Dear authors,

I have heard back from the two original referees, and while you have addressed many points they raised, both referees still have brought up important concerns that should be addressed before the manuscript can be accepted. The most important points are the convergence of the results, theoretical estimates for the resolution that would be required for convergence, and the effect of regularization viscosity on structural weakening, as well as an explanation for the change in initial conditions and the corresponding update of the results.

I understand that the stabilization method involves changing the viscosity at the same time as the mesh is refined, but that still allows for performing an additional resolution test in which the viscosity is kept constant to demonstrate that the solution does not substantially change with a finer mesh for that given viscosity. If converged results are not feasible for all cases, it would be useful for the readers to have a framework outlining what resolution would be required in comparison to the current resolution, an overview over the required computational resources for that resolution, and a justification for why the presented models capture the relevant physical mechanisms even if the results still change with increasing resolution.

Best regards,
Juliane Dannberg

Dear Dr. Dannberg, Dear reviewers,

Thank you for your detailed and constructive feedback. We have carefully considered both your comments and those from the reviewers, and have substantially revised the manuscript to address the key concerns.

Our original goal was to demonstrate spontaneous stress drops and strain localization in a minimalistic elasto-plastic model, supplemented by additional closely-related material on structural softening and the mechanics of stress release. However, we recognize the seriousness of the concerns raised, particularly regarding convergence and the possibility of numerical artifacts. These comments prompted a major revision and refocusing of the manuscript.

We admit that the reviewers suggested very high standards during the revision of this manuscript and two month (revision time) is not sufficient to address properly all the questions raised. For example, a single loading increment of a simulation with a resolution of N=8,000 grid cells takes around 1 day using a professional modern GPU. We need hundreds-thousands such loading increments to address some questions raised by reviewers. Therefore, we have removed some material from the original version of the manuscript and focus in the new version on only key novel aspects and converged setup.

In the updated version, we provide a clearly defined initial geometry for the 2D model and present a convergence study that spans a wide range of spatial resolutions—from N = 63^2 to N = 2047^2—while keeping the regularization viscosity constant across simulations. Based on this analysis, we identify \(N = 1023^2\) as the minimum resolution at which both stress drop behavior and strain localization patterns converge for the considered simulation length. **All subsequent results in the manuscript, including the earthquake sequence and stress drop statistics, are based exclusively on these converged simulations.**

To ensure clarity and focus, we have removed several secondary and closely-related sections from the original version. To include these closely-related sections we need to run more simulations which will take several months or more, therefore, we will answer other questions in the following studies.

The revised manuscript now concentrates only on the core contributions:
**(1) convergence behavior**, and
**(2) the emergence of stress drops and earthquake-like sequences in a pressure-sensitive elasto-plastic medium.**

We note that we do not introduce a new model here but we adopt a regularization approach commonly used in geodynamic modeling (e.g., as proposed by Duretz et al. in Geophysical Research Letters) and apply it consistently across simulations.

We hope that these substantial revisions address the reviewers' concerns and meet the expectations for publication in Solid Earth. We appreciate your time and consideration, and we look forward to your response.

Best regards,

Yury Alkhimenkov, Lyudmila Khakimova and Yury Podladchikov

Response to Reviewer 1: Our comments are provided in blue. Text modifications are provided in green.

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

Major comments:

1. The authors made an effort to address my previous comments. The clarifications have exposed a major weakness of this work: the conclusions and discussions are based on numerical simulations that have not converged yet. In computational earthquake mechanics, conclusions are drawn from converged simulations. This has been the case at least since Jim Rice introduced in the 90's the distinction between continuum models and inherently discrete models. In the context of this work, converged simulations would be simulations in which the value of viscosity has been fixed and the spatial grid size and time step have been reduced (say, sequentially by a refinement factor of 2) until the difference between results of subsequently refined simulations become insignificant. In this manuscript, in many instances the viscosity is changed as the grid is refined and in other instances there is no evidence that the results have converged, which makes it impossible to draw conclusions. In very simple words: in the results presented, the reader cannot distinguish between meaningful results and numerical noise.

We thank the reviewer for this important observation. In the revised manuscript, we have addressed this concern by conducting a detailed and systematic convergence study. This study spans spatial resolutions from 63^2 to 2047^2, while keeping the regularization viscosity fixed across all cases. We followed a standard refinement strategy by doubling the number of grid cells and observed that the differences between results diminish with increasing resolution.

Based on this analysis, we identified 1023^2 as the resolution at which both the stress drop amplitude and the strain localization patterns converge. All results and figures in the updated manuscript are based solely on these converged simulations. We believe that this now firmly addresses the issue of distinguishing between meaningful physical behavior and numerical artifacts.

