

Response to Reviewer 2: Our comments are provided in blue. Text modifications are provided in green.

The study provides a physics-based explanation for stress drops and earthquake nucleation using a simple elasto-plastic model, avoiding the need for complex frictional laws. The findings emphasize the importance of plastic deformation in fault slip mechanics, an aspect often overlooked in traditional models. The paper employs high-resolution 2D numerical simulations, carefully analyzing temporal and spatial resolution effects on stress evolution and earthquake nucleation. This methodology using GPU-based parallelization has great potential in achieving high-resolution earthquake modeling. I believe both the conclusions and the novel methodology used in this study should be promptly communicated to specialists and general audience.

We sincerely appreciate your thoughtful and constructive feedback on our manuscript. Your recognition of the significance of our findings and the potential impact of our GPU-based methodology is greatly valued.

In our revisions, we will focus on enhancing clarity and precision in our descriptions to ensure that the key concepts, methodologies, and results are effectively conveyed to both specialists and a broader audience. We are confident that these refinements will further strengthen the manuscript and improve its readability.

Thank you again for your valuable insights and for recognizing the relevance of our work.

Sincerely,
Yury Alkhimenkov, Lyudmila Khakimova and Yury Podladchikov

However, I found the writing (both texts and figures) quality can still be improved.

First, the text is in many locations verbose and repetitive, often explaining the same concept multiple times in slightly different ways. For example, the sentence line 275-279 is directly repeated in the next paragraph line 278-280. The phrase "finer temporal/spatial resolution leads to sharper stress drops" appears in sections 4.1, 4.2, 4.4, 4.5, and 4.9.1. The discussion (section 5) reiterates results (e.g., resolution impact, regularization, plasticity) rather than synthesizing new insights. The same applies to figures. For example, Fig. 16a is essentially the same as Fig. 14a, and is actually not described or mentioned in the text. Fig. 16b-d are largely overlapping. With only three sentences (line 343-348) describing this figure, I suggest the panels to be merged.

Second, the writing lacks conciseness. Many sections could be rewritten in a more direct and streamlined manner. Streamline results by grouping related findings (e.g., combine resolution tests 4.4-4.6 into one subsection with subheadings for temporal/spatial/regularization effects). Multiple figures show stress drop evolution

with slightly different grid resolutions, but the key insights do not change significantly. Additionally, the captions are overly descriptive, without highlighting the key takeaways (Figs. 2-17). Combine redundant figures to highlight the key messages. Use subplots to highlight contrasts (e.g., low vs. high resolution in Figs. 2-3) rather than separate figures.

I will add more suggestions on conciseness in line-by-line comments.

We agree with the reviewer that the quality of the text can be improved and made more concise. We have addressed the specific suggestions provided by the reviewer and revised the manuscript accordingly to reduce redundancy, streamline descriptions, and improve clarity. Additionally, we have reorganized sections where appropriate and adjusted figures to better highlight key insights.

Major comments:

1. Through section 4.4-4.6 and Figs. 5-8 the authors claimed that detailed convergence tests have been conducted. However, the convergence is not visually observable from the figures. I wonder if the authors have metrics to quantify the convergence and if the converging rate matches the analytical expectation. In addition, it is suggested in section 4.6 that the results are sensitive to the regularization parameter η_{vp} . If η_{vp} needs to be adjusted per model resolution, I wonder if this then still indicates the existence of model convergence. And if so, whether the authors could quantify the result sensitivity on η_{vp} .

We thank the reviewer for this insightful comment. Plasticity is a highly nonlinear problem—analogueous to turbulence in fluid dynamics—where traditional numerical convergence may not always be observed. Instead, what we assess is the statistical similarity of shear band patterns across different resolutions.

In our study, we show **trends**. For example, at low spatial and temporal resolution, the simulations do not show any stress drops. However, simulations with high spatial and temporal resolutions exhibit similar stress drops (both in number and amplitude). We agree that in the spatial convergence test, full convergence may not be reached; therefore, we have revised the wording and removed "convergence" from sections. Nevertheless, we clearly demonstrate the trend, which is the primary goal of this first study in this direction.

