

Response to Reviewer 2

We thank Reviewer 2 for the detailed and constructive review. The comments motivated additional physical framing, frictional-parameter and forcing-shape sensitivity tests, an explicit spatial-transition diagnostic, a reorganized Discussion, and a cleaned figure set. Corresponding changes in the manuscript are highlighted in blue.

Reviewer comment: This manuscript proposes a modified state-variable evolution model for the classic rate-and-state friction (RSF) aging law that accounts for a delayed state response. The study shows that incorporating a delay time t_c into the state variable and slip rate in the classic aging law modifies both the temporal and spatial evolution of slip during the nucleation stage, including the maximum slip rate and the emergence and extent of a high-velocity core. The authors further introduce a new dimensionless parameter, Π_d , that relates the delay parameter t_c to the spatiotemporal evolution of the high-velocity nucleation core. The manuscript has novelty and potential contribution to the field by proposing a simple constitutive model that captures a delayed update of the state variable. However, several important concerns need to be addressed before publication.

Response: We accept this suggestion. We have substantially expanded the physical, theoretical, numerical, and robustness basis of the study. The revised analysis now combines time-step and grid convergence, an explicit linear-stability calculation, near-critical finite-amplitude tests, equal-stage comparisons, alternative temporal architectures, a/b sensitivity, and stress-perturbation-shape sensitivity. The central dynamical quantities are the critical nucleation/stability length, modal and nonlinear growth tendency, and response timing; threshold-defined high-velocity contours are retained only as descriptive diagnostics.

Reviewer comment: My main concern is that the physical, observational, and theoretical basis for the motivation and interpretation of the study is not yet sufficiently developed. In particular, it is not clear why a finite constitutive response time needs to be introduced, or how this modification helps reproduce important observations or theoretical predictions. The authors state that the instantaneous update of the state variable is “convenient and often appropriate, yet it is not equivalent to demonstrating that the physical processes represented by the state are instantaneous”, while an in-depth review of previous work discussing when this approximation is expected to be appropriate is missing.

Response: We accept this suggestion. We now describe the conventional ageing law as a local-in-time (Markovian) closure: the state-evolution rate at time t is evaluated from the current slip velocity and state, although the state variable itself evolves over a finite time and slip history. The revised Introduction places this assumption in the broader RSF literature, including contact-age/area, gouge-structure, thermally activated, and microphysical interpretations of state. We define t_c narrowly as a phenomenological coarse-grained response time of an unresolved internal reorganization process, distinct from D_c . The input ratio $\chi = t_c V_s/D_c$ provides an explicit criterion: when $\chi \ll 1$ the local-in-time closure is the natural leading-order approximation. For the Master input-derived state, χ_Δ is approximately 5.7×10^{-3} at $t_c = 0.12$ s, consistent with the small converged effect in that configuration.

Reviewer comment: A related issue is the emphasis on the development of a high-velocity core within the nucleation zone. Given the lack of target observations or theoretical predictions supporting the presence or importance of such a core, it is not yet clear why this behavior should be considered a central feature that makes the proposed formulation distinctively important.

Response: We accept this suggestion. We clarify that the high-velocity inner contour is not treated as an established seismic or geodetic observation that the model is intended to reproduce. The physically relevant targets are instead the intrinsic nucleation/stability length, characteristic-root growth rate, acceleration/turnaround timing, and the phase lag of state adjustment. The 10^{-2} m/s contour is retained only as a descriptive diagnostic in the Supplement. Equal-stage comparisons further show that the normalized spatial profiles nearly collapse at matched accumulated slip, so the revised localization analysis is based on the intrinsic stability scale and evolutionary stage rather than on a presumed observational core.

Reviewer comment: In addition, the current Discussion section reads more like an extension of the Introduction, with continued emphasis on justifying the proposed model rather than discussing its physical implications and possible links to observations. For example, the physical meaning of t_c remains unclear. Because the classic RSF formulation is largely empirical, I am not sure how introducing an additional parameter without a clear physical meaning, and without support from a specific microphysical framework, would improve the physical relevance of the friction law. Delay parameters or thermal dependences, as introduced by Ikari et al. (2016), Ende et al. (2018), and Barbot (2022), aim to address this type of problem. A more focused discussion of what t_c represents physically, and how it may be constrained or tested, would substantially strengthen the manuscript.

