Authors' Responses to Reviewer's Comments

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Title: Hybrid implicit–explicit XFEM simulation of injection-induced seismicity: resolving multi-scale rupture nucleation and dynamics

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We thank the reviewers for their constructive and insightful comments and have revised the manuscript accordingly. We have marked changes made in the revised manuscript in **blue-colored** text, and describe the steps taken to address the reviewers' comments point by point below.
We hope that the revisions have improved the manuscript's contribution to the field and meet the reviewers' expectations. We are grateful for the opportunity to revise our work and look forward to the continued consideration of our manuscript for publication in GMD.

Response to Reviewer #1

Reviewer#1: The authors propose an implicit-explicit (IMEX) time-integration scheme to resolve different timescales in a fully coupled hydromechanical finite element model. The results look promising, especially because of the potential speedup of calculations. However, many details and definitions are missing, therefore improvements are needed before publication. Overall, I thus recommend major modifications of the manuscript before resubmission.

We would like to thank Reviewer#1 for his/her detailed comments, especially for pointing out places where we lack clarity in our first submission. We have carefully addressed all points raised by the reviewer as detailed in what follows

Comment #1: Line 13: Not all implicit time integration schemes are A-stable, hence your statement about all implicit schemes being unconditionally stable is not correct.

Response: We agree with the reviewer. The statement has been revised to avoid implying that all implicit time-integration schemes are unconditionally stable (Lines: 14-16).

Comment #2: Line 14: Shouldn't it depend in the maximal time step? Later you define a lower bound for the time step, but that is not clear when reading this first part, please reformulate.

Response: The text has been reformulated to clarify that the numerical resolution during dynamic rupture propagation is controlled by the prescribed minimum time-step size, i.e., the smallest time step allowed during adaptive time-step reduction (Lines: 14-16).

Comment #3: Line 46: Should be "well-suited".

Response: Corrected to “well-suited” (Line: 50).

Comment #4: Equation (3): What does it mean that $\mathbf{u}, p \in \Gamma_d$? I thought these are functions of $\mathbf{x}$ and t.

Response: We agree that the previous notation was imprecise. The equation has been revised to clarify that the pressure and displacement are evaluated on the fault interface using “$on\ \Omega'$” (Line: 93).

Comment #5: Equation (4): Please define $\boldsymbol{\sigma}$ and $\boldsymbol{t}$. Furthermore, it would generally improve the readability if you could use different names for the test functions, somthing like $\boldsymbol{v}$ and q instead of $\delta\boldsymbol{u}$ and δp.

Response: The definitions of $\boldsymbol{\sigma}$ and $\boldsymbol{t}$ have been added. In addition, the notation for the test functions has been revised to improve readability (Lines: 93-100).

Comment #6: Line 105: Give a precise mathematical definition of $\bar{\boldsymbol{t}}$.

Response: In the weak form, $\bar{\boldsymbol{t}}$ has been replaced with its precise mathematical definition (Line:107-108).

Comment #7: Line 106: Give a precise mathematical definition of Γ_t.

Response: A precise definition of Γ_t has been added (Line: 107-108).

Comment #8: Equation (5): Please define $\boldsymbol{q}$ and Γ_w and derive it in more detail.

Response: The Darcy flux vector $\boldsymbol{q}$ and the hydraulic Neumann boundary Γ_w have been defined, and the derivation of $\boldsymbol{q}$ has been added (Lines: 111).

Comment #9: Lines 111-112: It sounds better to me to multiply Eq. (3) by the test function than vice versa.

Response: The sentence has been revised to state that Eq. (3) is multiplied by the test function (Lines: 113-114).

Comment #10: Line 112: What does it mean for a domain to be discontinuous? Probably you mean that a function on this domain is discontinuous?

Response: We agree that the phrase "discontinuous domain" was imprecise. The discontinuity refers to the field representation across the fault, particularly the displacement jump, rather than to the domain itself. We have therefore revised the text by removing the term "discontinuous" (Lines:114)

Comment #11: Equation (6): Please derive it in more detail.

Response: Equation (6), which is weak form of Equation (3), represents the governing equation for fluid flow within the fracture. Its derivation follows the same procedure as for fluid flow in porous media, namely by substituting Darcy's law into the mass conservation equation. The main difference is that the fracture permeability is evaluated using the cubic law. In addition, a zero-flux condition is imposed at the fracture boundaries (Lines: 88-90).

Comment #12: Figure 1: Please align the angles/orientations of all letters/symbols in the figure.

Response: Done

Comment #13: Line 119: Please change "cap omega to the prime" to a formula.

Response: Done (Line: 123)

Comment #14: Equation (7): Please define $\langle\cdot\rangle$ and $[\cdot]$. Also, why is there a w^3 while in (6) there only was w^2 ?

Response: The operators $\langle\cdot\rangle$ and $[\cdot]$ have now been defined. The appearance of w^3 in Equation (7) results from integrating the fracture-flow equation over the fracture cross-section. Since the cubic law gives the fracture permeability as proportional to w^2, multiplication by the fracture aperture w during the cross-sectional integration leads to a transmissivity term proportional to w^3 (Lines: 127-129).

Comment #15: Line 135: Give a reference or explanation for the formula in (10).

Response: There was an error in Equation (10), which has now been corrected. The surrounding text has also been expanded to provide a clearer explanation of the equation's derivation (Equation 10 in the revised manuscript)

Comment #16: Equation (12): What does the notation with the mean?

$$r = \cdots \begin{cases} = 0 \text{ slip condition} \\ < 0 \text{ stick condition} \end{cases}$$

If it should just say that if r=0 it's a slip condition and if r<0 it's a stick condition, I would write it like that in the text.

Response: The notation has been revised to state the stick and slip conditions more explicitly (Lines: 153).

Comment #17: Line 152: "the above relation", cite an equation number (probably (12) or (13)) here.

Response: Done (Line: 159)

Comment #18: Line 155: "Coulomb's friction law", cite equation (12), where you defined it, here.

Response: Done (Line: 161)

Comment #19: Line 159: It should be Eq. (15) I think, please also check all preceding equation numbers.

Response: Done

Comment #20: Equation (16): Please explain in more detail how to get from (15) to (16).

Response: Equation (16) is obtained by differentiating the yield function F_c, given in Equation (15), with respect to the plastic slip variable (ζ). We have expanded the text to clarify this derivation and the role of this derivative in the linearization procedure (Lines: 163-165).

Comment #21: Equation (18): Give a reference or explanation for this law and why you chose it.

