Peer-Review

Jun, Mosu. 2026. “A Reproducible Burridge–Knopoff Spring–Block Toy Model with Explicit Numerical Diagnostics for Stick–Slip Dynamics.” *Journal of High School Science* 10 (2): 24–48. https://doi.org/10.64336/001c.160295

This manuscript addresses a well-established modeling framework and numerical approach. While the topic is relevant, the paper requires substantial revision before it can be considered for publication. The following comments are intended to help the author clarify the contribution, strengthen the analysis, and better position the work relative to existing literature.

1.Novelty, contribution, and positioning

At present, the manuscript does not clearly articulate a novel contribution beyond established results in the literature. To improve clarity and impact, the authors are encouraged to address the following points:

a) Condense the theoretical background and equations, and instead rely more heavily on appropriate citations to prior work where these formulations have already been developed.

b) Clearly define the contribution of the present study, particularly in terms of how the hardware assumptions, simulation setup, and parameter choices are specified with sufficient precision to enable independent replication.

c) Provide explicit comparisons with published results, demonstrating how the present findings align with, extend, or differ from prior studies.

d) Clarify the practical value of the compact model, for example by explaining how its low computational cost or simplicity may benefit the research community (e.g., rapid parameter sweeps, educational use, or benchmarking).

e) The statement “In this paper, I construct a Burridge–Knopoff model and solve its equations of motion with the Euler–Cromer method” describes an approach that has been widely used in the literature. The authors may wish to remove or rephrase this statement and instead emphasize any systematic parameter studies or quantitative trends that go beyond prior demonstrations.

f) Similarly, the three guiding questions regarding stick–slip stability, recurrence times, and elastic synchronization have been explored previously. The manuscript would be strengthened by focusing on what is added here, such as:

- Explicit demonstrations of failure modes of standard Euler integration versus improved stability using Euler–Cromer
- Quantitative measures of numerical drift (e.g., cumulative displacement error or energy balance)

g) A comprehensive table of parameter values, ranges, and expected outcomes, alongside comparisons with literature values, would be valuable. If the chosen parameter ranges are physically motivated, this could also illuminate the sensitivity of the results to initial conditions.

h) The authors are encouraged to release all simulation code in a public repository (e.g., GitHub) to support transparency and reproducibility.

i) While the model is intentionally simple, the manuscript should address whether—and to what extent—it reproduces real-world or experimentally inferred behavior, with explicit comparisons to prior studies. Clarifying the predictive or explanatory limits of the model would be helpful.

j) The paper would benefit from a clearer discussion of what new insights the model enables, such as how varying specific parameters improves understanding, prediction, or interpretation of fault dynamics, or how it may guide future work.

k) If long-term goals such as earthquake prediction are mentioned, the authors should outline what additional physics or model extensions would be required to move in that direction.

l) Table 1 requires clarification, as its current presentation is difficult to interpret.

m) The statement that the framework can support AI-based benchmarking and hybrid physics–AI strategies is interesting; however, the manuscript would be strengthened if this claim were demonstrated explicitly or more clearly scoped as future work.

n) The practical significance of the reported metric changes should be discussed, including whether

modest variations have meaningful physical or predictive implications.
o) Greater robustness analysis is needed. Sensitivity across time step size (Δt), threshold parameters (ε, vε), boundary conditions, and broader parameter ranges is not yet systematically quantified.
p) The overall design is clear for a minimal model with event detection, but the manuscript would benefit from additional methodological detail, including:

- Explicit criteria for Δt selection and convergence testing
- Clear boundary-condition specifications
- Comparisons with alternative numerical integrators
- Sensitivity to event-detection thresholds
- A reproducibility package (scripts, random seeds, parameter files, and software versions)

2.Figures and presentation
Several figures lack units, and some require clearer axis labels. Improving figure clarity would significantly enhance readability and interpretability.
3.Statistical analysis
The statistical treatment is currently limited. Confidence intervals, sample sizes, regression fits (including slopes and R^2 values), uncertainty quantification, and hypothesis testing are not reported. The authors are encouraged to include appropriate statistical analyses, such as linear fits where relevant, to better support the reported trends.

Response to Reviewers

Submission ID of previous manuscript: 2881574
Manuscript title: A Reproducible Burridge–Knopoff Spring–Block Toy Model with Explicit Numerical Diagnostics for Stick–Slip Dynamics

I thank the reviewers for their careful reading and constructive suggestions. Below I provide a point-by-point response and indicate where each change appears in the revised manuscript.

Reviewer comment	Response / revision	Location in revised manuscript
a) Condense the theoretical background and equations, and instead rely more heavily on appropriate citations to prior work where these formulations have already been developed.	Thank you for the positive assessment of the manuscript and its value as a teaching tool. I have incorporated the requested clarifications and consistency edits described below.	General; throughout
b) Clearly define the contribution of the present study, particularly in terms of how the hardware assumptions, simulation setup, and parameter choices are specified with sufficient precision to enable independent replication.	I revised the Introduction to (i) clarify that the goal is a reproducible, classroom-ready implementation rather than a new earthquake model, and (ii) provide an explicit roadmap of how the figures and tables answer the three guiding questions.	Section 1 (Introduction), paragraph beginning “Here I implement …” and the roadmap paragraph beginning “Roadmap of the results …”
c) Provide explicit comparisons with published results, demonstrating how the present findings align with, extend, or differ from prior studies.	To better connect the toy-model outputs to established context, I added an interpretation map (Table 2) and expanded the Discussion to explain how the reported observables relate to standard	Section V (Discussion), final two paragraphs; Table 2

	laboratory and seismological concepts, while also stating the limits of the simplified friction law.	
d) Clarify the practical value of the compact model, for example by explaining how its low computational cost or simplicity may benefit the research community (e.g., rapid parameter sweeps, educational use, or benchmarking).	I updated Table 1 to include explicit units/definitions and to document the tested ranges used for convergence and threshold-sensitivity checks (Δt, v_th, ε, t_q), as well as the boundary-condition and initialization choices used across figures.	Table 1 and surrounding text (Sections II–III)
e) The statement "In this paper, I construct a Burridge–Knopoff model and solve its equations of motion with the Euler–Cromer method" describes an approach that has been widely used in the literature. The authors may wish to remove or rephrase this statement and instead emphasize any systematic parameter studies or quantitative trends that go beyond prior demonstrations.	I replaced language that could be read as claiming a new model with wording that emphasizes reproducibility and pedagogy, and I aligned the framing across the Abstract, Introduction, Discussion, and Conclusion.	Section 1 (Introduction) and Sections V–VI
f) Similarly, the three guiding questions regarding stick–slip stability, recurrence times, and elastic synchronization have been explored previously. The manuscript would be strengthened by focusing on what is added here, such as: • Explicit demonstrations of failure modes of standard Euler integration versus improved stability using Euler–Cromer • Quantitative measures of numerical drift (e.g., cumulative displacement error or energy balance)	I audited all figure callouts and updated them to match the final figure set (Figs. 1–9 and Fig. A1). I also streamlined the roadmap text so each figure range is referenced once and is consistent with the Results section.	Sections 1, III–IV; figure captions
g) A comprehensive table of parameter values, ranges, and expected outcomes, alongside comparisons with literature values, would be valuable. If the chosen parameter ranges are physically motivated, this could also illuminate the	I made the event-detection and measurement procedure explicit: event start/end definitions from v(t), quiet-time merging, total slip S, and the participation criterion based on a displacement threshold ε. The	Section 3 (Event detection, observables, and uncertainty estimates); Table 1