Response: We accept this suggestion. The Discussion has been reorganized into physical interpretation and observable targets, robustness to frictional properties and forcing geometry, and testability/limitations. We distinguish t_c from D_c and discuss physical interpretations of state and time-dependent frictional strengthening using a broader literature set, including Nakatani (2001), Carpenter et al. (2016), Ikari et al. (2016), van den Ende et al. (2018), and Barbot (2022). We also test the physical architecture directly: a causal exponential time-relaxation model reproduces the discrete-delay stability shift and near-critical retiming, whereas a matched slip-controlled relaxation does not. This provides a concrete experimental target for large velocity-step, slide-hold-slide, or spatially resolved nucleation tests that distinguish elapsed-time from slip-controlled response.

Reviewer comment: The authors outline several useful follow-up directions in the Conclusions section (L423-L443). However, in my view, most of these points should be addressed in the present study rather than deferred to future work. In particular, additional parameter exploration seems essential. For example, the a/b value used in this study appears to be ≈ 0.67 . As highlighted by Rubin and Ampuero (2005), a/b values can affect the style of nucleation. Testing different shapes of the imposed stress perturbation would also help clarify the robustness of the reported behavior.

Response: We accept this suggestion. We moved the forward-looking material from Conclusions into the Discussion and carried out two of the requested parameter studies in the present revision. First, a/b is varied from 0.40 to 0.90 while the input-derived response speed is held fixed. The classical critical wavelength changes from about 1.70 to 4.44 km, confirming the strong control of a/b on nucleation style, whereas the fractional $t_c = 0.12$ s stability shift remains approximately -0.285 % throughout the sweep. Second, we compare raised-cosine, Gaussian, compact-quartic, and super-Gaussian stress perturbations normalized to the same peak and integrated stress increment. The absolute nonlinear crossover varies strongly with forcing shape, but the finite-response shift retains the same sign for all four profiles. These tests are summarized in the main text and documented in the Supplement.

Reviewer comment: Another concern is the limited set of references. The subjects of RSF and earthquake nucleation have been extensively studied over the past few decades, yet the manuscript cites fewer than 40 references, with substantial repetition in the Introduction and Discussion sections. Expanding the reference

list would better acknowledge existing work and would help place the proposed model in a clearer scientific context. It may also help strengthen the motivation and interpretation of the study.

Response: We accept this suggestion. The reference list and literature framing have been expanded substantially, especially for physical interpretations of state, laboratory frictional healing, microphysical and temperature-dependent formulations, extensions of state evolution, and nucleation sensitivity to the state law. New references include Nakatani (2001), Baumberger and Caroli (2006), Linker and Dieterich (1992), Nagata et al. (2012), Bhattacharya et al. (2015), Carpenter et al. (2016), and the already cited microphysical studies are integrated more directly into the motivation and Discussion.

Reviewer comment: Therefore, I suggest major revision, or possibly reject-and-resubmit, to allow the authors enough time to substantially revise the manuscript.

Response: We accept this suggestion. The manuscript has been substantially revised and is accompanied by expanded Supplementary Material and reproducibility files. The revision adds the requested physical framing, parameter and forcing robustness tests, extended-time calculations, convergence tests, and a reorganized Discussion and figure set.

Specific comments

Reviewer comment: The usage of the term “reduced-order” needs caution. It could be associated with surrogate models, which is not the case here. I suggest being more specific about what the authors intend to convey when using this term.

Response: We accept this suggestion. We removed the term “reduced-order” where it could imply a surrogate or projection-based model. The mechanics are now described specifically as an effective one-dimensional scalar shear-layer model, or equivalently as the continuum limit of a nearest-neighbour block chain. H is described as an effective-layer thickness parameter.

Reviewer comment: L273 reads as if this is an observational feature in current seismic/geodetic instrumentation. I suggest rephrasing this sentence.

Response: We accept this suggestion. The relevant sentence has been rephrased so that no high-velocity inner core is presented as an established feature of current seismic or geodetic observations. The revised text identifies theoretical and experimentally testable targets instead: nucleation length, growth rate, acceleration timing, and state-response lag.