Response: We used the aging law rather than the slip law because it better represents time-dependent fault healing during locked or very slow slip conditions. This is important for injection-induced seismicity, where pore-pressure diffusion may gradually reduce the effective normal stress before fault reactivation occurs. The aging law is commonly used in the Dieterich–Ruina rate-and-state friction framework to describe contact aging and frictional healing during quasi-stationary periods (Dieterich, 1978; Marone, 1998; Rice & Ruina, 1983) (Lines: 171-172).

Comment #22: Equation (19): This only holds if V is scalar, since you are considering 2 -dimensional problems later it should also be correct for vector-valued V 's.

Response: In our simulations, porous media is 2D domain and fault is lower dimensional 1D domain. So, the velocity on fault coordinate system is scalar. Apart from this, the magnitude of slip velocity should be used in state variable formula even in 2D velocity fields.

Comment #23: Equation (20): I think the formula for V is missing, please add it or cite its equation number in case I overlooked it.

Response: The formula for V has been defined along with Equation (20)

Comment #24: Equation (21): Should be N_{ui} instead of N_{uI} in the first sum. Also the meaning of $\mathbf{x}_J$ is not clear to me. And why do you use " $\equiv$ " instead of " $=$ " in the end?

Response: Suggested amendments have been made for equation (21).

Comment #25: Line 188: Should be "where $\mathcal{N}$ denotes the number of all nodal points".

Response: Done (Line: 187)

Comment #26: Lines 188-189: Explain formally what "enrichment by the Heaviside function" means, potentially this could also go into the appendix (depending on how complicated the definition is).

Response: The meaning of enrichment by Heaviside function has been integrated in the revised manuscript following Equation (21) (Lines:190-198)

Comment #27: Line 196: Move the definition of φ before or directly after Eq. (21).

Response: Done (Line: 189)

Comment #28: Line 199: What do you mean by "strong discontinuities", is it referring to the jump size?

Response: Thank you for this comment. By “strong discontinuity,” we do not refer to the magnitude of the displacement jump. Rather, this term refers to the nature of the discontinuity: the primary field itself, here the displacement field, is discontinuous across the crack surface. This is different from a weak discontinuity, where the displacement field remains continuous but its gradient, such as strain or stress, may be discontinuous. We have revised the text to clarify this point and avoid possible confusion (Lines: 196-198).

Comment #29: Equation (24): The last summand appearing in line 201 here should be $[\,[\mathbf{N}_u^{\text{tip}}(\mathbf{x})\overline{\mathbf{b}}(t)]\,]$ I think.

Response: Done.

Comment #30: Equation (26): Same as in 29, also is r the same quantity as defined in Eq. (12)? If yes, please cite that equation.

Response: No, these are not the same quantities. In equations (23) and (26), (r) denotes the radial distance from the crack tip, whereas in Eq. (12), (r) represents the yield function used to determine the stick–slip condition. To avoid ambiguity, we have revised the notation and clarified the definition of (r) where it appears (Line: 201).

Comment #31: Equation (29): Please define $\mathbf{N}_p^{\text{mod}}(\mathbf{x})\overline{\mathbf{d}}(t)$. Furthermore, you again use " $\equiv$ " here which I do not fully understand, see 24.

Response: Done

Comment #32: Line 226: What is ϕ_i ? Do you mean φ_i ? In that case, please decide for one symbol.

Response: It was the sign distance function same as defined in precious equations. The relevant text has been corrected in the revised manuscript.

Comment #33: Equation (31): Currently only the terms inside the first sum are time-dependent, please add all missing t 's.

Response: Equation (31) has been corrected by adding the missing time dependence to all relevant terms. We have also written the gradient of the modified level-set enrichment explicitly in a separate equation for clarity.

Comment #34: Equation (33): Please define $\mathbb{D}$.

Response: Equation (33), which is now Eq. (34) in the revised text, has been corrected by replacing $\mathbb{C}$ with $\mathbb{D}$, as defined earlier in the manuscript (Line: 283)

Comment #35: Line 249: Mathematically it's the Dirac delta distribution (not function) since it is not a function.

Response: We thank the reviewer for the clarification. We have corrected the definition in the revised manuscript. In fact, $\mathbf{m} = [1 \quad 1 \quad 0]^T$ is not a Dirac delta function or distribution; it is the Voigt representation of the second-order identity tensor in two dimensions (Line: 235).

Comment #36: Lines 273-274: Please move the paragraph about the stability parameters (lines 279-281) here so that it immediately explains the statements given here.

Response: Done (Lines: 258-259)

Comment #37: Lines 290-300: Why not considering time step n instead of $n + 1$? That would save space and make the equations more readable.

Response: In the manuscript, the time level (n) denotes the previously converged time step, while (n+1) denotes the current unknown time step being solved. We retained this notation to clearly distinguish known quantities from the unknown variables in the incremental formulation. Although re-indexing the equations using (n) could shorten the notation, the (n+1) convention is widely used in time-discretized formulations and helps avoid ambiguity in the solution procedure. Therefore, we have kept the original notation for consistency and clarity.

Comment #38: Lines 298-299: What does a "more symmetric structure" mean? Give a precise definition or stick to symmetric or non-symmetric cases.

Response: We agree that the phrase “more symmetric structure” was imprecise. We have revised the text to clarify that the row scaling produces an algebraically equivalent system but does not make the Jacobian symmetric. The revised manuscript now explicitly states that the scaled

Jacobian remains generally non-symmetric because the off-diagonal coupling blocks are not necessarily transposes of each other (Lines: 271-273)

Comment #39: Line 304: Please define E formally and explain what an algorithmic tangent operator generally is and what it means for it to be consistent.

Response: We have revised the text to formally define $\boldsymbol{E}$ as the derivative of the algorithmically updated interface traction vector with respect to the displacement jump across the interface. We have also added an explanation that an algorithmic tangent operator is the tangent associated with the time-discrete constitutive update, rather than a purely continuum tangent. The text now clarifies that the tangent is consistent when it is obtained by differentiating the same discrete traction update used to compute the residual vector (Lines: 276-284).

Comment #40: Equation (48): What happened with the sgn terms in (47)? Is K_T different from k_T ? And please give a more detailed derivation of this equation, since it is not obvious to me how to derive it from (20) and (47).

Response: We thank the reviewer for pointing this out. In Eq. (48), the $\mathrm{sgn}(P_T)$ terms were not clearly treated. One $\mathrm{sgn}(P_T)$ term cancels algebraically when Eq. (50) is substituted into Eq. (49) (in the revised text), but the remaining sign dependence must be retained in the numerator. We have revised the derivation to show this explicitly. We have also clarified the notation: K_T and k_T were intended to denote the same tangential stiffness parameter, and the notation has now been unified to k_T throughout the revised manuscript (Equation 50 and 51 in the revised manuscript).