sensitivity of the results to initial conditions.	corresponding thresholds are documented in Table 1.	
h) The authors are encouraged to release all simulation code in a public repository (e.g., GitHub) to support transparency and reproducibility.	I clarified the recurrence-time scaling analysis by reporting the Monte Carlo aggregation, standard error, fit parameters, goodness-of-fit (R^2), and the per-point sample sizes n. These details are now stated in the Fig. 7 caption and referenced in the Results narrative.	Fig. 7 caption; Section IV (Results) recurrence-time subsection
i) While the model is intentionally simple, the manuscript should address whether—and to what extent—it reproduces real-world or experimentally inferred behavior, with explicit comparisons to prior studies. Clarifying the predictive or explanatory limits of the model would be helpful.	I corrected outdated references to earlier figure numbering in the numerical-update and simulation narrative so that all “bridge” sentences point to the correct figures in the revised structure.	Section 2 (Numerical update) and Section III (Simulation)
j) The paper would benefit from a clearer discussion of what new insights the model enables, such as how varying specific parameters improves understanding, prediction, or interpretation of fault dynamics, or how it may guide future work.	I clarified how synchronization is quantified in the two-block study by describing the cross-correlation lag metric used in Fig. 9 and by stating how it is computed from the displacement time series.	Section IV (Results) two-block synchronization subsection; Fig. 9 caption
k) If long-term goals such as earthquake prediction are mentioned, the authors should outline what additional physics or model extensions would be required to move in that direction.	I strengthened the paper’s scope statements by explicitly noting that the model is not intended for operational earthquake prediction and by explaining what insights a minimal threshold-friction toy model can and cannot support.	Section V (Discussion) and Section VI (Conclusion)
l) Table 1 requires clarification, as its current presentation is difficult to interpret.	I verified that the nondimensionalization choice is stated clearly and that the baseline parameters define the natural time scale; I also ensured the scaling argument for $\langle T \rangle$ is presented in a compact form consistent with the Results and Fig. 7.	Section II (Theory)
m) The statement that the framework can support AI-	I adjusted the machine-learning/early-warning	Section 1 (Introduction) and Section VI (Conclusion)

based benchmarking and hybrid physics–AI strategies is interesting; however, the manuscript would be strengthened if this claim were demonstrated explicitly or more clearly scoped as future work.	motivation to be appropriately scoped (as future-facing context rather than a performance claim), and I retained only statements that the toy model can directly support.	
n) The practical significance of the reported metric changes should be discussed, including whether modest variations have meaningful physical or predictive implications.	To improve reproducibility, I kept a single recommended baseline parameter set and explicitly documented the robustness checks (convergence and threshold sensitivity) that show the reported trends are stable under reasonable analysis choices.	Appendix A (Fig. A1) and Table 1; referenced in Results/Discussion
o) Greater robustness analysis is needed. Sensitivity across time step size (Δt), threshold parameters (ε, vε), boundary conditions, and broader parameter ranges is not yet systematically quantified.	I incorporated and emphasized robustness in three places: (i) sensitivity of event statistics to detection thresholds, (ii) convergence with respect to Δt, and (iii) robustness to small symmetry-breaking initial offsets in the 50-block chain.	Figs. 3–6; Table 1; Appendix A
p) The overall design is clear for a minimal model with event detection, but the manuscript would benefit from additional methodological detail, including: • Explicit criteria for Δt selection and convergence testing • Clear boundary-condition specifications • Comparisons with alternative numerical integrators • Sensitivity to event-detection thresholds • A reproducibility package (scripts, random seeds, parameter files, and software versions) 2. Figures and presentation Several figures lack units, and some require clearer axis labels. Improving figure clarity would significantly enhance readability and interpretability. 3. Statistical	I performed a final consistency pass to correct small wording/notation issues (e.g., consistent “Burridge–Knopoff” formatting, “event-size” wording) and to ensure that captions, text, and tables match one another.	Throughout

analysis The statistical treatment is currently limited. Confidence intervals, sample sizes, regression fits (including slopes and R^2 values), uncertainty quantification, and hypothesis testing are not reported. The authors are encouraged to include appropriate statistical analyses, such as linear fits where relevant, to better support the reported trends.

The reviewer thanks the author for taking the time to respond to the reviewer's comments. The manuscript has improved in clarity and presentation as a result of these revisions. However, concerns regarding novelty remain.

At present, the manuscript does not introduce new physical mechanisms, new scaling laws, or new theoretical insights into fault dynamics. The behaviors reported—such as stick–slip cycles, the dependence of recurrence time on frictional weakening, and synchronization under elastic coupling—are well established in the existing Burridge–Knopoff literature. As such, the contribution is primarily pedagogical, emphasizing reproducibility and clarity rather than advancing the current state of knowledge. The manuscript demonstrates known phenomena using established physics and commonly used metrics.

While this type of work may be suitable for journals with a primarily educational or tutorial focus, the Journal of High School Science applies a higher standard for originality. As stated in the JHSS "For Authors" section:

"The Journal publishes original research and experiments that have quantitative results and which make a significant contribution to the existing corpus of knowledge in the field."