Reviewer comment: What is the rationale for choosing the simulation time $T = 12$ s? In Figure 4, V seems to keep increasing over time. This makes me wonder whether, if the simulation were run for longer than 12 s, the internal core in the instantaneous response model ($t_c = 0$) might also grow above the dynamic threshold.

Response: We accept this suggestion. The 0-12 s interval is now described only as a common finite-window used for the original response-time sweep. It is not treated as a physical termination criterion. We added converged extended calculations through 16 s, using a 120 km domain where necessary to exclude periodic return. In the converged 2 km Master case the true velocity maximum occurs during the initial direct-effect transient, and the center velocity subsequently decreases through 16 s for both $t_c = 0$ and 0.12 s. Thus the instantaneous case does not cross the 10^{-2} m/s descriptive threshold when the calculation is extended.

Reviewer comment: I am also not sure whether “simple” is a critical virtue for proposing a new state evolution law. Numerically, the aging law is already a well-adopted framework for explaining existing RSF experiment results and is reasonably efficient computationally.

Response: We accept this suggestion. Simplicity is no longer presented as a physical or numerical virtue of the proposed state evolution. The motivation is instead the isolation of a distinct time-controlled response hypothesis and the ability to test its stability consequences and its robustness to alternative causal temporal architectures.

Reviewer comment: I do not understand L117-118, where the authors state that the model accounts for “inertia” while using radiation damping. Radiation damping approximates inertial effects while ignoring the actual term in the elastodynamic equation for computational efficiency. This part needs clarification or revision.

Response: We accept this suggestion. The mechanical formulation has been rewritten and derived explicitly. Distributed inertia is retained through the acceleration term $\rho \mathbf{u}_{tt}$ in the scalar momentum balance. The separate $\eta(V-V_{\text{init}})$ term is described as a local radiation-loss closure representing energy carried from the effective layer into unresolved surrounding material. We explicitly note that this is not the quasi-dynamic construction in which radiation damping substitutes for the inertial term, and that the model is not a full elastodynamic boundary formulation.

Reviewer comment: Is there an explanation for the sharp transition of the slip rate from the background to the accelerated nucleating zone at $x = \pm 0.5$ km, as shown in Figures 3 and 5? Since the stress perturbation is imposed over $x = \pm 1$ km, I would expect a somewhat smooth connection from the background slip rate to the accelerated slip rate within the $x = \pm 0.5$ -1 km range, similar to, for example, Figure 1 in Rubin and Ampuero (2005).

Response: We accept this suggestion. We performed a dedicated spatial-transition diagnostic. The feature near $x = \pm 0.5$ km is not a frictional boundary: the raised-cosine stress perturbation extends smoothly to ± 1 km and has half its peak amplitude at ± 0.5 km. Because the RSF direct effect is logarithmic in velocity, this smooth stress taper maps into a multi-order-of-magnitude velocity contrast. In the converged calculation, V at 12 s decreases continuously from approximately 1.73×10^{-8} m/s at 0.5 km to 8.5×10^{-10} at 0.6 km, 5.7×10^{-11} at 0.7 km, and approaches the 10^{-12} m/s background only near 1 km. The $t_c = 0$ and 0.12 s outer tails are essentially identical. We therefore clarify that the visually sharp transition is not a physical discontinuity or a delay-created boundary.

Reviewer comment: How did the authors determine the time step size? I see that the authors tested two values, but the manuscript does not explain how they chose them.

Response: We accept this suggestion. The Methods now state that the reported reference time step $\Delta t = 1.25 \times 10^{-4}$ s was selected from a temporal-refinement sequence rather than chosen a priori. It is checked at 6.25×10^{-5} s, and independent semi-implicit and deeply refined explicit implementations converge to the same solution. The principal near-critical timing and architecture results are also unchanged under time-step halving.

Reviewer comment: I am concerned about the forced nucleation setup. The manuscript should more clearly explain how the imposed stress perturbation affects the resulting nucleation process, and whether the reported high-velocity core is controlled by the proposed delay formulation or by details of the imposed perturbation.

Response: We accept this suggestion. The revised manuscript now separates the imposed forcing scale from the intrinsic stability scale. At $H = 20$ m the 2 km Master perturbation is only $0.869 L_c$ and is subcritical in the converged model. We added a perturbation-size sweep around L_c , equal-stage profile comparisons, and a new forcing-shape sensitivity test. The shape test shows that forcing geometry strongly controls the absolute nonlinear crossover, while the smaller t_c -dependent shift retains the same sign across four matched shapes. We therefore state explicitly that the imposed perturbation is a first-order control on the absolute nucleation trajectory and that the robust constitutive effect is the smaller near-critical stability/timing shift.