Comment #41: Line 334: Please comment on which adaptive time stepping scheme you are using here.

Response: Done (Line: 306).

Comment #42: Line 344: Please briefly comment on what happens if you increase the value of ξ. I assume $\xi < 1$ should still work, but it would be interesting to see up to which value the algorithm works well.

Response: We agree that ξ controls an important accuracy–cost trade-off. Increasing ξ permits larger slip increments per time step and therefore reduces the number of time steps, but it may under-resolve nucleation and rapid slip if too large. Although values below unity may still allow the implicit algorithm to converge, the upper acceptable value is problem-dependent and should be interpreted mainly as an accuracy limit. Based on Lapusta & Liu (2009), values $\xi \leq 1/3$ give nearly indistinguishable results, while $\xi = 0.5$ introduces small differences. We therefore adopted the conservative value $\xi = 0.1$. This clarification has been added to the manuscript (Lines: 313-321).

Comment #43: Line 351: Please give an explanation on why you chose this switching criterium, maybe there is also some physical intuition behind?

Response: In the revised manuscript, we have mentioned that other switching indicators could include rupture speed, slip acceleration, kinetic energy, stress-drop rate, or inertial-force contribution. Here, slip velocity is used because it is directly available from the rate-and-state model and clearly indicates the transition to dynamic fault motion. Cumulative slip is less suitable, as significant aseismic slip may occur before dynamic rupture. Thus, V_{th} provides a simple, physically motivated, and practical switching criterion. (Lines: 332-339)

Comment #44: Line 354: Please give a reference for the efficiency and robustness of the method.

Response: It is true that central differencing is widely used in transient dynamic analyses and implemented in many finite element programs (Koutromanos, 2018). However, its stability and robustness remain conditional and require a sufficiently small time step. So, in the revised manuscript we have corrected the wording to be more precise and scientifically correct (Lines: 341-344).

Comment #45: Equation (59): Please write " $\min_{e}$ ... ".

Response: Done

Comment #46: Lines 346-375: Generally, it would be helpful if you could mention whether your implicit and explicit schemes are using the same step size or not.

Response: The implicit scheme uses adaptive time stepping according to Eq. (55), whereas the explicit scheme employs a fixed small time step to satisfy the CFL stability condition. Therefore, the two schemes do not generally use the same time-step size. The relevant revision has been made in the revised paper (Lines: 359-361)

Comment #47: Line 393: What is "the cubic law"?

Response: The cubic law has already been introduced in the manuscript in the context of fluid flow within a fault. It describes laminar flow between two parallel plates and relates the hydraulic transmissivity of the fault to the cube of its hydraulic aperture. We have clarified this point in the revised manuscript in section 2.6 (Lines: 379-380).

Comment #48: Lines 416-417: Since you mention it here, it would also be nice to show 2 d plots of your simulations later, not only evaluations at four points.

Response: We agree that 2D spatial plots are useful for illustrating the simulated response. Later in the manuscript, we present 2D contour plots showing the temporal evolution of slip, slip rate, shear stress, and friction coefficient during dynamic rupture (Fig. 6). In addition, we have included 2D contour plots of acceleration, velocity, and displacement obtained from the fully implicit and hybrid approaches in Appendix C.

Comment #49: Figure 4: In the caption some text is not bold, please fix that.

Response: Done.

Comment #50: Lines 433-434: The statement on implicit schemes is not true, see 1.

Response: We agree that not all implicit time-integration schemes are unconditionally stable. We have therefore revised the statement in the manuscript to avoid this over-generalization and to more accurately describe the stability properties of the implicit scheme used in this study (Lines: 453-457).

Comment #51: Line 450: The typesetting after this line is weird (half empty page), this should be fixed before a potential publication.

Done.

Comment #52: Figure 5: When you say that you shift the time axis, does it mean that all "initial conditions" (i.e. at the shifted time $t = 0$) are equal or do they differ due to different evolution before? Please clarify that.

Response: The time shift is applied only for visualization and comparison of the dynamic rupture phase. All simulations start from the same physical initial and boundary conditions at the beginning of injection. However, at the shifted time ($t = 0$), defined as the instant when the maximum slip velocity first exceeds 0.1 m/s, the internal fields are not necessarily identical among the three cases. This is because each simulation undergoes its own pre-dynamic evolution before reaching the threshold, and small differences in temporal resolution can lead to differences in slip, stress, pressure, and frictional state at rupture onset. We have clarified this point in the revised caption (Lines: 492-496).

Comment #53: Figure 7: Please clarify if the time step sizes for explicit and implicit schemes are equal here or if you used adaptive implicit time steps as mentioned in line 334.

Response: The time-step sizes used in the implicit and explicit phases are not generally equal. In the fully implicit approach, the time-step size is adaptive throughout the entire simulation, with the prescribed minimum time step acting only as the lower bound of the adaptive time-stepping algorithm. In the hybrid approach, the implicit phase uses the same adaptive time-stepping strategy as the fully implicit simulation, whereas the explicit phase is advanced using a fixed small time-step size to satisfy CFL condition. We have clarified this point in the revised manuscript (Lines: 516-520).

Comment #54: Line 490: Please change "slightly higher" to an objective value, in this case "around $25 - 30\%$ higher".

Response: Done (Line: 538)

Comment #55: Lines 550-551: Still not true, see 1 and 50.

Response: We have corrected the statement in the conclusion section (Lines: 634-635).

Comment #56: Line 589: It sounds odd to me that the assembly of the Jacobian is affected by longer time steps, please give additional explanations here or relate it instead to solving times.

Response: We agree that the wording was imprecise. The assembly of the Jacobian itself is not directly affected by the time-step size. The relevant issue is whether the consistent tangent operator

of the frictional interface law is included in the Jacobian. If this tangent is included, a full consistent Jacobian is obtained; however, its derivation can be complicated, particularly for nonlinear rate-and-state friction laws. In addition, for some friction formulations, deriving an analytical consistent tangent may not be straightforward or feasible.

When the consistent slip tangent is omitted, the Jacobian becomes an approximate one. In that case, the nonlinear convergence during dynamic rupture becomes more sensitive to the time-step size. Reducing the time-step size limits the nonlinear change in slip, slip velocity, stress, and state variable within each increment, thereby reducing the number of Newton iterations required for convergence. Therefore, the effect is related to the nonlinear solution procedure and total solving time, rather than to the Jacobian assembly itself. We have revised the text accordingly (Lines: 680-684, 695-697).