To strengthen the manuscript and move it toward a genuinely original contribution, the reviewer encourages the authors to consider incorporating atleast a few of the following directions, which build naturally on the existing model:

a) Rather than treating friction as a binary (on/off) parameter, allow it to weaken gradually through a single continuous control parameter. Demonstrating a qualitative transition in system behavior—such as a shift from regular periodic slip to irregular or clustered events—would reveal dynamics not accessible in the original formulation.

b) Instead of focusing solely on average recurrence times, investigate whether the size of a slip event influences the waiting time to the next event. Simulating and plotting slip size versus subsequent waiting time could provide insight relevant to earthquake predictability within a simple model framework.

c) Examine the impact of detection thresholds. Real fault systems experience many small, often undetectable slip events that nonetheless influence stress redistribution. By analyzing system statistics under different detection thresholds (e.g., considering only large events versus including smaller slips), the authors could assess how incomplete observation biases inferred dynamics.

d) Explore directional asymmetry by asking whether forward and backward slip events exhibit statistically distinct behavior. Directional bias in stress release or recurrence patterns could reveal emergent asymmetries in the system.

e) Investigate spatial interactions by testing whether a slip event in one block suppresses or promotes subsequent slipping in neighboring blocks. This would allow discussion of triggering and aftershock-like behavior.

f) Quantify predictability limits by initializing two nearly identical simulations and measuring how rapidly their trajectories diverge. This approach would provide a quantitative measure of the time horizon over which prediction remains meaningful.

g) Study observational bias by comparing statistics measured at different locations along the fault (e.g., edge versus center blocks, or strong versus weak blocks). This may reveal how local measurements can misrepresent global system behavior.

h) Assess degeneracy in statistical descriptions by tuning distinct parameter sets to produce identical mean recurrence times and mean slip sizes, and then comparing higher-order features such as clustering and event sequences.

Incorporating several of these ideas would substantially enhance the manuscript's originality and scientific contribution. With such additions, the work would be much better aligned with the publication standards of JHSS.

Response to Reviewers (Round 2)

Submission ID: 2881574
Revised manuscript title: A Reproducible Burridge–Knopoff Spring–Block Toy Model with Explicit Numerical Diagnostics for Stick–Slip Dynamics
Revised manuscript file: 2881574 _Manuscript_WithFigures_Round2_v2.docx

I appreciate the reviewer's careful reading and detailed guidance. In this second revision, I not only preserved the clarity and reproducibility improvements from the first revision, but also added several quantitative extension analyses to address the remaining concern about novelty. Below, I reproduce each reviewer comment verbatim and describe exactly how and where it is addressed in the revised manuscript.

0. Summary of changes in this revision

- Expanded the Results section with four originality-focused analyses: (i) slip size versus subsequent waiting time (Fig. 10), (ii) observation bias from size-based detection thresholds (Fig. 11), (iii) a predictability horizon from divergence of nearly identical simulations (Fig. 12), and (iv) two additional bias/identifiability demonstrations—location-dependent statistics along the chain (Fig. 13) and a simple degeneracy example (Fig. 14).
- Updated the Abstract, Introduction roadmap, Discussion, and Conclusion to reflect these additions and to clarify scope as a transparent toy model rather than an operational forecasting tool.
- Maintained the fully specified reproducibility package (code, parameter files, fixed random seeds, and environment/version information) as Supplementary Material; a private repository link is available during review and will be made public upon acceptance.

1. Global revision map (original submission → revised manuscript)

To meet the editor's request for a detailed description of where and how the manuscript changed, Table 1 below summarizes the major structural and content revisions from the original submission to the current revised manuscript.

Manuscript element	Original submission	Revised manuscript (current)	What changed / why it matters
Title	Stick–Slip Earthquakes: A Spring–Block Model with the Euler–Cromer Method	A Reproducible Burridge–Knopoff Spring–Block Toy Model with Explicit Numerical Diagnostics for Stick–Slip Dynamics	Reframed the work explicitly as a reproducible toy model with numerical diagnostics and quantified observables.
Abstract	Broad motivation	Rewritten to	Clarified scope (not

	and results statement; limited emphasis on reproducibility specifics.	emphasize a single recommended parameter set, explicit event parsing, convergence checks, and the full set of quantitative results (including new Figs. 10–14).	operational prediction) and highlighted new extension analyses added for originality.
Theory	Longer background narrative and equations.	Condensed to the minimum governing equations in dimensionless form, with a clear scaling argument for recurrence time.	Shifted detailed background to citations; kept only what is required to interpret results.
Simulation / Methods	Incomplete parameter documentation; limited event-parsing specification.	Added Table 1 with baseline parameters, units, and tested ranges; expanded event-detection rules and uncertainty reporting; added Appendix A comparing Euler vs Euler–Cromer stability.	Made replication possible figure-by-figure, including threshold sensitivity and convergence checks.
Results	Focused on reproducing known stick–slip behavior and basic parameter trends.	Organized by model complexity (N=50 chain → N=1 slider → N=2 synchronization) and expanded with new analyses (Figs. 10–14) addressing predictability, observation bias, and degeneracy.	Added new quantitative relationships and diagnostics within the same toy framework.
Discussion / Limitations	General discussion; limited linkage to lab/seismology; scope creep toward prediction/AI.	Added an interpretation map (Table 2) linking observables to laboratory and seismological context; explicitly stated model limitations and what added physics would be needed for forecasting.	Improved positioning relative to literature and avoided overstated claims.

Reproducibility materials	Not fully specified.	Provided complete supplementary reproducibility package (code, parameters, seeds, environment/version info) and a private repository link during review.	Directly addresses transparency expectations and reviewer request for public release.

2. Reviewer 1 comments and responses (verbatim)

Comment (verbatim)	Response / revision	Where in revised manuscript
a) Condense the theoretical background and equations, and instead rely more heavily on appropriate citations to prior work where these formulations have already been developed.	I condensed Section II (Theory) to the minimum governing equations in dimensionless form and added/strengthened citations where the Burridge–Knopoff formulation and related results are already established. The revised Theory focuses on the specific equations and scaling used in the paper and avoids extended background narrative.	Section II (Theory).
b) Clearly define the contribution of the present study, particularly in terms of how the hardware assumptions, simulation setup, and parameter choices are specified with sufficient precision to enable independent replication.	I rewrote the Introduction to state the scope explicitly (a reproducible, classroom-ready toy model rather than a new forecasting model) and added a roadmap linking each figure to a guiding question and observable. I also expanded Section III with explicit event-detection rules, thresholds, and uncertainty reporting, and provided a single recommended parameter set in Table 1.	Abstract; Section 1 (Introduction) roadmap paragraph; Section III.1–III.3; Table 1.
c) Provide explicit comparisons with published results, demonstrating how the present findings align with, extend, or differ from prior studies.	I expanded the Discussion and added an interpretation map (Table 2) that links each measured observable (recurrence, size proxies, synchronization lag, and the new extension diagnostics) to established	Section V (Discussion); Table 2.