Regarding the regularization parameter η_{vp} , it must be adjusted to maintain a consistent shear band thickness across different model resolutions. We have now clarified this in the revised manuscript. Additionally, we note that a more detailed discussion on this topic is available in our previous publication in JGR: Solid Earth (Alkhimenkov et al, 2014).

Modifications in the text: We removed the word "convergence" and replaced it with "trends," along with some minor rephrasing for better flow.

2. The definition of “nucleation” is not clear. As a major topic as appeared on the tile, the term should be strictly defined. The interseismic loading and stress drop have been described, but it is not clear how nucleation makes the transition in between. Given that the time marching scheme in this study is implemented via strain increment, I wonder if more insights on the temporal behaviors of these processes (interseismic, nucleation, stress drop) can be added.

This is an excellent question, and we fully agree that the term *nucleation* should be explicitly defined in the manuscript. We think that in this early study we do not study nucleation process but only earthquake triggering. Therefore, we removed the word nucleation from the manuscript.

Triggering

Stress drop manifest the jump between the two quasi-static solutions and perhaps indicate the absence of a static transition between the stress state at the end interseismic period and onset of a new interseismic period.

This transition can be modeled with simplified linear elasto-dynamics by setting the difference in strain just before and after a stress drop as an initial condition for wave propagation.

3. Throughout the paper the simulated stress drop has been linked to “earthquake magnitude”. However, it is known from both numerical models and seismic observations that stress drop and magnitude are not (strongly) related. I found this extrapolation from varied stress drop to varied magnitude, and hence the stated link to the G-R law, too speculative. The authors also need to be careful when linking stress drop to seismic events.

We understand the reviewer’s concern and have revised the manuscript to adjust the wording accordingly. We acknowledge that stress drop and earthquake magnitude are not necessarily strongly correlated, as supported by both numerical models and seismic observations. In our revisions, we have taken care to avoid overgeneralizing this relationship and have refined our discussion regarding links to the Gutenberg-Richter law. We do not link stress drops to earthquake magnitudes in the revised manuscript. Regarding the distribution of stress drops, we now only describe it as G-R-like behavior.

Additionally, we emphasize that our study presents one of the first detailed numerical models demonstrating earthquake generation with a constant friction coefficient. This fundamentally differentiates our approach from previous models and highlights the novel aspects of our findings.

This insight suggests a resemblance to the Gutenberg-Richter-like law, which describes the frequency-magnitude distribution of earthquakes; however, a more detailed analysis is required to establish a direct connection, particularly from a plastic deformation perspective.

The periodic nature of stress drops, interspersed with slower periods of strain accumulation, mirrors the earthquake-like cycle seen in nature.

The paper needs a consistent coordinate system. Although the authors prefer a dimensionless computational system, it is not properly introduced in section 3.4. With $x, y \in [0, L_x] \times [0, L_y]$ stated at the beginning of section 3.5, the readers might be confused if the followed equations 22-27 were expressed in dimensionless coordinates or not. To add more confusion, the authors used no axis labels (Figs. 1), "Grid Cells (-)" (Figs. 2-3, 9-11), "x(-), y(-)" (Figs. 5-8, 12-13, 17), "x, y" (Figs. 15) in different figures, and in many cases without ticks. I suggest the authors to unify the expression and add ticks to the axis for better reference and comparison.

We understand the reviewer's concern and have improved the description of the initial model setup to ensure clarity in the coordinate system and its dimensional consistency. We have also revised the notation in the manuscript to make it more consistent.

Regarding the figures, we carefully evaluated different formatting options and found that our current choices balance readability and necessary detail without overloading the visuals. While full unification of all figures would make some of them too dense and difficult to interpret, we have made adjustments where possible to improve clarity and consistency, including adding axis labels and ticks where appropriate.