Comment #57: Lines 597-598: I don't see any columns in Figure A1, please fix this.

Response: Done

Comment #58: Figure A3: If I understand correctly, you compare both implicit and IMEX schemes without using the slip tangent operator. Since this operator improves the speed of the fully implicit method, I think it would be more fair to compare implicit+operator with IMEX (with or without). It would be nice if you could add such tests to give a better understanding of the potential speedup one can expect.

Response: Following the reviewer's suggestion, we have now included a comparison between the fully implicit scheme with the slip tangent operator and the IMEX scheme, both with and without the slip tangent operator. This comparison provides a clearer assessment of the speedup achieved by the IMEX approach under a fairer reference configuration. It also shows how neglecting the contribution of the slip tangent operator within the IMEX formulation affects the overall CPU time and computational efficiency (Lines: 718-729).

Comment #59: Generally, the "." symbols you use to indicate multiplications are looking more like normal dots at the end of a sentence since they are very low, maybe it is possible to fix that.

Response: We have revised the notation and replaced the low-positioned dot symbols used for multiplication with properly centered multiplication dots to avoid confusion with sentence punctuation.

Response to Reviewer #2

Reviewer#2: The manuscript contains a potentially useful computational idea, but in its present form it substantially overstates the novelty, physical completeness, numerical performance, and practical applicability of the proposed method. I do not think that the current results are sufficient to support the main claims of the paper.

The formulation requires significant clarification, especially regarding the meaning of the proposed "IMEX" scheme, the governing poroelastic equations, the treatment of the fault, and the switch from slow loading to dynamic rupture. The numerical evidence is also not strong enough. In particular, the manuscript lacks independent validation of the new algorithm, systematic spatial and temporal convergence analysis, convincing demonstration of elastodynamic wave propagation, and performance tests at resolutions that would justify claims about scalability or operational applicability.

I therefore recommend major revision. However, I consider the required revision to be substantial rather than incremental. To become acceptable, the manuscript would need a much clearer and more defensible formulation, corrected or justified terminology, independent benchmark validation, convergence studies, larger and more realistic performance tests, and a more honest discussion of the limitations of the model. At present, the claims are too broad and too strong relative to what is actually demonstrated. My main concerns are listed below.

We would like to thank Reviewer#2 for his/her critical reading of our manuscript and for his/her suggestions to improve the scientific merit of our study. We have addressed all criticism as detailed below in the revised version of the manuscript.

Comment #1: The quasi-static to dynamic switching idea is not new by itself. The manuscript presents the transition from a quasi-static or implicit loading stage to a dynamic stage as a central novelty. I do not think this claim is sufficiently justified. The general idea of switching from slow quasi-static deformation to fully dynamic evolution once instability develops has already been used in related earthquake-cycle and rupture simulations. For example, Pampillón et al., "Dynamic and Quasi-Dynamic Modeling of Injection-Induced Earthquakes in Poroelastic Media", already modeled injection-induced earthquake nucleation and dynamic rupture in poroelastic media, and Alkhimenkov et al. (2025), "Stress drop sequences in the simplest pressure-sensitive ideal elasto-plastic media: implications for earthquake cycles", used a transition from slowly accumulated deformation to dynamic rupture evolution in an elasto-plastic setting.

Response: We thank the Reviewer#2 for raising this important comment, which highlights some lack of clarity in our first submission. Indeed, our intention was not to discuss the switching concept as the main novelty of the manuscript. We have therefore revised the text to clarify this point and avoid overstating the originality of the quasi-static-to-dynamic transition. In the revised version of our manuscript we now make it clear that the main contribution is the development of

a fully coupled poro-viscoelastodynamic numerical framework for injection-induced seismicity, which is further implemented as an open-source numerical platform thereby freely available for the interested community. To the best of our knowledge, fully coupled poro-elastodynamic formulations incorporating rate-and-state-dependent friction remain limited in the literature, and several related studies have relied on commercial software platforms, particularly COMSOL Multiphysics (Pampillon et al., 2018; Katsman & Ben-Avraham, 2025).

An additional aspect of novelty stems from the proposed **hybrid time-integration strategy** for simulating rupture dynamics in poroelastic media, which offers as demonstrated in this study reduced computational cost. This is particularly relevant for injection-induced seismicity, a multiphyiscs problem involving notably different time scales: from slow pore-pressure diffusion and quasi-static stress evolution during injection, to rapid dynamic rupture once instability develops. The proposed approach is therefore designed to efficiently resolve both regimes while preserving the essential coupled hydro-mechanical and dynamic processes. We have revised the manuscript accordingly to better distinguish between the established idea of switching between deformation regimes and the specific contributions of the present work (Lines: 57-60).

Comment #2: The use of the term IMEX is not sufficiently justified. The manuscript presents the method as a hybrid implicit-explicit (IMEX) scheme. However, what is described appears to be closer to a solver-switching strategy, in which the simulation is advanced implicitly during the slow loading stage and then switched to an explicit mechanical update once a prescribed slip-velocity threshold is exceeded. This is not necessarily the same as a classical IMEX time discretization in the numerical-analysis sense, where implicit and explicit operators are usually combined within a single time-integration framework.

This distinction is important because the manuscript makes methodological claims based on the IMEX terminology. The authors should clarify whether the proposed method is a genuine IMEX discretization or a hybrid implicit-to-explicit switching algorithm. Moreover, the hydraulic equation appears to remain implicitly advanced during the nominally explicit phase. Therefore, the method does not seem to be fully explicit for the coupled hydromechanical problem. This should be stated clearly and reflected in the terminology.

Response: As mentioned by Reviewer, the term IMEX may be misleading if interpreted in the classical numerical-analysis sense, where implicit and explicit operators are combined within the same time-discretization framework. The method proposed in this study is not intended to represent a classical IMEX discretization strategy in that strict sense. Rather, it is a hybrid implicit-to-explicit solver-switching strategy. In the revised manuscript, we have therefore modified the terminology throughout the text. The method is now referred to as a hybrid implicit–explicit solver-switching strategy instead of an IMEX scheme.

We also agree that the method is not fully explicit for the entire coupled hydromechanical problem during the dynamic phase. In the proposed approach, the hydraulic equation remains implicitly advanced, while the explicit formulation is applied to the mechanical dynamic update. This point has now been stated explicitly in the revised manuscript to avoid any ambiguity (Lines: 329-332).