	laboratory and seismological concepts and representative sources. This clarifies what is reproduced from the literature and what is newly quantified here.	
d) Clarify the practical value of the compact model, for example by explaining how its low computational cost or simplicity may benefit the research community (e.g., rapid parameter sweeps, educational use, or benchmarking).	I clarified that the value of this work is practical: a fully specified, low-cost simulator that can be regenerated figure-by-figure and used for rapid parameter sweeps or as a transparent benchmark for computing recurrence and synchronization metrics. I also documented parameter ranges tested for numerical convergence and threshold sensitivity (Table 1).	Abstract; Section 1 (Introduction); Section III.1; Table 1.
e) The statement “In this paper, I construct a Burridge–Knopoff model and solve its equations of motion with the Euler–Cromer method” describes an approach that has been widely used in the literature. The authors may wish to remove or rephrase this statement and instead emphasize any systematic parameter studies or quantitative trends that go beyond prior demonstrations.	I removed/rephrased language that could be read as claiming novelty for implementing Burridge–Knopoff or Euler–Cromer itself. The revised manuscript instead emphasizes quantified trends and diagnostics (Monte Carlo recurrence scaling, cross-correlation lag, and the new extension analyses in Figs. 10–14).	Abstract; Section 1 (Introduction); Section IV (Results), especially Figs. 7–14.
f) Similarly, the three guiding questions regarding stick–slip stability, recurrence times, and elastic synchronization have been explored previously. The manuscript would be strengthened by focusing on what is added here, such as: • Explicit demonstrations of failure modes of standard Euler integration versus improved stability using Euler–Cromer •	To clarify what is added beyond the established phenomena, I included an explicit numerical-stability demonstration in Appendix A. Fig. A1 compares forward Euler and Euler–Cromer for a spring–mass oscillator, showing the energy drift failure mode in forward Euler and the improved long-run behavior of Euler–Cromer at the same time step.	Appendix A; Fig. A1.

Quantitative measures of numerical drift (e.g., cumulative displacement error or energy balance)		
g) A comprehensive table of parameter values, ranges, and expected outcomes, alongside comparisons with literature values, would be valuable. If the chosen parameter ranges are physically motivated, this could also illuminate the sensitivity of the results to initial conditions.	I rebuilt Table 1 as a comprehensive parameter table: each symbol is defined, baseline values and units are listed, and tested ranges/notes are provided for convergence checks (Δt), threshold sensitivity (v_th, ε, t_q), and control sweeps (ΔF, k_c). Where appropriate, the Discussion cites representative laboratory and seismological references to contextualize observables and typical behaviors.	Table 1; Section III.1–III.3; Section V (Discussion) with Table 2.
h) The authors are encouraged to release all simulation code in a public repository (e.g., GitHub) to support transparency and reproducibility.	I prepared and included a complete reproducibility package as Supplementary Material, including source code, parameter files, fixed random seeds, and environment/version information. For reviewer convenience, the same package is also available via a private repository link during review and will be made publicly accessible upon acceptance.	Cover letter; Supplementary Material; mentioned in Section III.4 (Modeling / reproducibility).
i) While the model is intentionally simple, the manuscript should address whether—and to what extent—it reproduces real-world or experimentally inferred behavior, with explicit comparisons to prior studies. Clarifying the predictive or explanatory limits of the model would be helpful.	I strengthened the Discussion to clarify what the toy model does and does not represent. Table 2 explicitly links each observable to laboratory and seismological context, while the surrounding text states the limits of the two-level friction law and what additional physics (rate-and-state friction, heterogeneity, realistic geometry) would be required for operational prediction.	Section V (Discussion); Table 2; Conclusion.
j) The paper would benefit	I clarified the “insight” as a	Section IV (Results),

from a clearer discussion of what new insights the model enables, such as how varying specific parameters improves understanding, prediction, or interpretation of fault dynamics, or how it may guide future work.	set of measurable, reproducible diagnostics that can be computed from a minimal model: (i) recurrence-time scaling with ΔF (Fig. 7), (ii) synchronization quantified via cross-correlation lag (Fig. 9), and (iii) the new extension analyses (Figs. 10–14) that connect event size to subsequent waiting time, quantify observation bias and predictability limits, and demonstrate parameter degeneracy.	especially Figs. 7–14; Section V (Discussion).
k) If long-term goals such as earthquake prediction are mentioned, the authors should outline what additional physics or model extensions would be required to move in that direction.	I explicitly scoped the manuscript away from operational forecasting and added a concrete discussion of what physics would be required for prediction-oriented work (e.g., rate-and-state friction, heterogeneity, more realistic fault geometry, and assimilation of observational constraints).	Abstract final sentence; Section V (Discussion); Conclusion.
l) Table 1 requires clarification, as its current presentation is difficult to interpret.	I rebuilt Table 1 with clear column headings, explicit symbol meanings, baseline values, units, and tested ranges/notes. I also ensured the table uses a standard grid layout for readability.	Table 1 and its surrounding explanation in Section III.1.
m) The statement that the framework can support AI-based benchmarking and hybrid physics–AI strategies is interesting; however, the manuscript would be strengthened if this claim were demonstrated explicitly or more clearly scoped as future work.	I re-scoped any AI-related language as future work and avoided implying that the present toy model directly enables prediction. The revised Discussion frames the model as a controlled testbed for generating synthetic time series and benchmarking metrics, while emphasizing that real forecasting would require additional physics and data.	Section V (Discussion); Conclusion.
n) The practical significance of the reported	I expanded the Discussion to interpret metric changes	Section V (Discussion); new text connected to Figs.

metric changes should be discussed, including whether modest variations have meaningful physical or predictive implications.	qualitatively and to emphasize what they do and do not imply physically. For example, the cross-correlation lag is presented as a synchronization diagnostic rather than a forecasting tool, and the new predictability-horizon and bias analyses are discussed as illustrative limits and caveats in interpreting toy-model outputs.	9–14.
o) Greater robustness analysis is needed. Sensitivity across time step size (Δt), threshold parameters (ε, $v\varepsilon$), boundary conditions, and broader parameter ranges is not yet systematically quantified.	I added explicit convergence and sensitivity checks in the Methods/Simulation section and documented the tested ranges in Table 1 (Δt convergence check; v_th, ε, t_q threshold tests; boundary conditions and initialization notes).	Section III.1–III.3; Table 1.
p) The overall design is clear for a minimal model with event detection, but the manuscript would benefit from additional methodological detail, including: • Explicit criteria for Δt selection and convergence testing • Clear boundary-condition specifications • Comparisons with alternative numerical integrators • Sensitivity to event-detection thresholds • A reproducibility package (scripts, random seeds, parameter files, and software versions)	I expanded Section III to include explicit event parsing and uncertainty reporting, documented Δt and threshold tests (Table 1), and added Appendix A to compare Euler and Euler–Cromer numerically. I also assembled a reproducibility package (code, parameters, seeds, software versions) as Supplementary Material and referenced it in the cover letter and Methods.	Section III.1–III.4; Table 1; Appendix A and Fig. A1; Supplementary Material; cover letter.

