Dear Editor,

We thank the reviewers for their relevant comments about the manuscript. You will find below our response to all the reviewers' comments, and some information about how we modified the manuscript following the suggestions. The reviewers' comments are in black, while our responses are highlighted in blue.

**Associate Editor:**

Dear Pierre Dublanchet, Jean-Arthur Olive:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Inelastic deformation accrued over multiple seismic cycles: Insights from an elastic-plastic slider-and-springboard model". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. Both reviewers point out the interest of this simple model in understanding the anelastic deformation during the earthquake cycle. You will find below the two reviews reports that make detailed comments and suggestion to improve your manuscript.

I will add to those valuable comments that although simple, the proposed model cannot be linked intuitively to a real fault zone. A effort could be made in this respect by explaining (with a sketch?) how the geometry proposed mimic a fault and its surroundings. In particular, I am concerned that a quick reader might make an erroneous analogy between the subduction zone presented in Fig.1 and the model geometry with a bended springboard shown in Fig. 2a.

Thank you for the suggestion. We removed Figure 1, in order to avoid the confusion between our model and real subduction geometry. We also made it clearer in the model description (L 133-138) that the model does not represent a fault zone (as noted by reviewer A, our phrasing may have been unclear in places), but a plate boundary, where lithospheric blocks come in contact at a fault zone. The springboard represents one of the blocks, and the contact with the rigid wall is the fault zone itself. We also mention more clearly at the beginning that the model could be seen as a simple improvement of an elasto-plastic spring-and-slider system, where a length scale (plate length, and thickness) has been added. However, since we cannot relate this to a real fault geometry (normal, subduction etc.), we propose to only keep Figure 2a (1a in the new manuscript) for the model description, highlighting now what is the fault zone and the fault block.

**Reviewer A**

Review of the article titled:
“Inelastic deformation accrued over multiple seismic cycles: Insights from an elastic-plastic slider-and-springboard model.”

This article investigates accumulation of permanent plastic deformation over long-term seismic cycles. It uses simplified numerical and analytical models of slider-and-springboard. The results highlight the parameter space involving bulk strength, fault strength, and stress drop that control fault and bulk deformation. Though the model is intensely idealized for natural earthquakes, the article contains important ingredients towards understanding the interacting factors associated with the long-term evolution of frictional instability and bulk deformation. I recommend revision by addressing the following points before publishing.

General comments:

- In the model setup section, it is mentioned that "the thin plate represents the behavior of a finite width fault zone." However, the accumulation of the permanent inelastic deformation is found more concentrated near free surface at left boundary (fixed end) as shown in the results. In natural fault zone, damage or inelastic deformation is more concentrated near the fault and decreases away from the fault. This suggests a contradiction with the results presented in this article and questions the relevance of the model setup to the fault zone. Can the authors provide clarifications regarding this in the model set up section and further explains in the discussion section pointing out possible implications of the assumptions made in the model?

Our phrasing was unfortunate in this sentence: the term fault zone is not appropriate. We are not interested in dynamic damage accumulation close to the fault, but in distributed permanent deformation accumulating in a quasi-static manner in fault blocks while finite offset is accumulated on the fault. This part of the model setup has been reformulated to clarify this point (L 133-138).

- The geometry of the model specially the length (L) of the plate turns out to be an important parameter here. L affects the distance between the fixed boundary (left) and the frictional interface (right). L is an input to both the stress ratios presented in this study. The equation of effective stiffness presented here shows stronger dependence on L than H. I wonder whether the authors have simulated different cases by varying L which may show how it influence the results.

In the current state of the model, L is the only horizontal length scale, and the behavior is essentially controlled by stress ratios involving L/H, as shown by the analytical predictions. We simulated two different values of L/H, and overall the numerical results are in agreement with the analytical predictions, so we do not think testing different L values will bring more information here.

However, this question will be important if buoyancy effects are introduced, through a vertical force involving the density contrast between the crust and the mantle. In this case, the response will probably depend on the ratio between L and the flexural wavelength (4D/Delta rho g)^(1/4). The flexural parameter controls the maximum extent of the elastic deflection generated by a point load on the plate. We therefore added a paragraph discussing the implications of replacing L by the flexural parameter in the discussion section (L 692-702).

In rate-and-state friction law, $V$ is the reference slip rate which is not necessarily the $o$ imposed plate rate at the boundary ($v_o$). It seems from Equation (24) that this study has taken the reference slip rate as the plate rate. Is there any specific reason behind this? The results demonstrate that the plate rate is one of the controlling factors of accumulation of the inelastic deformation. $v_o$

has also been used to nondimensionalize the rate of inelastic deformation. Is it constant or varied in different cases? The values of reference slip rate and the plate rate are missing in the table of input parameters.