Comment #3: The switching criterion is empirical, not convincingly physics-based. The switch from the implicit to the explicit phase is controlled by a prescribed slip-velocity threshold. I do not find this criterion sufficiently justified as a physics-based condition. A slip-velocity threshold can be a useful numerical trigger, but in the manuscript, it is not derived from nucleation theory, process-zone size, characteristic slip distance, stress drop, wave speed, or any nondimensional physical argument. The authors should either provide a clear physical derivation or interpretation of the threshold, or describe it more modestly as an empirical numerical switching parameter. Calling this switch "physics-based" is too strong in the present form.

Response: We agree that the prescribed slip-velocity threshold should be described more modestly, since it is not directly derived from the underlying physics. Therefore, in the revised manuscript, we have replaced the term "physics-based switching parameter" with just "switching parameter" (Lines: 62-63)

We have also revised the description of the hybrid solution approach to better explain why slip velocity was selected as the switching indicator. We further clarify in the manuscript that other empirical indicators could also be considered, such as rupture-front speed, slip-velocity acceleration, kinetic energy, stress-drop rate, or changes in inertial-force contribution (Lines: 333-339).

In addition, in the revised manuscript, we have noted in the limitations and future work section that a physics-based, non-dimensional switching criterion should be developed. Such a criterion should not only optimize computational efficiency but also remain applicable across different geological conditions (Lines: 671-673).

Comment #4: Implicit schemes with large time steps are not only problematic for dynamic wave propagation. The manuscript discusses the limitation of implicit schemes mainly in relation to dynamic rupture and wave propagation. However, the need for small time steps is broader than that. During fluid injection, even before fully dynamic rupture, the system may develop rapid localization, accelerating slip, or new shear bands. These processes require adequate temporal resolution even if the governing problem is treated quasi-statically or implicitly.

Therefore, the issue is not only that implicit schemes become inefficient for elastodynamic wave propagation. Large implicit time steps may also miss or delay important transient processes during nucleation and localization. The manuscript should discuss this limitation more carefully.

Response: We agree that limitation of large implicit time steps is not restricted to dynamic wave propagation, but may also affect the temporal resolution of nucleation, accelerating slip, localization, and other transient processes prior to fully dynamic rupture.

In our numerical framework, the fully implicit scheme employs the adaptive time-stepping strategy proposed by Lapusta & Liu (2009), which ensures that slip at each time step remains smaller than a prescribed fraction of the characteristic slip distance (Lapusta et al., 2000). This strategy is designed to maintain adequate temporal resolution during the pre-nucleation and localization stages and to reduce the risk of missing or delaying important transient processes. Nevertheless, once fully dynamic rupture develops, it becomes necessary to prescribe a minimum allowable time-step size for the adaptive time-stepping scheme. Without such a lower bound, the algorithm may reduce the time step to excessively small values, resulting in prohibitive computational cost (Lines: 308-321).

Therefore, the main issue addressed in Section 5.1 concerns the dynamic rupture phase and the associated wave-propagation problem. In this section, we assess how small the minimum implicit time step must be to adequately resolve fully dynamic rupture. Our results show that, to achieve sufficient accuracy during the dynamic phase, the minimum implicit time step may need to approach the CFL-restricted explicit time step, which substantially increases the computational cost.

We have revised the manuscript to clarify that large implicit time steps may also affect the resolution of nucleation and localization prior to dynamic rupture, and that adaptive implicit time stepping can mitigate this limitation, although at increased computational cost when very small time steps are required (Lines: 471-475).

Comment #5: The governing equations do not appear to represent full dynamic Biot Poroelasticity. The manuscript refers to the formulation as dynamic poroelastic or elastodynamic Biot-type modeling. However, the governing equations presented appear to be a simplified u-p formulation rather than the full dynamic Biot system. In particular, I do not see the standard two-field inertial structure with the full 2 by 2 dynamic mass matrix, added-mass coefficients, tortuosity, and relative fluid acceleration terms.

The equations include an inertial correction in the Darcy-type pressure equation, but this is not the same as the full dynamic Biot poroelastic formulation. If the authors intentionally use a simplified formulation, they should state the approximation regime explicitly and justify it. Otherwise, the formulation should not be described as general dynamic Biot Poroelasticity.

Response: The governing equations used in the manuscript do not represent the full dynamic Biot poroelastic formulation. In particular, the formulation does not include the complete two-field

inertial structure, the full dynamic mass matrix, added-mass terms, tortuosity effects, or relative fluid acceleration terms.

We have therefore revised the manuscript to avoid describing the model as a general or full dynamic Biot poroelastic formulation. Instead, we now describe the simulations as fully coupled poroelastodynamic simulations throughout the revised manuscript, while explicitly clarifying the approximation involved. Specifically, the pressure equation is formulated as a Darcy-type diffusion form, whereas dynamic effects are included through the solid momentum balance. This distinction has been clarified in the revised manuscript (Lines 93-100).

Comment #6: The model is restricted to reactivation of a prescribed fault. The manuscript is framed broadly in terms of injection-induced seismicity. However, the numerical model appears to address only reactivation of a pre-existing prescribed fault. It does not include bulk plasticity, damage, or a fracture-propagation criterion capable of generating new fractures or new shear bands in the surrounding medium.

This is an important limitation. In injection-induced seismicity, existing faults can indeed be reactivated, but new fractures and off-fault deformation can also develop depending on the stress state, injection conditions, and material properties. Since the present model cannot capture these processes, the authors should clearly restrict their claims to prescribed-fault reactivation. Broader statements about general injection-induced seismicity and physics-based hazard assessment should be moderated.

Response: We agree that the proposed framework is designed to model the reactivation and dynamic instability of prescribed pre-existing discontinuities, rather than the nucleation of new fractures, hydraulic-fracture propagation, or off-fault plastic/damage localization. These processes require different constitutive assumptions, failure criteria, and numerical treatments, and are outside the intended scope of the present work. The focus of this study is therefore on injection-induced slip and seismicity associated with known faults or fractures, which is a central problem in many induced-seismicity applications.

We have also revised the manuscript to moderate broader statements related to general injection-induced seismicity, physics-based hazard assessment, and operational forecasting. These points are now discussed more explicitly in the “Limitations and future work” section (Lines 669-675).

Comment #7: Rate-and-state friction is phenomenological and should not be oversold as physics-based. The manuscript uses a rate-and-state friction law. This is a valid and widely used constitutive model, but it is phenomenological. It is based on fitted parameters and an assumed state evolution law. Therefore, the use of rate-and-state friction alone does not make the model fully physics-based.