3. Figures and presentation

I audited every figure for axis labels and units, expanded captions to be self-contained, and ensured that the visual narrative matches the text roadmap. Where the manuscript reports regression trends (e.g., recurrence scaling), the corresponding fits, R^2 values, and sample sizes are reported in the caption and/or surrounding text.

Where addressed: Section IV (Results), figure captions for Figs. 1–14 and Appendix Fig. A1.

4. Statistical analysis

I strengthened the statistical reporting in two ways. First, for parameter sweeps I report sample sizes, uncertainty estimates (standard error where applicable), and regression results (slope/intercept and R^2) directly in the Results text and captions (e.g., Fig. 7 and Fig. 10). Second, the new extension analyses explicitly quantify correlations (size versus subsequent waiting time), sensitivity to detection thresholds, and a predictability-horizon marker from divergence tests.
Where addressed: Section IV.7 and Fig. 7; Section IV.10–IV.14 and Figs. 10–14.

5. Reviewer follow-up: novelty concerns and suggested directions (a–h)

In the follow-up review, the reviewer noted that the core phenomena originally emphasized are well established in the Burridge–Knopoff literature and suggested several natural extensions (a–h). In response, I implemented several of these directions in the revised manuscript (Figs. 10–14) while keeping the core model transparent and reproducible. A separate "Implementation Map" file is also provided for convenience.

Item	Reviewer suggestion (verbatim summary)	What I changed / added	Where addressed
a	Continuous, gradually weakening friction through a single control parameter; demonstrate a qualitative transition.	Not implemented in this revision. I kept the binary static/kinetic switch to preserve the pedagogical simplicity of the toy model and to keep event parsing unambiguous. I added explicit discussion of this limitation and described how a velocity-weakening or rate-and-state law could be incorporated as future work.	Section V (Discussion), limitations/future-work text.
b	Investigate whether slip size influences the waiting time to the next event.	Implemented: I added a slip-size versus subsequent-waiting-time analysis in a 50-block chain using an event-size proxy S_total and quantified the correlation and linear fit.	Section IV.10; Fig. 10.
c	Examine the impact of detection thresholds (missing small events) on inferred statistics.	Implemented: I added an observational-bias analysis that recomputes inferred recurrence statistics	Section IV.11; Fig. 11.

		after discarding small events below a size threshold S_min. This directly demonstrates how incomplete detection biases recurrence estimates.	
d	Explore directional asymmetry (forward vs backward slip).	Not emphasized because the present driven model produces overwhelmingly forward slip under the chosen parameters. I clarified this point in the Discussion and noted that directional asymmetry becomes meaningful when additional physics (e.g., heterogeneity or state-dependent friction) allows reverse motion episodes.	Section V (Discussion), limitations/future-work text.
e	Investigate spatial interactions: whether slip in one block suppresses or promotes subsequent slip in neighbors.	Partially addressed: the new Results discussion reports event-size proxies built from multi-block participation (Section IV.10) and explicitly analyzes location-dependent statistics (Section IV.13). I also discuss how neighbor participation metrics can be computed from the same event catalog as a next-step extension.	Section IV.10–IV.13; Section V (Discussion), future-work paragraph.
f	Quantify predictability limits	Implemented: I added a divergence	Section IV.12; Fig. 12.

	via divergence of nearly identical simulations.	test with a small initial perturbation and defined a simple predictability-horizon marker based on RMS trajectory separation crossing a threshold.	
g	Study observational bias by comparing statistics at different locations (edge vs center).	Implemented: I added a location-dependent observational-bias analysis comparing inferred recurrence at edge and center blocks under the same detection rule.	Section IV.13; Fig. 13.
h	Assess degeneracy: distinct parameter sets produce identical mean recurrence and mean slip sizes; compare higher-order features.	Implemented in a simplified form: I added a degeneracy example showing that two distinct (ΔF, V) pairs with the same $\Delta F/V$ ratio match the same mean recurrence $\langle T \rangle$ but yield different slip sizes, illustrating non-uniqueness in inference from limited summary statistics.	Section IV.14; Fig. 14.

I hope these revisions address the reviewer's concerns thoroughly, and I appreciate the opportunity to improve the manuscript.

The reviewer thanks the author for the detailed responses to the previous comments. While several points have been clarified, a number of substantive issues remain that should be addressed before the manuscript can be considered for publication. In particular, the reviewer encourages the author not only to present results, but also to provide clear physical explanations of those results, with explicit discussion of their interpretation and relevance, especially in the Discussion section.
The following points require further clarification or revision:

1. In Figures 1–4, displacement, velocity, and time appear to be presented as normalized or dimensionless ratios. Please explicitly describe how these quantities are defined and where this normalization is introduced in the text.
2. In Figure 3, there appears to be a slight overshoot at intermediate times. Please explain the origin of this behavior and discuss its physical or numerical significance.
3. In Figure 4, the velocity appears to decrease with time. Please clarify whether this represents a genuine physical effect or an artifact of the modeling or initial conditions. Additionally, specify

what random initial offsets were used, and whether Figures 3 and 4 represent averages over multiple realizations or a single representative run.

4. Figures 1 and 5, as well as Figures 2 and 6, appear qualitatively very different despite representing related quantities. Please explain the reason for this discrepancy.
5. Units are missing from Figures 5, 6, and 7, as well as Figures 8–12. All axes and reported quantities should include appropriate units or be clearly identified as dimensionless.
6. In Figure 7, the reported fit of $38.48(\Delta F) \pm 4.52$ is unclear. As written, it suggests a nonzero intercept at $\Delta F = 0$, which does not appear physically meaningful. Please clarify the fitting procedure and interpretation of the uncertainty.
7. In Figure 8, Block 1 becomes difficult to see after a certain time. Please clarify whether Block 2 (orange) is overlaid on top of it, and if so, make this explicit in the figure or caption.
8. In Figure 9, the manuscript should clarify that synchronization manifests primarily through event coincidence rather than time delay, with coupling strength affecting the probability of joint slip events rather than their temporal offset. In particular, the interpretation of zero lag at small spring stiffness should be explained. Additionally, please justify why only a two-block system is used in Figures 8 and 9.
9. Figure 10 would benefit from additional statistical analysis, including correlation measures (e.g., R^2) and a larger number of simulation points to support the observed trends.
10. For Figure 11, please clarify the interpretation along the lines of:
“Figure 11 shows that ignoring small events makes the system appear to have much longer recurrence times, even though no physical change has occurred.”
The relevance of this result to earthquake analysis should be discussed explicitly.
11. For Figure 12, please clarify the interpretation along the lines of:
“Figure 12 shows that even infinitesimal initial differences grow to macroscopic disagreement after a finite time, defining a practical limit on predictability in the stick–slip model.”
12. For Figure 13, please clarify the interpretation along the lines of:
“Figure 13 shows that different positions along the same chain yield different recurrence times, indicating that observations at a single location can bias conclusions about the system’s overall dynamics.”
13. The Discussion section should synthesize the insights from the preceding figures and explicitly address what this toy model implies for earthquake behavior and interpretation.
14. For Figure 14, the earthquake relevance should be emphasized clearly, as this figure represents a key conclusion of the paper. In particular, the discussion should convey that:
- Two faults may exhibit similar average recurrence intervals.
- One fault may release stress through many small events, while another does so through fewer, larger ruptures.
- If only recurrence statistics are considered, these faults may appear similar despite having very different hazard implications.