`\subsection{Model configuration, boundary conditions, and non-dimensionalization}`

The computational domain is a square with dimensions $(x, y \in [0, L_x] \times [0, L_y])$. All simulations in this study are performed using a simple initial model configuration and non-dimensional equations. To ensure a consistent dimensionless framework, we define the following characteristic scales: length $(l^* = L_x)$, time $(t^* = 1/a)$, and pressure $(p^* = G_0)$. Here, (L_x) represents the domain size in the (x) -direction, and (a) denotes the background strain rate. Deformation evolves over timescales inversely proportional to the initial background strain rate (a_0) at $(t = 0)$. The ratio of cohesion (c) to the pressure scale (p^*) is defined as $r = \frac{c_0}{G_0}$.

For all computations, we set the coefficient of internal friction to $(\mu = 0.6)$. Pure shear boundary conditions are applied by prescribing velocities at all boundaries in the dimensionless framework:

Minor comments:

1. Line 11: the usage of "decay" not accurate. Decay usually refers to a temporal process. It is not clear the decay is with what.

We agree with the reviewer and have revised the text to use a more precise term that accurately describes the intended meaning.

The histogram of stress drop amplitudes shows a non-Gaussian distribution, characterized by a broad peak with long tails on both sides,

2. Line 13: across which “scale” (temporal or spatial)?

We have clarified in the text whether the scale refers to temporal or spatial.

This "solid turbulence" behavior suggests that stress is redistributed across spatial and temporal scales, with implications for understanding the variability ...

3. Line 14: I doubt if the link to “magnitude” is proper here, see major comment 3.

We have revised the terminology and adjusted the wording to ensure accuracy and avoid misleading implications.

with implications for understanding the variability of stress drop magnitudes.

4. Line 49: I don't see the necessity of introducing heterogeneity here. Heterogeneity was only mentioned in the discussion of model limitation once. I suggest the whole paragraph can be eliminated.

We agree with the reviewer and have removed the paragraph for consistency

5. Line 90: D/Dt should be defined here already.

We have now modified this in the text.

where the Jaumann rate of Cauchy stress, represented as $\frac{D}{Dt} \sigma_{ij}$, is provided in the following section and the deviatoric plastic strain rate is

6. Line 177: is the model a “square” ($L_x=L_y$)? I can find nowhere the values of L_x and L_y .

We have added the values in the text to clarify the model dimensions.

The computational domain is a **square** with dimensions $(x, y \in [0, L_x] \times [0, L_y])$.

Here, (L_x) represents the domain size in the (x) -direction, $(L_x=L_y=1)$ and (a) denotes the background strain rate.

7. Line 193: connecting to comment 6, what does 0.2 refers to? Is the equation dimensionless or not?

We have clarified in the text whether the equation is dimensionless and provided a proper explanation of the value 0.2.

The expression is the following in the dimensionless framework:

8. Fig 1: axes should be marked and labeled.

We have added axis labels and markings for clarity.

We introduce a circular inclusion in the non-dimensional pressure (p) , representing a localized increase with the highest value at the center of the model. The expression in the dimensionless framework is as follows:

9. Fig 2: it is not clear where the “three different stages” were visualized. Could you mark them in panel a?

We have now marked the three stages in panel (a) for better visualization.

10. Line 242: which red circle?

We have improved the description to clearly indicate that the first red circle is being referenced.

where t_1 corresponds to the total strain just before the stress drop (first red circle)

11. Section 4.2.1: Mohr’s circle analysis is not really where your novelty locates. The section can be condensed.

This is one of the first numerical studies where stress drops are explicitly analyzed, with a detailed explanation using Mohr’s circles. Reflecting the reviewer’s comment, we have made the section more concise.

12. Sections 4.4-4.6: consider to merge

We have merged these sections to subsections for better organization and readability.

`\subsection{Trends}`

13. Line 261: I wonder if figure 9 shows a convergence. The results are not visually identical as claimed. Have you tried larger N? I also find the whole subsection a bit speculative. Terms like “too high” requires quantification

We have revised the text to be more concise and precise. Additionally, we acknowledge the reviewer’s concern and have clarified the interpretation of convergence while improving the wording of subjective terms. We removed word “convergence”.