For simplicity, we considered the reference slip rate equal to the plate rate. Changing the reference slip rate used in rate-and-state friction definition would be equivalent to only changing the reference friction coefficient f0. v0 in the scaling derived indeed does not arise from the definition of the friction coefficient (eq. 24) but from (eq. 2) . The effect of f0 (and thus of the reference slip rate) is already captured by the stress ratio Rsigma. We have furthermore simulated different values of f0 (0.4 and 0.6) showing a good agreement with the theoretical scalings. Again, testing other values of the reference slip rate will probably not bring new critical information.

The value of v0 is constant (about 3 cm/yr) for all the simulations presented here (the value has been added in table 1). The effect of v0 is also captured by the analytical scalings. Furthermore, the elasto-plastic model, and in particular the yield stress envelope does not depend on strain rate here.

- In Figure-4, the plastic yielding is shown before the start of stick-slip phase. How does the plastic deformation evolve later in the cycle? Also, it is not clear overall that how the coseismic plastic deformation evolves with time and how it is different from interseismic phase.

Figure 4 has been modified to provide more information about the evolution of plastic deformation over the succession of earthquake cycles, during the inter seismic and co-seismic phases. We also show the evolution of the elastic core thickness through time. We added a comment about the evolution of interseismic and coseismic yielding beyond the first loading phase (L. 287-288 and L. 295-299)

- All the simulations consider rate weakening friction, right? Then the legends in Figure-7, Figure-9 and other places should have ‘b-a=xx’ instead of ‘a-b=xx’.

We indeed only consider velocity weakening. The figure captions have been modified.

- To avoid confusion, in all the figures’ x and y axis labels, it would be better to separate the text and symbol by using comma, brackets or equal sign like in equation.

We added a coma in all axis labels.

- Line 14-17: “Our model produces stick-slip cycles consisting of interseismic plate downwarping and coseismic plate upwarping as long as the moment of the frictional force at the contact exceed the maximum (purely plastic) bending moment the plate can sustain.” It reads contradictory with Line 494-495: “Finally, if the moment of the frictional force exceeds the plate's maximum bending moment ($R\sigma \leq 1$), the bulk of the plate fails and seismic cycles cannot occur on the plate interface."

Thank you for reporting this typo. We meant "does not exceed" (L. 14-17). This has been corrected.

- Line 14-17: no need to use symbol for yield stress. Use comma in the list.

This has been corrected (L. 14-17)

- Line 19-21: "We show that non-recoverable plate deflection accumulates over successive earthquake cycles only if the plate's yield strength decreases through time, causing a progressive decrease of the aforementioned stress ratios." Line 523-524: "The only way to create permanent deformation that accrues over successive earthquake cycles is to alter the fault's frictional properties, or the plate's elastic parameters or yield stress envelope through time." These should be rephrased to avoid 'only' as there are alternatives.

These sentences have been rephrased (L. 19-21 and L. 558-559).

Line 248: What is the value of $\theta_0$ when shear force is zero at the initialization? What is the initial slip rate?

This has been clarified (L. 264-269).

Line 291: It would be better to have a brief explanation here why seismic cycle duration is longer for elasto-plastic case.

A sentence has been added (L. 316-318). The longer cycle duration is related to a slower inter seismic elastic energy accumulation, because of the plastic dissipation.

Figure-4(a-b): Any reason to make the y-axis (time) increasing downwards? Also, it might be better to include plot in terms of simulation time step to show the coseismic phase.

We changed the orientation of the y axis in Figure 4. We also added plots where the y-axis corresponds to simulation time step, which better highlight sudden slip events

Figure-6: Are the solid and dashed lines overlapped? I cannot locate any dashed lines in 6(a). The color bar does not have any red color, but the lines have. Is there a reason to cap the color bar?

The solid and dashed lines do not exactly overlap, but the difference (shown in panel b) is very small. We now use dotted lines in panel (a) to clarify the distinction between purely elastic and elasto-plastic deflection. We also modified the color bar.

Line 616-617: "...distance to fault..." or "...distance from fault..." ?

This has been corrected (L. 654)

Line 902: Data and code availability section: is the citation *Dublanchet and Olive (2023)* a peer reviewed paper? If not, the citation can be replaced by providing the doi link directly.