The authors should be more precise in their language. They should also provide all required details of the frictional initialization, including the reference velocity, reference state, initial state variable, initial slip velocity, and any regularization used at very small slip rates. These details are essential for reproducibility and for interpreting the switch based on slip velocity.

Response: We agree that rate-and-state friction is a phenomenological constitutive framework, with parameters that must be calibrated and with assumed state-evolution laws. Our intention was not to imply that the use of rate-and-state friction alone makes the model fully physics-based. Rather, the framework combines balance laws for coupled hydromechanical deformation with well-established constitutive laws, including the friction law for fault weakening and healing. We have therefore revised the manuscript to avoid overstating the formulation as a fully physics-based model and to describe the rate-and-state law more precisely as an empirically motivated and widely used frictional constitutive model (Lines: 166-167).

In addition, we have added the required details of the frictional initialization, including the reference slip velocity, reference state variable, initial state variable, initial slip velocity, and the lower bound imposed on slip velocity to avoid numerical singularities at very small slip rates. These details are now provided in the revised manuscript to improve reproducibility and to clarify the interpretation of the slip-velocity-based switching criterion (Lines 418-420 and Table 1).

Comment #8: The initial stress and fault state need clearer specification. The manuscript provides initial stress values, but the initial mechanical state of the fault is not described in sufficient detail. The authors should specify the initial normal and shear tractions on the fault, the initial Coulomb ratio, the initial state variable, the initial slip velocity, and whether the model is equilibrated before injection begins. This is important because the onset of fault reactivation depends strongly on the initial stress state and on how close the fault is to failure. Without this information, it is difficult to assess whether the reported nucleation behavior is physical or partly a consequence of initialization.

Response: We have revised the manuscript to clarify the initial fault conditions before injection. The model is first brought to mechanical equilibrium under the prescribed initial stresses, and the equilibrated fault state is then used as the initial condition for the injection simulation. We now report the initial shear stress, effective normal stress, Coulomb ratio, and aperture distribution along the fault in Figure 5. We have also specified the initial state variable and initial slip velocity used in the rate-and-state friction formulation (Table 1). These revisions clarify the initial proximity of the fault to failure and improve reproducibility of the results (Lines: 420-423, and Figure 5).

Comment #9: The treatment of the fault as a lower-dimensional interface needs more justification. The fault is represented as a lower-dimensional interface with hydraulic aperture and cubic-law

flow. This is a useful modeling idealization, but it introduces assumptions that must be discussed more carefully.

The authors should clarify the effective fault thickness, how flow perpendicular to the fault plane is treated, how fluid exchange between fault and matrix is modeled, and how aperture-dependent permeability is coupled to the governing equations. If permeability, aperture, or other fault properties evolve during deformation, these evolution laws should be stated explicitly and consistently. Introducing deformation-dependent hydraulic properties also makes the governing system nonlinear and modifies the standard poroelastic equations. The manuscript should reference previous work using similar modifications and should discuss the consistency and limitations of the resulting coupled formulation.

Response: In the revised manuscript, we have clarified the treatment of fluid exchange between the fault and the surrounding porous medium. The weak formulations for flow in the porous matrix (Eq. 5) and along the fault (Eq. 7) are coupled through the exchange flux $\bar{q}_f$, which is also flow normal to fault interface. In the present formulation, Eq. (7) can be rearranged to isolate the exchange-flux contribution $\bar{q}_f$, with all remaining terms placed on the right-hand side. The resulting exchange flux is then directly incorporated into the porous-medium flow equation (Eq. 5) as a source/sink term. Therefore, flow perpendicular to the fault plane is represented through the matrix–fault exchange flux, rather prescribing an independent fault-normal permeability. In contrast, conventional zero-thickness interface-element formulations often require a transversal conductivity or leak-off law to relate the matrix–fault pressure difference to the normal exchange flux (Segura & Carol, 2008; Cerfontaine et al., 2015) (Lines: 130-136).

In addition, Section 2.6 has been revised to state more explicitly the evolution law for the stress-dependent fault aperture and the corresponding fault permeability derived from the cubic law. The revised section also clarifies how the updated aperture and permeability enter the governing equations, thereby making the hydraulic problem nonlinear and directly coupled to mechanical deformation. We have also added a discussion of the assumptions and limitations of this coupled formulation, including the fact that aperture heterogeneity, roughness-induced channeling, gouge migration, damage, chemical effects, or shear-induced permeability enhancement are not resolved (Lines: 366-393).

Comment #10: Fault geometry is unclear. The schematic figure and the computational setup appear to suggest different fault geometries. In one figure the fault/aperture representation looks elliptical or lens-like, whereas in the computational setup the fault appears as a long rectangular or line-like feature. The authors should clearly distinguish between the conceptual aperture schematic and the actual fault geometry used in the simulations. The exact fault geometry, length, orientation, aperture distribution, and representation in the XFEM discretization should be stated clearly.

Response: In the revised manuscript, we now clearly distinguish these two representations. The lens-like or elliptical shape shown in the schematic figure (Fig. 1) was intended only as a conceptual cross-sectional illustration of the fault aperture. It does not represent the actual fault geometry used in the simulations (Lines: 115-120).

In the revised model setup section (section 3), we have explicitly mentioned that the fault is represented as a lower-dimensional internal discontinuity: a one-dimensional line embedded in the two-dimensional porous domain. We have revised the model-description section to clarify this. In addition, the actual fault length, orientation, and initial aperture distribution are now stated explicitly (Lines: 407-411).

Comment #11: Dynamic wave propagation is not sufficiently demonstrated

The manuscript emphasizes dynamic rupture and elastodynamic, but I do not see clear snapshots of wave propagation generated by fault nucleation. If inertia and elastodynamic wave propagation are central to the method, the authors should show the resulting wavefield, for example snapshots of velocity, displacement, stress, or pressure perturbations, or receiver seismograms at several locations. Without such evidence, it is difficult to assess whether the dynamic elastodynamic component of the solution is actually resolved or whether the analysis is mainly based on fault slip variables.

Response: Figure 11 already presents the acceleration, velocity, and displacement time histories recorded at the injection-well location for both the fully implicit and hybrid solution approaches. However, we agree that receiver time histories alone do not sufficiently demonstrate the spatial propagation of the elastodynamic wavefield generated during rupture.

Following the reviewer's suggestion, we have added spatial snapshots of acceleration, velocity, and displacement over the computational domain in the revised manuscript. These snapshots show the development and propagation of dynamic wavefields during fault rupture and demonstrate that the elastodynamic component of the solution is explicitly resolved by both numerical approaches. The corresponding discussion has been added to the revised manuscript (Lines 736-770, Appendix C).