Due to high regularization, the results are nearly identical and the thickness of the shear bands is the same in all panels. However, due to over-regularization, the stress drop is not visible.

14. Lines 278-280: repeating lines 275-277, should be eliminated.

We have removed these redundant lines to avoid repetition.

15. Fig 11: panel b is identical to Fig 10b. A different visualization is not needed.

We agree and have removed the redundant panel.

16. Section 4.8: this section is not pure results. Sentences like the first paragraph and lines 298-299 are either introduction or discussion materials and should have come earlier or later. I also wonder why the deformation mechanism in metallic material is mentioned in the end (line 304-306), which is largely off topic.

We removed lines 298–299. We believe that referencing deformation mechanisms in metallic materials is important to highlight the differences with rocks.

17. Fig 12: after “ $\psi = 5^\circ$ ”, the symbol of degree should be added, same below.

We agree and have added the degree symbol.

18. Line 323: is the set of notion (1-3) the same as (t1-t3) in section 4.3? Better keep consistent.

We agree with the reviewer and corrected the explanation.

Figure [\ref{Seq_evolzoom_UxOK}](#) shows the displacement increment $\Delta u_x = u_x(t_3) - u_x(t_2)$, where $u_x(t_2)$ and $u_x(t_3)$ represent the displacement fields at the beginning and end of the interseismic period, respectively (the period between two high-amplitude stress drops. Similarly, the displacement increments $\Delta u_x = u_x(t_2) - u_x(t_1)$ during major stress drops are shown in Figures [\ref{Seq_evolzoom_UxOK}](#)c-d.

19. Line 330: “leading up to” implies there is a causal relationship between the “aseismic slip accumulation” in interseismic and the “stress drop event”. This is not proved by the authors. Moreover, the general recognition would link any aseismic slip deficit in the shear band to the following earthquake. More elaboration is needed here.

We agree and have revised the text to avoid implying causality without proper evidence. We have also elaborated on the relationship between aseismic slip and stress drops to ensure clarity.

Our simulation results also demonstrate the material’s behavior during interseismic periods, where displacement gradually accumulates without significant stress drops. As shown in Figure [\ref{Seq_evolzoom_UxOK}](#), displacement increments Δu_x during interseismic periods increase progressively as loading continues. This behavior mirrors the slow, aseismic slip observed between seismic events in fault zones. The gradual buildup of displacement during interseismic periods reflects the loading of the fault system, while the rapid displacement during stress drops corresponds to seismic slip. However, our model does not explicitly establish a causal link between aseismic slip accumulation and subsequent stress drops, which requires further investigation.

20. Line 343: add reference to “solid turbulence”.

We have modified the text.

The histogram of stress drop amplitudes shown in Figure [\ref{Hist}](#) provides a

quantitative representation of the frequency and magnitude of stress drops occurring during the simulations. The distribution of stress drop amplitudes is notably non-Gaussian, characterized by a broad peak with long tails on both sides, indicating that while small stress drops are more common, larger stress drops still occur with significant probability. This distribution resembles turbulence-like spectrum, where a few large events (bursts) coexist with numerous smaller fluctuations. For solid systems, this phenomenon was first analyzed by \cite{poliakov1994fractal}, who explored the multi-fractal characteristics of shear bands in elasto-plastic media, that makes it similar to the fluid turbulence.

21. Fig 16: panel a is the same as Fig 14a, consider to remove. Panels b-d should be merged.

Figure 16, panel (d), contains four curves, all of which are different, whereas Figure 14a has only two curves. We believe that the merged panel (panel d) is more intuitive while still preserving the end-member cases that are well-separated—panels (b) and (c).

22. Line 356-357: the discussion on “magnitude” and the link to G-R law is not necessarily true. See major comment 3. Have you tried to calculate the magnitude of the events and check if it fits G-R law? It might be difficult because the simulations were 2D.