Figure 14 should therefore be discussed along the lines of:
“Figure 14 demonstrates that different model parameter choices can yield the same average recurrence time while producing systematically different slip magnitudes, illustrating a fundamental degeneracy in inference based on recurrence statistics alone.”

Response to Reviewer’s Defense-3 Requests (14 items)

I appreciate the reviewer’s additional, detailed requests. Below, I reproduce each request (verbatim) and describe exactly what I changed and where it is addressed in the revised submission package.

Item	Reviewer request (verbatim)	My revision	Where addressed
1	In Figures 1–4, displacement, velocity, and time appear to be	I added an explicit nondimensionalization paragraph defining the scales for time,	Section II; Results preface; captions for Figs. 1–14.

	presented as normalized or dimensionless ratios. Please explicitly describe how these quantities are defined and where this normalization is introduced in the text.	displacement, velocity, and force, and I cross-referenced these definitions wherever I describe “dimensionless” quantities in the Results.	
2	In Figure 3, there appears to be a slight overshoot at intermediate times. Please explain the origin of this behavior and discuss its physical or numerical significance.	I explained the small overshoot in Fig. 3 as a short-lived elastic ringing after a large slip step (inertia + coupling), and clarified that it is not a numerical artifact.	Section IV (Fig. 3 discussion); Fig. 3 caption.
3	In Figure 4, the velocity appears to decrease with time. Please clarify whether this represents a genuine physical effect or an artifact of the modeling or initial conditions. Additionally, specify what random initial offsets were used, and whether Figures 3 and 4 represent averages over multiple realizations or a single representative run.	I clarified that the apparent decrease in peak velocity in Fig. 4 is a transient during relaxation from the heterogeneous initial prestress, documented the random initial offsets, and stated explicitly that Figs. 3–4 are a single seeded realization (not an average).	Section IV (Fig. 4 discussion); Figs. 3–4 captions.
4	Figures 1 and 5, as well as Figures 2 and 6, appear qualitatively very different despite representing related quantities. Please explain the reason for this discrepancy.	I added a direct comparison explaining why the embedded-chain block (Figs. 1–2) looks different from the isolated single slider (Figs. 5–6): neighbor coupling and stress transfer in the chain	Start of Section IV.5 (single-slider comparison paragraph).

		disrupt strict periodicity and produce multi-block effects.	
5	Units are missing from Figures 5, 6, and 7, as well as Figures 8–12. All axes and reported quantities should include appropriate units or be clearly identified as dimensionless.	I fixed the unit/notation ambiguity by labeling the affected plots as dimensionless and adding a one-sentence caption note for consistency.	Captions for Figs. 5–12; Results notation sentence.
6	In Figure 7, the reported fit of $38.48(\Delta F) \pm 4.52$ is unclear. As written, it suggests a nonzero intercept at $\Delta F = 0$, which does not appear physically meaningful. Please clarify the fitting procedure and interpretation of the uncertainty.	I rewrote the Fig. 7 fitting description to state the fitting procedure explicitly and to interpret the fit as a descriptive trend. In the revised manuscript, Fig. 7 reports the ordinary least-squares fit to the four Monte Carlo means as $\langle T \rangle = 99.61\, \Delta F + 3.41$ ($R^2 = 1.000$); I emphasize the slope and avoid physical interpretation of the intercept at $\Delta F \to 0$.	Section IV.7 and Fig. 7 caption.
7	In Figure 8, Block 1 becomes difficult to see after a certain time. Please clarify whether Block 2 (orange) is overlaid on top of it, and if so, make this explicit in the figure or caption.	I made the overlap explicit by differentiating line styles (Block 1 dashed) and stating in the caption that reduced visibility reflects near-coincident traces rather than missing data.	Fig. 8 plot + caption.
8	In Figure 9, the manuscript should clarify that synchronization manifests primarily through event coincidence rather than time delay, with coupling	I reframed synchronization as event coincidence (joint slip probability) with coincidence window $w=1.0$, explained why a lag metric can appear near zero at small coupling,	Section IV.9; Fig. 9; Appendix Fig. A2.

	strength affecting the probability of joint slip events rather than their temporal offset. In particular, the interpretation of zero lag at small spring stiffness should be explained. Additionally, please justify why only a two-block system is used in Figures 8 and 9.	and justified the two-block system as the minimal isolation of coupling effects. I also added Appendix Fig. A2 to visualize the lag-metric limitation.	
9	Figure 10 would benefit from additional statistical analysis, including correlation measures (e.g., R^2) and a larger number of simulation points to support the observed trends.	I increased statistical support for Fig. 10 by pooling multiple seeded runs and reporting regression/correlation measures (including R^2) on the figure and in the caption.	Section IV.10; Fig. 10 caption.
10	For Figure 11, please clarify the interpretation along the lines of: “Figure 11 shows that ignoring small events makes the system appear to have much longer recurrence times, even though no physical change has occurred.” The relevance of this result to earthquake analysis should be discussed explicitly.	I added the requested interpretation sentence verbatim and explicitly discussed the earthquake relevance as a catalog-completeness bias affecting recurrence inference.	Section IV.11; Fig. 11 caption; Discussion synthesis paragraph.
11	For Figure 12, please clarify the interpretation along the lines of: “Figure 12 shows that even infinitesimal initial differences grow to macroscopic	I added the requested interpretation sentence and defined a practical predictability horizon using trajectory divergence (δ crossing 10^{-2}).	Section IV.12; Fig. 12 caption.

	disagreement after a finite time, defining a practical limit on predictability in the stick–slip model.”		
12	For Figure 13, please clarify the interpretation along the lines of: “Figure 13 shows that different positions along the same chain yield different recurrence times, indicating that observations at a single location can bias conclusions about the system’s overall dynamics.”	I added the requested interpretation sentence emphasizing location-dependent recurrence and the risk of single-location observation bias.	Section IV.13; Fig. 13 caption.
13	The Discussion section should synthesize the insights from the preceding figures and explicitly address what this toy model implies for earthquake behavior and interpretation.	I expanded the Discussion to synthesize what Figs. 10–14 imply for earthquake interpretation (measurement bias, predictability limits, and inference degeneracy) while keeping the toy-model scope explicit.	Section V (Discussion).
14	For Figure 14, the earthquake relevance should be emphasized clearly, as this figure represents a key conclusion of the paper. In particular, the discussion should convey that: • Two faults may exhibit similar average recurrence intervals. • One fault may release stress through many small events, while another does so	I strengthened the Fig. 14 discussion to emphasize earthquake relevance using the reviewer’s bullet points and the suggested framing about degeneracy in inference from recurrence statistics alone.	Section IV.14; Fig. 14 caption; Discussion paragraph referencing Fig. 14.

	through fewer, larger ruptures. • If only recurrence statistics are considered, these faults may appear similar despite having very different hazard implications. Figure 14 should therefore be discussed along the lines of: “Figure 14 demonstrates that different model parameter choices can yield the same average recurrence time while producing systematically different slip magnitudes, illustrating a fundamental degeneracy in inference based on recurrence statistics alone.”		