Technical corrections

Reviewer comment: A number of terms are not defined before being used. A non-exhaustive list of such examples is:

Response: We accept this suggestion. Definitions have been moved forward and the revised manuscript and Supplement now define the requested quantities at first use.

Reviewer comment: The concept of “velocity-weakening” in L29 and “velocity-strengthening” in L127 is not yet introduced.

Response: We accept this suggestion. The paper now states at first use that velocity weakening denotes $b > a$ and velocity strengthening denotes $a > b$; the same definitions are repeated in the constitutive section.

Reviewer comment: The definition of t_c in L79 is not yet introduced in the main text. It is introduced in the abstract, but not in the main text.

Response: We accept this suggestion. The main text now explicitly introduces t_c as the additional constitutive response time in the Introduction before it is used in the research questions or scaling discussion, and Section 2.2 gives its formal constitutive definition.

Reviewer comment: The definition of L_{dyn} and its reference should be added for Figures 6 and 7.

Response: We accept this suggestion. In the retained finite-window diagnostic figure, now Supplementary Figure S17, L_{dyn} is explicitly defined as the width over which the finite-window maximum exceeds $V = 10^{-2}$ m/s and $L_{nuc} = 2$ km as the imposed perturbation diameter. The caption also states that these are descriptive threshold quantities rather than intrinsic stability lengths.

Reviewer comment: Figure 1 is not properly rendered. The colorbar is blocking the main figure. Information about contour colors should be added to the figure caption, not in the master configuration box embedded in the figure.

Response: We accept this suggestion. The main-text figure set has been reorganized, and the former space-time Figure 1 is retained only as Supplementary Figure S11 for continuity. Its layout has been cleaned so the colorbar no longer interferes with the panels, the embedded Master-configuration box has been removed, and the caption now states explicitly that cyan contours denote 10^{-3} m/s and green contours denote 10^{-2} m/s. The common configuration is referenced through Table S1.

Reviewer comment: In all figures, please remove the master set configuration. This makes the figures look busy and is unnecessary since the model setup does not vary across the study. Referring to Table S1 in the caption should be enough.

Response: We accept this suggestion. The repeated Master-configuration boxes have been removed from the retained finite-window figures. Figure captions now refer to Table S1 for the common setup.

Reviewer comment: L112-113 is hard to understand. Adding relevant equations and/or illustrations would help.

Response: We accept this suggestion. The mechanical section now gives the explicit cell mass $m_i = \rho H \Delta x$ and neighbour stiffness $K_s = \mu H / \Delta x$ and derives the scalar momentum equation. The text separately identifies along-layer elastic redistribution, explicit inertia, and the local radiation-loss closure.

Reviewer comment: Figure 5 would be more informative if it focused only on the nucleation zone ($x = \pm 0.5$ km), with associated colorbar limits. The current color range from yellow to bright green makes it hard to see the velocity pattern within the nucleation zone. The caption also needs to explain what the red contour indicates.

Response: We accept this suggestion. The former Figure 5 is now Supplementary Figure S15 and has been replotted over $x = \pm 0.5$ km with a restricted color scale appropriate to the accelerated nucleation zone. The contour line is explicitly identified as $V_{\max} = 10^{-2}$ m/s, and the common configuration is referenced to Table S1.

Reviewer comment: Figure 7 needs a clearer caption explaining why $L_{\text{dyn}}/L_{\text{nuc}}$ is shown as a shaded area rather than a curve.

Response: We accept this suggestion. The former Figure 7 is now Supplementary Figure S17. Its caption defines $L_{\text{dyn}}/L_{\text{nuc}}$ and explains that the filled region is only a visual fill from zero to the ratio, not an uncertainty band or a separate data quantity.

Reviewer comment: L423-L443 does not belong in the Conclusions section. It would be more suitable for the Discussion section.

Response: We accept this suggestion. The forward-looking material has been moved from Conclusions to the reorganized Discussion, where it is presented under testability and remaining limitations. The Conclusions are now restricted to the results established in the present study.