Comment #12: The mesh is too small to support strong performance and scalability claims. The numerical model uses a 200 by 200 mesh, corresponding to only 40,000 elements. This is a modest 2D problem. I do not think such a calculation can support strong claims about computational scalability, operational traffic-light systems, or future field-scale forecasting.

For comparison, existing high-resolution 2D and 3D numerical studies in related coupled mechanical and hydromechanical problems use more than 100 million voxels. A 10000 by 10000 two-dimensional voxel grid already contains 100 million elements, and modern three-dimensional

GPU-based studies can reach hundreds of millions of cells, for example 500^3 = 125 million voxels. Against this background, a 200^2 simulation is far too small to demonstrate scalability or high-performance capability.

The authors may report that their hybrid strategy reduces CPU time relative to their own fully implicit implementation for this specific test. That is useful. However, it should not be presented as evidence of general scalability or readiness for operational hazard systems. To support performance claims, the authors should provide problem sizes, number of degrees of freedom, hardware, linear solver, preconditioner, number of time steps, Newton iterations, assembly time, solve time, and scaling behavior with increasing resolution.

Response: Although the present simulations are performed on a moderate-size two-dimensional mesh, the observed reduction in computational cost suggests that the proposed hybrid strategy has potential for larger-scale applications. This potential arises because the method uses the fully implicit scheme during the slowly evolving pre-nucleation phase, where large time steps are efficient, and switches to an explicit dynamic update during rapid rupture, thereby avoiding repeated nonlinear iterations and Jacobian assembly during the most computationally demanding phase. However, we agree that the present results do not constitute a formal scalability analysis. Therefore, we have revised the manuscript to state that the hybrid solution approach demonstrates computational efficiency for the present benchmark problem and may offer potential advantages for larger-scale simulations, while systematic scaling tests are left for future work (Lines: 656-668).

A systematic scalability analysis over substantially larger two-dimensional and three-dimensional problems is also beyond the scope of the present work and is identified as a subject for future research (Lines: 673-675).

Comment #13: A systematic convergence study is missing. A major weakness of the manuscript is the absence of a systematic spatial and temporal convergence analysis. The authors should run the solver at several spatial and temporal resolutions, for example four or five levels, and demonstrate convergence of key quantities: rupture time, peak slip velocity, final slip, rupture speed, seismic moment, pressure evolution, and wavefield measures.

Without such a study, it is unclear whether differences between the implicit and hybrid simulations are caused by the time-integration strategy, by spatial under-resolution, by the fault/contact discretization, or by insufficient temporal resolution. In dynamic rupture and rate-and-state friction problems, resolving the rupture process zone and nucleation length is essential. A CFL condition alone is not a sufficient accuracy criterion.

Response: In the present study, the fully implicit and hybrid simulations are performed using the same mesh, fault/contact discretization, hydraulic discretization, and material and frictional

parameters. Therefore, the observed differences between the two solutions are not expected to originate primarily from spatial discretization, because any mesh-related error is common to both approaches.

Nevertheless, in response to the reviewer's request, additional simulations were performed using the proposed hybrid approach with five different mesh resolutions (Fig. 1). As shown in the figure, increasing the mesh resolution leads only to a slight increase in the peak slip velocity, even for the finest mesh with 255×255 elements. Further mesh refinement would considerably increase the computational cost. These results suggest that the remaining differences between the fully implicit and hybrid solutions are mainly associated with the time-integration strategy, the switching slip-velocity threshold, and the minimum explicit time-step size.

To clarify this, we have revised the manuscript and added a new sensitivity analysis, shown in Figure 2 below, (Figure 12 in the revised manuscript). This analysis examines the effects of the minimum explicit time step, and the switching slip-velocity threshold, V_{th} , on the slip-rate evolution at point P4 during the seismic event. The results show that reducing the explicit time-step size and lowering the switching threshold improve the agreement between the hybrid and fully implicit solutions, although a small discrepancy remains. Achieving closer agreement would require substantially smaller explicit time steps and/or lower switching thresholds, which would significantly increase the computational cost. The remaining discrepancy is also expected to partly reflect the different numerical characteristics of the central-difference explicit update compared with the fully implicit time-integration scheme. All relevant descriptions have been included in the revised manuscript (Lines: 590-628).

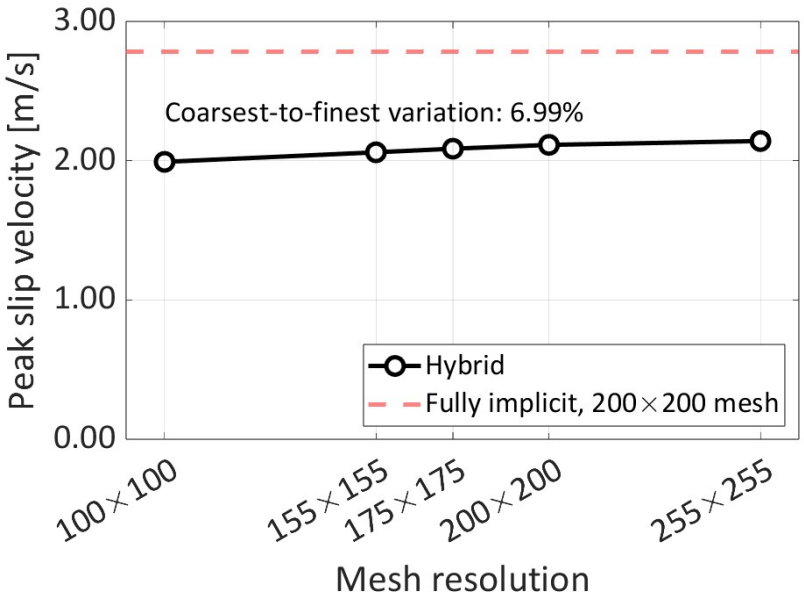


Figure 1. Mesh-sensitivity analysis of the peak slip velocity predicted by the hybrid solution strategy for mesh resolutions ranging from 100×100 to 255×255. The peak slip velocity increases slightly with mesh

refinement, with a coarsest-to-finest variation of 6.99%. The dashed line indicates the corresponding fully implicit solution obtained using the 200×200 mesh.