The reviewer thanks the author for carefully considering the previous comments and submitting a revised manuscript. The revisions have improved the clarity of the paper in several places. However, a number of substantive issues remain that should be addressed before the manuscript can be considered further for publication.

The reviewer also notes that this manuscript has already undergone multiple rounds of review. Accordingly, the next revision should ensure that the concerns raised below are addressed comprehensively. The author is encouraged to carefully review the manuscript in its entirety to confirm that all outstanding issues have been resolved prior to resubmission.

1.Axis labeling for nondimensional variables

The manuscript states that all variables are nondimensionalized and that figures therefore use dimensionless units. While nondimensionalization is appropriate for this type of modeling, the current axis labeling lacks sufficient clarity. Simply labeling axes as “dimensionless” does not communicate the specific scaling used. The author should explicitly indicate the nondimensional variables in the axis labels (for example t/t0, x/x0, v/(x0/t0)) or otherwise clearly specify the normalization used in the plotted quantities. This would improve transparency and interpretability.

2.Physical interpretation of nondimensional quantities

Because all quantities are presented in nondimensional form, it is difficult for readers to interpret the physical meaning of the reported magnitudes. For example:

- What physical velocity corresponds to a dimensionless value such as 0.4?
- What physical timescale is implied for the simulated recurrence intervals?

Providing representative dimensional scales or example conversions would help readers better interpret the results.

3.Statistical robustness of the recurrence-time analysis (Fig. 7)

The recurrence-time scaling shown in Fig. 7 is based on only four sampled values of ΔF, each averaged over four realizations. The resulting linear fit with R2=1 should therefore be interpreted cautiously, as such a value can easily arise from a very small number of points. In addition, the figure omits uncertainty estimates despite the limited number of realizations. Including error bars and expanding the parameter sampling would improve the statistical robustness of this analysis.

4.Predictability analysis (Fig. 12)

The predictability analysis presented in Fig. 12 would benefit from further clarification and methodological strengthening. Divergence between nearly identical simulations in nonlinear threshold systems is generally expected behavior and has been widely discussed in studies of Burridge–Knopoff–type models. The current analysis does not estimate Lyapunov exponents or perform ensemble analyses to quantify sensitivity to initial conditions. Additionally, the predictability horizon is defined using an arbitrary displacement threshold and a single perturbation experiment. The author should clarify the intended scope of this analysis and consider strengthening the methodology if broader claims are intended.

5.Limited Monte Carlo sampling

Several results rely on a small number of realizations (e.g., four runs per parameter setting). While this may be sufficient for illustrative demonstrations, it limits the statistical strength of the conclusions. Increasing the number of realizations and reporting uncertainty estimates would improve the reliability of the results.

6.Limited exploration of parameter space

The manuscript varies only a small subset of model parameters (primarily the friction gap ΔF and the coupling stiffness kc). However, Burridge–Knopoff models can be sensitive to several parameters, including:

- friction law
- loading rate
- stiffness ratios
- system size
- heterogeneity

A broader exploration of parameter space would help clarify the generality of the reported behaviors.

7.System size considerations

The primary simulations use a system size of N=50. Because Burridge–Knopoff systems can exhibit size-dependent behavior, it would be helpful to discuss whether the results are robust to different system sizes or boundary conditions. Even a brief scaling comparison could strengthen the conclusions.

8.Overall contribution

The manuscript presents a clear and reproducible implementation of a Burridge–Knopoff spring-block model, which may be valuable as a pedagogical demonstration. However, many of the current results primarily reproduce qualitative behaviors that are already well established for this class of models. To strengthen the manuscript’s scientific contribution, the author may wish to expand the analysis beyond illustrative time-series plots. In particular, a more systematic statistical analysis of event sizes and waiting times, along with a broader exploration of parameter space (for example coupling strength, friction gap, or system size), could significantly enhance the novelty and impact of the work. Increasing the number of Monte Carlo realizations and presenting uncertainty estimates would also improve the robustness of the conclusions.

Response to Reviewer — Defense 4

I thank the reviewer for the comprehensive evaluation and for the clear request to address all remaining issues prior to further consideration. Below I provide an item-by-item response, and I indicate exactly where each change appears in the revised manuscript (Sections/Figures/Appendices).

Item	Reviewer request (summary)	What I changed (response)	Where in the revised manuscript
1	Clarify axis labeling for nondimensional variables; make scaling explicit on plots and in the text.	I added a dedicated notation statement that explicitly lists the nondimensional scalings used in every figure (e.g., time, displacement, velocity, RMS separation, and mean recurrence). I also ensured that each figure caption includes an "Axes:" line stating the plotted nondimensional ratios.	Main text: "Notation (units)" paragraph near the start of Results; Figure captions: Figs. 1–14 and A1–A5 (each includes an "Axes:" line).
2	Provide physical interpretation of nondimensional quantities (example conversions for velocity and recurrence time).	I added an explicit example conversion paragraph that maps representative dimensionless values to illustrative physical scales (a worked example for velocity and recurrence time). I also clarified that the mapping is interpretive (not a calibration) and that a real-fault mapping would require separate parameter calibration.	Section II (Theory): paragraph beginning "In dimensional terms…" and "For example, choosing … (illustrative) …".
3	Strengthen statistical robustness of the recurrence-time analysis in Fig. 7 (more samples / uncertainty clarity).	I expanded the recurrence-time sweep to eight tested friction-gap values and increased the ensemble size to 25 realizations per value. I report standard-error uncertainty for the ensemble mean and keep the linear fit as	Results (Recurrence-time scaling) + Fig. 7 narrative and caption (ensemble size and uncertainty description).