This is not a peer reviewed paper but an online repository, so we replaced the citation by the doi link (L. 957)

**Reviewer B:**

This manuscript uses a simple 1d flexural model to make some inferences regarding the accumulation of permanent deformation over repeated earthquake cycles. The paper presents some numerical results along with more formal analysis to back up their numerics. While I feel that the authors have obtained a very good understanding of their model, I wonder more to which extent the model results can be applied to nature. Nevertheless, I find the study very interesting and it is certainly worth publishing.

In general, while I accept that within the framework of the model, stick slip may give way to bulk failure as damage accumulates, I am less convinced that this occurs in nature. I also think it's still open as to when permanent deformation actually accumulates over an earthquake cycle. The authors consider it accumulated during the interseismic period, but is this necessarily the case ? What about the possibility for permanent deformation produced during earthquakes ? These are interesting questions (at least to me), and maybe the authors can shed a little more light on these issues in the discussion.

We indeed focus on the accumulation of permanent deformation during inter seismic loading, as motivated by the recent study of (Oryan et al. 2024 Science Advances) for instance, and by the observed correlation between morphological features and interseismic deformation observed by geodesy (L. 45-50). Since we were interested in this distributed damage, we did not consider the dynamic coseismic damage generally localized close to the fault or close to the earth surface (Baker, 2013;Rodriguez, 2022).

We however do not completely rule out the occurrence of coseismic yielding: our yield-strength envelope is symmetric, so that yielding also occurs (to a lesser extent) during the coseismic phase, as shown in Figure 4, but again, it is not localized close to the fault and can not be viewed as coseismic damage in the sense of, e.g., Thomas & Bhat 2017.

We have reformulated different parts of the introduction and the discussion to clarify the physics modeled (interseismic yielding), and to explicitly mention the possibility of co-seismic yielding, which would need to be included in future models. See L. 60-63, L. 133-138, L. 201-203, L. 540-557, L. 645-647, L. 643-653.

Below I have listed more detailed points that arose during reading that might serve to make some improvements to the manuscript.

Why is the term springboard used, as opposed to flexural? I understand the analogy, but is this usage standard ? It stuck me as a little strange.

We made this choice thinking it could be illustrative, but we can change it, if it really poses a problem.

Paragraph starting at Line 52 (plus line 67, 72): a significant issue here is when this permanent strain is accumulated? (during earthquakes, e.g. Baker et al 2013 or the interseismic period). See also Simpson (2024, tectonophysics).

The question of when permanent strain is accumulated is now evoked here, with references to (Baker, 2013), (Simpson, 2024) and a recent paper by (Oryan et al., 2024) showing the accumulation of inelastic deformation during the interseismic stage of megathrust earthquake cycles in different subduction zones (L. 60-63).

Paragraph starting at Line 72 : once again, the study of Simpson (2024) is relevant here, since this study considers spontaneous emergence of faults and earthquakes over times scales from seconds to hundreds of years (i.e., multiple seismic cycles).

The study (Simpson, 2024) is now referenced in this paragraph (L. 103-106).

Line 122 : I imagined that the thin plate represents the elastic crust adjacent to a fault, but this doesn't seem to be what you are stating here ? Also, on line 124, 'most of the seismic cycle' occurs in the fault core ? I don't understand what is meant.

This paragraph has been reformulated, following the first comment of reviewer A. Our phrasing was inadequate here, the thin plate does not represent the zone immediately adjacent to the fault (i.e. the fault zone where coseismic damage accumulates), but the lithospheric fault blocks surrounding a fault zone (L.133-138).

Line 207 : Maybe better to write : 'can be written as' (see also line 215)

Corrected (L. 225-232)

Line 217 : describes

Corrected (L. 234)

Line 374: I'm not sure I agree with the statement: ...no obvious mechanism that would drive long-term, uniform changes in the frictional properties of a tectonic fault. Geological studies of exhumed faults show that in many instances, fault rocks have anomalously low friction coefficients due to the presence of phases like phyllosilicates - e.g., see Carpenter et al. 2012, Geology, doi:10.1130/G33007.1. These weak phases are presumably formed over quite long time scales in response to fluid-rock interaction.

This paragraph has been reformulated in a more nuanced manner: there are several possibilities, we explore one of these (L. 400-406).

Line 277-: Yes, but the deflection you compute is only on the lower downwarping plate, which is normally not analysed by geologists. They focus entirely on the emerged upwarping plate. Is this difference relevant? (for example, when comparing results with nature, e.g., section 4.2 and Fig 11).