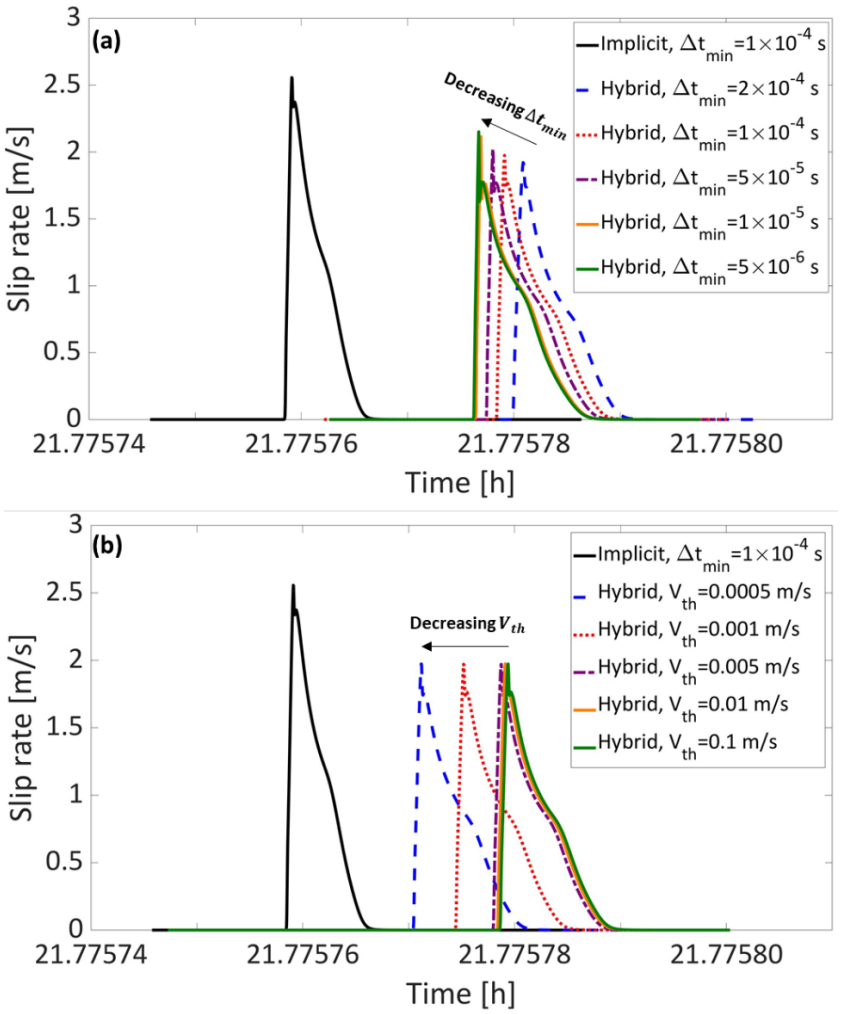


Figure 2. Sensitivity of the slip-rate response at point P4 during the seismic event to the numerical parameters of the hybrid implicit–explicit scheme. (a) Comparison between the fully implicit solution and hybrid solutions obtained using different minimum time-step sizes during the explicit phase for a fixed switching criterion. (b) Comparison between the fully implicit solution and hybrid solutions obtained using different switching slip-velocity thresholds for a fixed minimum explicit time step.

Comment #14: Validation of the new method is insufficient

The manuscript relies heavily on comparison with the authors' own fully implicit solver. This is not sufficient validation of a new hybrid implicit-explicit switching algorithm. The fully implicit solver may be a useful reference, but the new method should be validated independently. At minimum, the authors should include benchmark tests against analytical or semi-analytical solutions where possible. For the poroelastic part, classical tests such as Mandel-type problems or other fluid-injection solutions in poroelastic media would be appropriate. For the dynamic rupture part, comparison with established rupture benchmarks or highly resolved reference solutions is

needed. The present validation does not convincingly establish the correctness of the coupled dynamic solver.

Response: Mandel-type benchmarks are appropriate for validating the poroelastic and quasi-static components of the formulation. In our previous work (Sabah et al. 2026), the quasi-static poroelastic part of the model was verified against Mandel's analytical solution for saturated porous media (Wang, 2000) and also established numerical benchmarks for fractured porous systems (Damirchi et al., 2022). However, the Mandel problem, as well as other analytical or semi-analytical solutions for injection into poroelastic media, neglects inertia. Therefore, such benchmarks cannot directly validate the explicit central-difference mechanical update used in the present work.

For the rupture component, to our knowledge, the available two-dimensional plane-strain SEAS or SCEC benchmarks that incorporate rate-and-state friction are mainly formulated within a quasi-dynamic framework. Nevertheless, to evaluate the explicit elastodynamic component of the proposed hybrid solution approach, we used the TPV205-2D dynamic rupture benchmark from the SCEC/USGS Earthquake Code Verification Exercise (Harris et al., 2009). However, it should be noted that TPV205-2D is based on a slip-weakening friction law rather than a rate-and-state friction law. The corresponding verification results are provided as Supplementary Material.

For the pre-dynamic and nucleation phases, which are simulated using the fully dynamic implicit component of the proposed numerical framework, the rate-and-state frictional response and nucleation behavior were previously verified against the SEAS quasi-dynamic benchmark of Erickson et al. (2020). Although this benchmark does not validate the elastodynamic wave-propagation component, it provides a relevant reference for assessing the rate-and-state friction implementation and the nucleation-stage slip evolution.

We believe that the previously conducted benchmarks for verifying the fully implicit component, together with the newly added verification test for the explicit dynamic component, provide sufficient evidence for the validity of the proposed dynamic solver. For completeness, all verification results, including those related to both the fully implicit and explicit components of the framework, have been provided in the Supplementary Material.

Comment #15: The conclusions overstate what has been demonstrated. The conclusions discuss broader applicability to 3D fault networks, realistic induced-seismicity forecasting, and adaptive traffic-light systems. These statements are not supported by the numerical evidence presented. The manuscript demonstrates a hybrid switching strategy on a small, homogeneous, two-dimensional, prescribed-fault problem. It does not demonstrate scalability to realistic 3D problems, complex fault networks, heterogeneous media, new fracture creation, or operational forecasting. The authors should substantially narrow the conclusions and clearly separate what is demonstrated from what is only a possible future extension.

Response: The conclusions have been revised to better reflect the scope of the numerical evidence presented in this study. The revised manuscript now explicitly states that the proposed hybrid switching strategy has been demonstrated for a two-dimensional, homogeneous, prescribed-fault reactivation problem. Broader statements regarding realistic three-dimensional fault networks, heterogeneous media, new fracture generation, operational forecasting, and adaptive traffic-light systems have been moderated and are now clearly presented as possible future extensions rather than demonstrated capabilities of the current framework (Lines: 654-661).

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