We agree with the reviewer’s concern. We have revised the discussion to avoid overgeneralizing the relationship between stress drops and earthquake magnitude, acknowledging the limitations of 2D simulations in directly comparing to the G-R law. The calculation of seismic events magnitudes is a topic for a future study.

This insight suggests a resemblance to the Gutenberg-Richter-like law, which describes the frequency-magnitude distribution of earthquakes; however, a more detailed analysis is required to establish a direct connection, particularly from a plastic deformation perspective.

23. Line 364: how is your “nucleation” defined? Does it align with others’ such as Rubin & Ampuero 2005? I also find it unfair that this term comes too late and is not extensively described in the results. It is one of the key features in your title and should be well addressed.

We have removed “nucleation” from the article.

24. Line 376: how is your “seismic event” defined? Do you see fast (m/s) slips in your model? Does your definition align with others’?

Seismic event corresponds to the wave propagation which is explained in the text section 4.3. In our model the transition from one quasi-static solution to another occurs as a “jump” (very fast, one or several loading increments) but we do not analyzed how fast they are in dimensional quantities in this early study.

Stress drop manifest the jump between the two quasi-static solutions and perhaps

indicate the absence of a static transition between the stress state at the end interseismic period and onset of a new interseismic period.

This transition can be modeled with simplified linear elasto-dynamics by setting the difference in strain just before and after a stress drop as an initial condition for wave propagation.

25. Section 5: reiteration of results should be removed so that the whole section can be streamlined.

We have condensed Section 5 to reflect the reviewer's comment.

26. Section 5.2: can you comment more on how the regularized parameter η_{vp} influence the results? Do you have a quantification for this? Also see major comment 1.

We have expanded the discussion of how η_{vp} affects the results and, where possible, provided a more quantitative assessment of its impact.

This is an excellent point. Indeed, with each refinement, we adjust the artificial viscosity (regularization parameter) proportionally to maintain a constant shear band thickness across different spatial and temporal resolutions. This rescaling is crucial for preserving the physical consistency of the localization process. The rationale for this approach is explained in detail in our previous study:

Y. Alkhimenkov, L. Khakimova, I. Utkin, Y. Podladchikov (202X), Resolving strain localization in frictional and time-dependent plasticity: Two- and three-dimensional numerical modeling study using graphical processing units (GPUs), Journal of Geophysical Research: Solid Earth.

Note that we re-scale the viscosity damper proportionally to the resolution in each simulation to maintain the physical thickness of the shear bands [\cite{alkhimenkov2024resolving}](#).

The numerical viscosity is usually set to a small value. If this value is too high, the shear bands become very thick; conversely, if the value is too small, the thickness of the shear band is just one pixel. The correct value of the viscosity damper lies between these limits. In the following section, we examine how the choice of viscosity damper affects the solution.

27. Line 396: you claimed that the 3D results you published earlier showed good agreement with the results in this paper. Could you elaborate more? I would expect clear differences between 2D and 3D simulations. Many numerical studies show that the third dimension has impacts on nucleation and rupture that are not negligible.

We did not analyze nucleation in 3D but rather observed that the general pattern of shear band patterns and the presence of stress drops occur in both 2D and 3D. However, we acknowledge that the magnitude of stress drops and rupture dynamics may differ in 3D. A proper analysis of these differences is an important topic for future research. 3D simulations are very computationally expensive and along deserve a separate study.

28. Sections 5.4-5.5: these comparisons to nature and previous models are useful. I wonder if you can further comment on how the weakening process occurred in your model differentiate itself from that in rate-and-state friction. Do they predict similar features such as some slip-weakening distance? Such insights would be inspiring.

We agree that relating our results to rate-and-state friction models would be valuable. However, to make a precise comparison, a more detailed analysis is required, which may be best addressed in a separate study. This comparison deserves a separate manuscript.

29. Section 5.6: largely repeating section 4.2.1, can be removed.

We removed this section.

We would like to thank the reviewer again for valuable comments, which helped us improve the quality of the manuscript.

Sincerely,
Yury Alkhimenkov, Lyudmila Khakimova and Yury Podladchikov