Sorry, we do not clearly see how this statement specifically pertains to l. 277 (the line number may be incorrect), or . Anyhow, thespringboard is not meant to model a lower or upper plate specifically. It does not relate to a specific geodynamic context (subduction, strike slip, normal fault): it is just a very simple representation of a fault block in general, that could be viewed as the most simple improvement of an elasto-plastic spring-and-slider system, in the sense that it incorporates a length scale normal to the fault.

This point has been clarified in the model description (L. 133-138).

Line 509: I understand that because no permanent deformation accumulates, the authors consider a changing yield stress. But this seems to be a model problem, not necessarily relevant to nature? For example, in my experience with elastic plastic models (with constant yield stress) permanent deformation does continue to accumulate. Thoughts?

The absence of permanent deformation accumulation under constant yield envelop seems logical within the framework we defined, but we agree that this framework (thin plate scraping on a vertical wall) may not be the most representative of real-world systems. It is possible that the geometry, or the absence of gravity (that should result in a vertical buoyancy force related to the density contrast between the crust and the mantle) prevents the accumulation of permanent deformation over successive cycles. This issue is already pointed out in the discussion section (Future directions L. 683-702). We also acknowledge that we might have missed an important physical feature, that is present in your model. To go further on this issue, we would be interested to have the reference of the model you are citing.

Line 513: The statement “Coseismic deformation is fast (seconds to minutes), and much shorter than the characteristic deformation time needed to activate inelastic processes (e.g., the Maxwell time of the lithosphere)” is only true for viscous-like inelastic deformation. However, earthquakes generate considerable fracturing in the surrounding rocks (a type of plastic deformation) which does not depend on time. So while a large part of coseismic deformation may indeed be elastic, a fraction may be permanent (plastic, of Coulomb type). I think care needs to be taken in the discussion here, and that the possibility of time-independent permanent deformation (potentially coseismic) should be considered.

This part has been reformulated in a more nuanced manner, clearly making the distinction between viscous-like and time-independent coseismic permanent deformation, which is now mentioned. Our message here is that the preferential activation of viscous like processes during inter seismic can motivate an asymmetry of the yield envelop, but does not suppress the occurence of coseismic yielding. Therefore we also have removed the sentences suggesting a purely elastic coseismic deformation (L. 540-557).

Line 523: I assume you are referring only to your model, otherwise the statement is too general to be correct.

Correct, we are referring only to our model. This sentence has been reformulated (L. 558-559).

Line 530: I’m not sure that Bhat et al., 2012 is an accurate reference for the preceding statement, since their failure is not the result of changing elastic properties. More generally, I could even imagine a scenerio when decreasing elastic properties (say shear modulus) could move the stress state further form failure.

We do not claim in this sentence that the decrease in elastic properties leads to failure, but that it could be a diagnostic of failure (« It manifests as a change in elastic properties ») . Stress still accumulates, but more slowly.

Line 541, I not sure what “they” is referring to.

Large values of the fatigue parameter. The sentence has been rephrased for clarity (L.577).

Section 4.2: In general, it appears clear that the authors consider that most permanent deformation is generated during the interseismic period. Some other authors share this view, personally I am not convinced (See also the old classic papers like King et al, 1988). Can the authors really rule out that a large portion of permanent deformation produced over the earthquake cycle is not produced during or shortly following earthquakes?

Following the first comment: we have reformulated different parts of the introduction and the discussion to clarify the objective of the paper, and to mention also the possibility of co-seismic yielding. We re-emphasize, however, that co-seismic fracturing should be confined to the fault zone and to very shallow levels of the crust, where fracturing thresholds are lowest.

Line 557: Continuing on the same theme, why does non-recoverable uplift necessarily shape the landscape on time scales of 100kyr (why not also at much shorter scales?).

We only mean that the process is so slow that we need a long observation window to detect significant deformation. This part has been reformulated accordingly (L. 594-595).

Overall, especially in this section, I think more care needs to be taken. Just because in the model progressive weakening (drop of \sigma_y) of the beam may mean that sliding eventually becomes stable doesn’t necessarily mean that this scenerio actually occurs in nature.

The progressive weakening of the yield stress does not lead to a transition to stable slip, since this transition is controlled by the ratio k/k_c, that does not involve the yield stress (section 4.1.1). However, the weakening can lead to bulk failure in the plate (fault block), which likely does not occur in nature, but could be replaced by the creation of a new major fault further away in the plate. This is already mentioned in the manuscript (L. 615-624)

Having said that, I commend the authors for trying to apply their model to natural systems, never an easy task.

Recommendation: Revisions Required