewer for pointing out this ambiguity. We agree that the previous captions of Fig. 6 and Fig. 8 were not sufficiently explicit and could lead to confusion. In the revised manuscript, we have rewritten both captions in the revised version of the manuscript.

10. l.408-413 : I really appreciate this new paragraph, which clarifies the methodology adopted overall and demonstrates the impact of the analysis on volcanic activity forecasting. I think this interpretative framework is correct, but it could be supported by more bibliography. I also feel that the figures/results provided in the manuscript at this stage do not provide sufficient and quantified evidence to convince readers that the methodology can be used for forecasting volcanic unrest.

Answer: We thank the reviewer for this comment. We believe that the remark may arise from a misunderstanding of the scope of this paragraph. The lines 407–413 (we are referring to the lines of R1 manuscript that we report following) stats: "In parallel, we performed a spatial and temporal analysis of the Gutenberg-Richter bvalue using the CUBIT algorithm and the b-more-positive estimation method. Our results reveal a clear spatial pattern: higher bvalues are associated with the younger, hotter volcanic centers (Whakaari and Taupo), while older volcanoes (Tongariro and Ruapehu) exhibit lower bvalues. This is consistent with previous results obtained for other volcanoes of the world (Godano et al., 2024b). The distribution of b values across space and time displays a slight positive skewness, which we interpret as the result of a correlation between b and the local magnitude range δm_r. This highlights the genuine relationship between b values and the stress state: when earthquakes are smaller (small δm_r) b assumes larger values". Here we do not aim at demonstrating forecasting capability, but rather at describing the spatial distribution of the b-value and its physical interpretation in terms of crustal properties. In particular, the observed correlation between b-value and the local magnitude range δm_r is interpreted as a signature of varying stress conditions and material heterogeneity, consistent with previous studies in volcanic environments. Regarding the comment on forecasting capability (we assume that reviewer B is referring to all Section 5), we agree that the current formulation may give the impression that the proposed methodology directly provides forecasting capability for volcanic unrest. This was not our intention. In the revised manuscript, we have clarified that the primary goal of this study is to characterize seismic regimes through the joint analysis of seismic rate and b-value variations, rather than to develop or validate an operational forecasting tool. In particular, we emphasize that: (i) the swarm-informed ETAS model provides a framework for probabilistic forecasting of seismicity rates, consistent with standard ETAS approaches, and (ii) the b-value variations are interpreted as diagnostic indicators of the evolving physical state of the system, rather than predictive variables. We further clarify that a quantitative assessment of forecasting performance would require a dedicated validation framework (e.g., prospective testing or retrospective skill evaluation), which is beyond the scope of the present study. The text in Section 5 has been revised accordingly to avoid any overinterpretation of the results.