Dear Patricio Venegas-Aravena and Davide Zaccagnino,

I hope this email finds you both well. I have now received the reviews of your submission to Seismica, titled "Large earthquakes are more predictable than smaller ones." Thank you again for submitting your work to the journal.

Based on the evaluations, I believe your manuscript may be suitable for publication after some revisions. Below is a brief summary of the reviewers' comments:

Reviewer 1 raised substantial concerns regarding the clarity and theoretical grounding of the manuscript, particularly in relation to the use and interpretation of Lyapunov exponents and fractal dimensions. The reviewer noted inconsistencies between the proposed mathematical relations and the figures presented, a lack of clarity on key definitions (e.g., "thermodynamic fractal dimension"), and what they perceived as a fundamental misunderstanding of chaotic dynamics. Due to these concerns, the reviewer recommended rejection.

Reviewer 2, on the other hand, offered a more positive assessment of your work. They appreciated the novelty of combining multiscale modeling with real seismic data and acknowledged the overall quality of the writing. However, they also made several constructive suggestions to improve the manuscript.

Despite these points, Reviewer 2 recommended moderate revisions, emphasizing that no additional analyses were necessary but only clarifications and more cautious framing of results.

Given the diverging perspectives, I encourage you to revise the manuscript thoroughly, taking both sets of comments into account. In particular, I recommend:

1. Carefully addressing the technical criticisms from Reviewer 1, especially regarding the mathematical foundation of your framework and the interpretation of key figures.

2. Incorporating the constructive suggestions from Reviewer 2, especially on clarifying model limitations and better contextualizing your work within the broader seismological literature.

When you are ready to resubmit, please upload:

• A clean version of the revised manuscript (without markup),

• A marked-up version showing all changes (e.g., tracked or highlighted),

• A detailed response letter addressing each reviewer's comments point by point, including a summary of the main changes made to the manuscript.

Please also consider acknowledging the reviewers' contributions in your revised manuscript, should you feel it is appropriate.

There is no strict deadline for submitting your revised manuscript, but submitting it in a timely manner is encouraged. If you anticipate any delays, feel free to let me know.

Wishing you the best with the revisions. Don't hesitate to contact me if you have any questions or feedback regarding the review process or your submission.

Kind regards,

Giuseppe Petrillo

Nanyang Technological University – Earth Observatory of Singapore

giuseppe.petrillo@seismica.org

R: Dear Editor

Thank you very much for the insightful feedback. We have carefully considered all the comments provided by the reviewers. We regret that Reviewer 1 did not continue the review process for our manuscript. Nevertheless, we have implemented the necessary revisions to address the concerns raised in their initial assessment. This includes enhancing the explanation and the physical foundations of the equations utilized throughout this work. Furthermore, we have taken into account the minor revisions suggested by Reviewer 2. We present below our responses, which we believe sufficiently improve the quality of our manuscript. We therefore extend our sincere gratitude to the reviewers and the editor for their time and patience.

Yours sincerely,

Drs. Venegas-Aravena and Zaccagnino

%%

REVIEWER #1

Review of the manuscript Large earthquakes are more predictable than smaller ones by Patricio Venegas-Aravena and Davide Zaccagnino

The manuscript concerns the chaotic behaviour of the earthquake occurrence being the magnitude dependent on the Lyapunov exponent. The manuscript appear to be very confused and reveals a poor knowledge of the chaotic dynamics. Please find in the following some observations

1 Given a certain differential equation describing a dynamical system, Lyapunov exponents are the eigenvalues of the Jacobian matrix of the differential equation. The authors should show at which equation they refer.

R: The summation of Lyapunov exponents employed in this study is that proposed by Hoover and Posch (1994), which is not directly derived from the eigenvalues of the Jacobian matrix of the differential equation at an arbitrary point. Instead, this summation arises from the analysis of the average behavior along a trajectory in the phase space of nonequilibrium steady-state systems maintained by time-reversible thermostats. Its relevance lies in its provision of a global characterization of phase-space contractivity, directly related to the entropy production of the system. This approach, numerically validated by Hoover and Posch for various systems, offers a potent tool for understanding macroscopic irreversibility from reversible microscopic dynamics, a central aspect in the study of systems far from equilibrium. This explanation has been added to the text.

Line 105

2 Relation 1 states an exponential relation between the sum of the Lyapunov exponents and the fractal dimension. Then relation 2 states a linear relation between the magnitude of an earthquake and the fractal dimension. As a consequence relation 3, stating a dependence of magnitude on the Lyapunov exponent, should be exponential. However the plot of figure 1 in not exponential. Why?

R: The apparent deviation from a purely exponential form in the graph is attributed to the specific choice of the parameter k_V=1 in conjunction with the constrained ordinate range of 2 to 3. Were the parameter k_V to be reduced, or the ordinate range expanded, the underlying exponential trend would become more pronounced.