2. In computational earthquake mechanics, in models based on fault friction, the resolution criterion is that the grid size should be much smaller than the size of the process zone (Day et al. 2005, https://doi.org/10.1029/2005JB003813). Analogously, for regularized plasticity, a natural criterion is that the grid size should be smaller than the thickness of shear bands. Some of the papers cited on regularized plasticity (e.g. by Duretz) might contain theoretical estimates of the thickness of shear bands as a function of the assumed viscosity and other model parameters, which can form a basis for a resolution criterion to guarantee convergence.

We appreciate the reviewer's suggestion regarding the use of theoretical estimates for shear band thickness as a resolution criterion. In our convergence study, we keep the regularization viscosity constant and observe that in the converged simulations (1023^2 and above), the thickness of shear bands spans more than 10 grid cells. This indicates that the regularization is functioning as intended, and that the localized deformation is well-resolved. Based on these converged results, we draw our conclusions regarding stress drops and earthquake sequence behavior.

3. Also, if theoretical studies are available about the effect of regularization viscosity on the existence of structural weakening, those would be important to shed light on your simulation results, from a more fundamental perspective.

We agree that it may be possible to derive theoretical relationships between regularization viscosity and shear band thickness, and that such analysis could provide deeper insights into structural weakening. However, this is beyond the scope of the present manuscript. Our focus here is on the dynamics of stress drops and earthquake sequences.

We adopt a regularization approach commonly used in geodynamic modeling (e.g., as proposed by Duretz et al. in Geophysical Research Letters) and apply it consistently across simulations. We do not introduce any novel regularization method or a completely new model. We base our conclusions on simulations that have demonstrably converged under this regularized framework. We believe this is sufficient for the current scope, which centers on emergent earthquake-like behavior in pressure-sensitive elasto-plastic media.

Minor comments:

1. In earthquake research the term "triggering" is associated with seismicity caused by a loading different than the slow tectonic loading, for instance by static or dynamic loading due to another earthquake, or by tides, hydrological loads, anthropogenic loads, etc. This topic is not treated in this paper, thus the word "triggering" should be replaced to avoid confusion. For example, in many instances, it can be replaced by "occurrence".

We have changed the title in the revised version.

Stress drop sequences in the simplest pressure-sensitive ideal elasto-plastic media: Implications for earthquake cycles

2. Lines 34-5 (of the pdf with tracked changes): As I noted in my previous review, Andrews (1976) introduced simulations with plasticity in the bulk. He was clearly very far ahead of his time. To my knowledge this was the first paper modeling earthquakes with off-fault plasticity. I think the paper deserves to be cited in that context too, not only as a paper introducing slip-weakening.

We appreciate the reviewer's suggestion and fully acknowledge that Andrews (1976) was an important and pioneering work. However, after carefully reviewing the paper, we note that while it does introduce the concept of slip-weakening and mentions plasticity in the bulk, it does not provide a rigorous investigation of off-fault plastic deformation in the modern sense. Specifically, the model lacks an analysis of strain localization, shear band formation, or systematic resolution testing to demonstrate the effects of bulk plasticity. In contrast, the current study explicitly focuses on the role of plastic strain localization, including convergence testing, visualization of shear bands, and quantification of stress drop behavior. For this reason, in the context of our discussion on elasto-plastic models and strain localization, we have cited Andrews (1976) as the first work to introduce slip-weakening—an idealization consistent with perfect plasticity—but do not include it as a detailed model of off-fault plasticity in the same sense as recent works on strain localization in geodynamic and earthquake modeling.

3. Line 38: Ma (2008) and Ma and Andrews (2010) should be cited instead in the next paragraph, as perhaps the earliest studies of dynamic rupture with plasticity in 3-D (as I noted in my previous review).

Thank you for the suggestion. As noted in our previous response regarding Andrews (1976), we acknowledge that Ma (2008) and Ma and Andrews (2010) are among the earlier studies to incorporate plasticity in 3D rupture simulations. However, similar to the 1976 study, these works do not explicitly focus on strain localization, convergence analysis, or the systematic resolution of shear bands. While they represent an important step in modeling plastic deformation in 3D, our study emphasizes detailed visualization and resolution testing of plastic strain localization, which is not the primary focus in the cited works. For this reason, we continue to reference these studies in the context of early plasticity-based rupture models but distinguish our contribution by focusing on resolved shear bands and converged earthquake sequences in elasto-plastic media.