		a compact summary. To avoid confusion, I state that standard errors are smaller than the marker size in the plotted range.	
4	Improve predictability analysis in Fig. 12 (interpretation and methodological clarity).	I clarified that the analysis is an operational decorrelation-horizon experiment (not a Lyapunov-exponent estimate) and I report the median crossing time with interquartile-range (IQR) shading across an ensemble of realizations. I also explicitly limit the claim to the chosen perturbation size and metric.	Results (Predictability) + Fig. 12 narrative and caption (median + IQR; statement of scope/limitations).
5	Address limited Monte Carlo sampling across the manuscript (ensure reliability with uncertainty where appropriate).	For each statistical result, I now state the number of realizations and include an uncertainty summary (SE or bootstrap SE) when reporting a mean. When pooling events, I report the pooled sample size (event pairs) to make the effective sample clear.	Figs. 7, 10, 11, 13; captions and nearby narrative (realizations, pooled n, SE/bootstrap SE).
6	Broaden exploration of parameter space (loading rate, stiffness ratios, system size, distribution-level views).	Within the same reproducible framework and baseline parameter set, I added compact appendices that (i) sweep loading rate, (ii) compare system sizes, and (iii) report distribution-level CCDF summaries for slip sizes and waiting times pooled across realizations.	Appendices A3–A5 (loading-rate sweep; system-size comparison; CCDF distribution summaries).

		These extensions clarify generality without changing the core “toy model” scope.	
7	System-size considerations (N=50 baseline; demonstrate robustness to N).	I kept N=50 as the representative baseline for the main figures, and I added an explicit system-size sensitivity comparison that evaluates N=25, 50, and 100 under baseline parameters. I summarize the result as modest variation over this range, consistent with using N=50 as a representative baseline.	Appendix A4 + Fig. A4 narrative/caption; Table 1 notes column (system-size comparison reference).
8	Clarify overall contribution and ensure the paper is comprehensively resolved prior to resubmission.	I tightened the scope statement to emphasize a classroom-ready, reproducible toy model (not a new forecasting model). I ensured that integration, nondimensionalization, and event parsing are fully specified; figures/tables/captions are consistent; and a complete reproducibility package (code + parameter files + fixed seeds + environment notes) is included as Supplementary Material.	Introduction/ Abstract scope statements; Methods/diagnostics sections; Supplementary Material package and README.

I hope this revised manuscript is now fully compliant with the reviewer’s request for a comprehensive resolution of all outstanding issues.

The reviewer thanks the author for taking the time to address the reviewer’s concerns. Based on the latest review, I recommend this paper for publication.

What is missing from your paper is how your model can be purturbed and how this ties into actual real world seismology.
To this effect, I would like you to incorporate the content attached (as both pdf and libreoffice writer- same doc) into your manuscript. Please read carefully, ensure that it reflects continuity and is coherent with the mathematical model you have proposed. Also ensure that you include the references.

Response to Reviewer and Editorial Resubmission Instructions

Prepared for final JHSS resubmission

Manuscript: A Reproducible Burridge–Knopoff Spring–Block Toy Model with Explicit Numerical Diagnostics for Stick–Slip Dynamics

Comment 1 (verbatim reviewer comment)

"What is missing from your paper is how your model can be purturbed and how this ties into actual real world seismology.
To this effect, I would like you to incorporate the content attached (as both pdf and libreoffice writer- same doc) into your manuscript. Please read carefully, ensure that it reflects continuity and is coherent with the mathematical model you have proposed. Also ensure that you include the references."

Response

Thank you. The revised manuscript now explicitly explains how the Burridge–Knopoff model can be perturbed, defines a coherent perturbation-response framework inside the existing event-detection pipeline, ties that framework to real-world seismology, and adds the requested references. The inserted material was not pasted in as a disconnected add-on; it was integrated into the existing notation, logic, and scope of the manuscript so the revision remains mathematically continuous and appropriately qualified.

The substantive revisions are as follows:

- Abstract: added a sentence stating that the manuscript now includes an explicit perturbation-response extension based on a localized kick applied during the stick phase, quantified by cascade size, participation fraction, and mean amplification.
- Introduction: added motivation linking the toy model to stress transfer, earthquake triggering, and cascading rupture in natural fault systems, supported by the added seismology references (Refs. 14–21).
- Section III.5, "Perturbation-response extension": added a new subsection defining how the model is perturbed and how the response is measured with S_{cascade}, P, and χ while reusing the same catalog rules already defined elsewhere in the paper.
- Section IV.14 and Section V: linked the perturbation-response framework to the degeneracy result, location dependence, predictability discussion, earthquake triggering, Coulomb stress transfer, branching-ratio ideas, and the limitations of the simplified friction law.
- Conclusion: updated the summary so the perturbation-response extension appears as an explicit contribution of the revised manuscript rather than an isolated mid-paper addition.
- References: added Refs. 14–21 requested by the reviewer (Stein; King, Stein, and Lin; Ogata; Helmstetter and Sornette; Scholz; Ide, Baltay, and Beroza; Dieterich; Ruina).

These revisions directly satisfy the reviewer's request that the manuscript explain how the model can be perturbed, how that perturbation framework connects to actual seismology, and which references support that interpretation.

Comment 2 (verbatim editorial resubmission instruction)

"Dear author,
Please address all the comments of the reviewer and revise your manuscript accordingly. When you resubmit, please also submit separately a word file that lists verbatim each comment by the reviewer

and then describe how and where in the revised manuscript you have addressed that particular comment. Do this for all comments in one separate file.

Please note that if the the concerns/comments/questions of the reviewer are addressed inadequately, incompletely or insufficient address is paid to detail, your manuscript may be rejected. Please therefore thoroughly review your responses before resubmission."

Response

This separate Word file is submitted to satisfy that instruction. The reviewer comment and the editorial resubmission instruction are both reproduced verbatim above, and each is followed by a precise description of what was changed and where it was addressed.

To make the resubmission package consistent with both the reviewer request and the current JHSS upload format, the final package now contains:

- a narrative-only manuscript DOCX with tables retained in the text, plus its PDF companion;
- a complete manuscript DOCX/PDF with all figures and tables retained for full visual verification;
- a separate figure-legends DOCX/PDF;
- all figures exported as separate PNG files for upload as individual figure files;
- this separate response-to-reviewer DOCX/PDF;
- a brief QA checklist and submission README explaining the final file set.
- Submission-detail corrections were also checked one more time: the inline expressions $t_q = 1.0$ and $N = 25$, 50, and 100 were kept in stable math text; the synchronization paragraph no longer ends with a stray citation; and the code-availability sentence no longer claims that scripts are already included in the upload package.

Accordingly, the resubmission set now addresses the reviewer's scientific concern, satisfies the requirement for a separate verbatim response file, and is organized in the file structure expected for upload.

Thank you for addressing my comments. Accepted.