You can see an expanded range of this Equation 1 and Figure 1a by plotting the code: "plot y = - exp((3 - x - 1)/1), x in [0, 10]" wolfram alpha:

https://www.wolframalpha.com/input?i=plot+y+%3D+-+exp%28%283+-+x+-+1%29%2F1%29%2C+x+in+%5B0%2C+10%5D

If you restrict the range to [2,3] in the online code line ("plot y = - exp((3 - x - 1)/1), x in [2, 3]") you will obtain the same plot which is shown in Figure 1a. You can repeat for the other subplots and Equations. A comment is added after Equation 3.

Line 144

3 The authors introduce the fractal dimension in Eq. 1 without specifying in which euclidean space this is defined. Later they use the fractal dimension of the earthquakes space distribution. How it is related to magnitude?

R: Within the context of our work, D_E represents the dimension of the Euclidean space in which the spatial distribution of earthquake epicenters, and their ruptures, is embedded and subsequently analyzed to obtain an empirical fractal dimension. Specifically, D_E defines a volume and is therefore equal to 3. In the context of multiscale thermodynamics, the fractal dimension D is a global parameter of the fault that also quantifies the geometric irregularities of the faults and, consequently, their fracture energy. The relationship with magnitude lies in the fact that lower values of D imply a reduced fracture energy, leading to a larger area of positive residual energy and, consequently, a higher probability of the occurrence of earthquakes with greater magnitude M_W. Therefore, the fractal dimension of the spatial distribution of earthquakes, obtained within a Euclidean space D_E, is indirectly related to magnitude through its connection to the global parameter D of the fault. This explanation has been added after Equation 5.

Line 189

4 In figure 1 they plot negative Lyapunov exponents as a function of the thermodynamic fractal dimension. What is the thermodynamic fractal dimension? Negative Lyapunov exponent implies convergent trajectories in the phase space excluding chaos. Later the authors claim a chaotic behaviour

At this point my patience in reading the manuscript was completely exhausted. I find the manuscript too much confused and substantially wrong. As a consequence I suggest its rejection.

R: It is crucial to distinguish between individual Lyapunov exponents and their sum. While a negative sum of all Lyapunov exponents indicates overall phase-space volume contraction and dissipation, the presence of even a single positive Lyapunov exponent is the hallmark of chaos. This positive exponent signifies exponential divergence of initially infinitesimally close trajectories along a specific direction in phase space, leading to the unpredictability and sensitive dependence on initial conditions characteristic of chaotic systems. Therefore, a system can exhibit a net dissipative behavior (negative sum) yet still be fundamentally chaotic due to the local instability introduced by at least one positive Lyapunov exponent, driving the complex and seemingly random evolution of its dynamics. This is indicated before Equation 4.

Line 158

%%%

REVIEWER #2

Dear Editor, first of all, I deeply apologize with you and the Editor for the delay in providing my recommendations I have reviewed the manuscript “Large earthquakes are more predictable than smaller ones”, by Patricio Venegas-Aravena and Davide Zaccagnino. This study introduces a mathematical tool incorporating multiscale physics to describe both chaotic and deterministic earthquake dynamics. The results obtained by simulating the rupture process at different scale suggest that larger earthquakes are less sensitive to perturbations and thus potentially more predictable than smaller ones. This hypothesis is supported by numerical simulations and validated through comparisons with real seismic data from Southern California.

The work is well written and suitable for the journal.

Hereinafter, I have a few questions and suggestion for the authors.

5 Line 37. In the abstract, the relation between b-value and fractal dimension is presented as a result, but this is something discussed since Aki (1981), as also discussed by the authors themselves later, or by Hirata 1989 (https://doi.org/10.1029/JB094iB06p07507). Thus, more than as results, the good agreement between b-value and fractal dimension should be presented as supporting evidence about the good quality of the modeling.
R: This is true. The relationship between b-value and fractal dimension is not a new result of our paper; we rephrased the abstract in order to stress that our model predicts a correlation between fractal dimension and b-value (which generalize the results by Aki and Hirata) but is not unprecedented. We clarify that the agreement between theory and observational analysis is not a new result; anyway, it supports our model. Moreover, we take care to recognize this in the main text.
Line 34-37

6 Line 109. Even if the reference is provided, a few words more on the Lyapunov exponents could be provided to the readers. Besides the referenced paper, are there other studies where it was used?

R: In response to the request of Reviewer 1, several sentences have been added to provide a clearer explanation of the context within which this work is situated. Furthermore, a more physical description (including corresponding references) has been incorporated following Equation 1.

Line 112

7 Line 138. It is not clear to me the use of = in Eq. (3) being it originated by the approximation ~ in Eq. (2). Please clarify.

R: It only dictates the dependency of Mw on Lambda.

8 Line 158. Actually, E^{res} in Eq. (4) is the Radiated Energy. You can call it residual, but it may be good for the readers to clarify that this is the amount of energy radiated during the rupture process.