4. The discrete version of the regularization is presented in equation 30, but (as noted in my previous review) the continuum visco-plasticity equations that defined the regularized rheology should also be presented. I believe these can be taken from the cited references (e.g. by Duretz) The best place to present them is in section 2.3.

Implemented.

For the case of regularized plasticity, the the yield function is defined as
\citep{heeres2002comparison}:
\begin{equation}\label{A61H}

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F(\tau, p) = \sqrt{J_2} - \sin(\varphi) p - \cos(\varphi) c - \eta^{\mathrm{vp}}
\dot{\lambda}.
\end{equation}
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5. Section 3.4 is empty.

Corrected.

6. Line 186: define Go. Is it simply G?

Corrected.

7. Line 187: the background strain rate is defined only later, in equations 22 and 23. Reorganize the text in such a way that quantities are defined the first time they appear.

Corrected.

8. Line 194: I think you should remove “in the dimensionless framework” because equations 22 and 23 are not dimensionless.

Corrected.

9. Line 212: “integrated stress” appears before it has been defined. Reorganize the text to avoid that.

Corrected.

10. Lines 223-226, “The absence of stress drops in low-resolution simulations suggests that grid refinement is necessary ... In contrast, our sufficient resolution simulations with N = 1023^2 grid cells reveal several significant stress drops”: I suspect this is due, more fundamentally, to the effect of viscosity on the existence of structural weakening. However, this cannot be disentangled in your manuscript because you keep changing viscosity when you refine the grid, and the reader cannot tell if your simulations have converged. I think the proper way to study this problem is to fix the viscosity and refine the grid until convergence, then change the viscosity and repeat, and finally only show the converged results for each value of viscosity.

We performed exactly this experiment in the new version of the manuscript.

11. Lines 229-230: Here you could introduce and explain the concept of structural weakening, with proper references to fundamental papers on the topic.

We do not have this section in the new version of the manuscript. However, the references and the concept are still explained in the following section.

12. Section 4.4.1: Has convergence been achieved? Show also a simulation with strain increment 2e-5. Convergence would manifest as a decrease in the difference between subsequent pairs of simulations with a same refinement ratio of 2.

We performed exactly this experiment in the new version of the manuscript. We report now only converged results.

13. Line 407, "to the inability of strain localization to continue growing in the prescribed direction": this needs more explanation.

In the updated manuscript we have removed this sentence. We note that the reviewer have raised many important questions but it requires substantial work to proof the answers. Therefore, we focus only on the results that are based on converged simulations.

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

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

Thank you for your responses. I believe the manuscript is nearly ready for publication, pending a few minor revisions. Below are my comments on the revised manuscript:

Line 206: I observed that the initial condition has been modified. Originally, the authors defined a Gaussian-style cohesion, which has now been replaced by a stepwise pressure condition. However, the results have not been updated to reflect this change. I suggest the authors provide an explanation of the modifications made in this revision.

Thank you for pointing this out, in the present manuscript we used a cohesion inclusion. This has now been corrected in the updated manuscript, and all figures and simulation results have been regenerated to reflect the correct setup

Section 4.1: It appears that the low-resolution simulation was also conducted with a lower temporal resolution. This is not explicitly stated. However, it is evident from Figure 6 that temporal resolution plays a significant role. I recommend clarifying this in the manuscript.

We do not have this section in the updated manuscript. We report now only converged results.

Line 215: Could the authors specify the value of x0?

Corrected.

($x_0 = L_x/4$)

Line 269: The purpose of this newly added paragraph is unclear. The authors should provide additional context for this section, or, if no further explanation can be provided, consider removing it.

We do not have this section in the updated manuscript.

Figure 7: I noticed that the simulation with N=1023 exhibits a sharper stress drop compared to the higher-resolution simulations. This discrepancy likely relates to the choice of the regularization parameter. It would be helpful to include a brief note on this in the main text or the figure caption.

Yes, you are absolutely right. However, in the new version of the manuscript we show only converged results and do no analyze how viscosity affect the shear bands (this is outside the scope of the study).

Figure 8: This section seems less conclusive compared to the previous two. I suggest the authors indicate which of the three simulations they consider the most effective, along with an explanation of why. Additionally, it is claimed in the introduction that using a zero regularization parameter leads to localization in a single pixel. However, this is not clearly reflected in the figures. I question the validity of this claim and recommend adding a note about this observation in the figure caption.

We do not have this figure in the updated manuscript. In the new version of the manuscript we show only converged results. We do not present “trends” in the new version of the manuscript.

Sincerely,
Yury Alkhimenkov, Lyudmila Khakimova and Yury Podladchikov