R: I fully concur with the reviewer's point. Nevertheless, as the concept derived by Noda et al. (2021) pertains to the energy within the faults and not the energy radiated into the surrounding medium, the term 'Residual Energy' has been retained. However, a sentence indicating the equivalence with 'Radiated Energy' and the rationale for maintaining the concept as defined by Noda has been added prior to Equation 5.

Line 178

9 Line 163. Not clear how Eq. (5) is derived. Please clarify

R: This Equations comes from the paper Venegas-Aravena and Cordaro (2023a). We have added the missing reference before Equation 5.

Line 185

10 Line 172. Figure (1). When you consider the magnitude change DMw it is not clear which are the two events considered. Which is the reference? is it a foreshock with specific magnitude in all simulation?

R: Figure 1d illustrates the variation in magnitude (determined by Equation 2) as a function of the residual energy (determined by Equation 5) present within a fault. Fundamentally, this figure demonstrates the change in magnitude resulting from the addition of a small increment of residual energy to a fault, relative to the same fault without this energy increase. Specifically, a fault with a low level of residual energy is capable of generating an earthquake of low magnitude. However, the injection of a small amount of energy into such a fault can lead to a considerably larger expected earthquake magnitude compared to the scenario without this additional energy. Conversely, repeating this procedure when a fault already contains a large amount of residual energy results in a negligible difference in the expected magnitude before and after the energy injection. This suggests a saturation effect in the expected magnitude as a function of the residual energy level within the fault. This difference in expected magnitude is what is depicted in Figure 1d. This explanation has been added after Figure 1.

Line 223

11 Lines 187-192. Not clear. Still on Figure (1c). DMw/Eres is, in practic, the opposite of the scaled energy (radiated energy over seismic moment ratio). Taking into account Kanamori and Rivera 2004 BSSA, your results implies that the seismicity is always non self-similar. It derives from the assumptions with which the model is built? In nature, we have cases where this is true, but also cases showing that the seismicity is self-similar. In that last cases the model you propose would not be adequate. Your discussion could thus benefit of the work by Kanamori and Rivera 2004 BSSA to clarify the applicability limits of your model.

R: The analysis presented by Kanamori and Rivera (2004) is not strictly equivalent to the analyses conducted in this study. Primarily, as demonstrated by Venegas-Aravena (2024a), the concept of residual energy can be equivalent to that of radiated energy only on average, but they are not identical in the case of heterogeneous faults. Furthermore, Kanamori and Rivera (2004) employ seismic moment (M_0), which differs from the change in magnitude (ΔM_W) utilized in this work. Nevertheless, an important relationship can be identified: the behavior of earthquakes may differ with respect to their size. Indeed, as the work of Kanamori and Rivera (2004) illustrates the variation in radiation efficiency, we compare it in a more abstract sense with sensitivity (to initial conditions). We propose that efficiency decreases with increasing sensitivity. This has been added to the beginning of the discussion section.

Line 554

12 Lines 214, 255, 256. Figure 2 instead of Figure 1

R: Thanks, it is fixed now.

13 Line 444. Figure 5. Looking at the results the fit is poor to justify the conclusions. It seems that the trend is only related to the two values for D smaller than 1.6 (b smaller than 1), while all the other measures are spread in the same b range (1 – 1.3) for D larger than 1.6. If you consider only data for D>1.6, the fit would be a horizontal line. That is not a nice result, and it confirms, as often happen, that the reality is much more complicated than that we can capture with our theoretical models. I am sure that you model that you propose it is useful to describe a range of conditions, but I am also rather confident that it does not work for all the conditions. A discussion of the limit of the model it is necessary.

R: We added a new paragraph about the limitations of our model. Moreover, we changed the algorithm we used for the sampling of the different regions we used to calculate the b-value and the fractal dimension in order to reduce the noise produced by stochastic fluctuations. We also removed the regions with fewer seismic events. The new picture shows a better signal to noise ratio and better fit quality. See new Figure 5.

Lines 512-516 + 722-746

14 Line 623. It seems that Longobardi et al. 2024 it only a preprint. I am not sure that work it is robust enough to support the results of other studies.

R: We removed the reference to the preprint by Longobardi et al.

Line 716

Final comment.

The work is certainly interesting, and it provide an interesting point of view.

My opinion/suggestion is that the work looks too much assertive about the validity of the model, which only with time and more observation will be proved. Hopefully, it will work in a good way for a certain set of faults.

R: We better clarified the hypotheses of our model in the session of results.

I believe **discussing more the limitation** of the model will make the reader think deeply about the ideas. Another issue is that most **key concepts** presented and discussed in this work are **linked to the works of the sacred monsters of seismology.** This should emerge more clearly.

R: In the session devoted to discussions we added new comments about the relationship of our model and results with previous major results in seismology and physics of earthquakes.

Lines: 647-668

Since, I believe that not new analyses are necessary, but only work on the text, the work can be accepted with moderate revision.

Thank you.