Summary

This paper presents the second version of DynEarthSol, which becomes a 2D and 3D geodynamic modelling framework designed to solve the dynamics of geodynamic, surface process and earthquake cycle processes. Three technical breakthroughs justifying a second version of this model and this paper are (i) bitwise reproducibility, (2) coupling to surface processes, and (3) including state evolution of RSF in the existing rate-dependent formulation. The paper is mostly well-written, although it is often unclear what is exactly physically and mathematically implemented, such that several major points involve questions about that. Hence I think that the guidelines of GMD for reproducibility based on the text are not met. Nonetheless, remarkable technical has been done to get such a complex model with unstructured meshes and remeshing working fast across CPU-GPU architectures. I think this is important technical work that deserves publication in Geoscientific Method Development (GMD). However, two out of three technical challenges do not meet the standards I expect from GMD. Bitwise reproducibility is claimed as one of three novelties, but this is not demonstrated in the paper. RSF is claimed as "validated", but the verification is limited to one local closed-form check of equations that does not verify many key processes of earthquake cycle dynamics. Since your RSF implementation is not fully physically sound in terms of an underlying assumptions on stress being equal to strength not being met and a physical process limiting rupture speeds that seems to be missing, it is important to either fix that and/or have a robust reproduction or closer evaluation of the resulting earthquake loading and release and large scale earthquake pattern. Since such robust technical proof is either absent or too limited, I recommend major revisions. If the editor thinks that some of the main points below on technical correctness are of high importance to GMD, more time might be needed for the technical implementation, such that reject and encouragement for resubmission would better allow for that time.

* Major points *

* Implementation of RSF using plasticity

Adding the state evolution to RSF and doing earthquake cycling is one of three main novelties. However, exactly how this is implemented is not well described and seems not in line with fundamental assumptions in RSF.

RSF is derived under the assumption that fault slip occurs always - even at extremely low and negligible rates down to the -20, -30 or even -40 m/s -, such that stress and strength are always equal (e.g., Nakatani, JGR, 2001; Herrendorfer et al., JGR, 2018; Sirorattanakul et al., Nature, 2025). In your plasticity formulation, stress and strength are per definition not equal, as you use strength as a threshold. In the interseismic period between earthquakes stress is smaller than (interface) strength, while we now know that ample of slip occurs below seismic slip rates as well, such as during slow slip events (hence the seismic coupling at faults is often less than half). Hence this leads to the question if, and how, your threshold plasticity formulation meets the fundamental assumption of RSF stating stress = strength, and how it then captures slip transients during the inter-seismic period. Note that this is the reason that Herrendorfer and related papers implement RSF as a highly non-linear viscous formulation, where slip can always occur - and can be very important interseismically - and stress and strength are always equal. In that paper interface strength defined by Nakatani, JGR, 2001 is used to locally approximate plasticity's threshold at which slip rates become noticeable.

Formulations in the abstract, conclusions and many other locations in the main text you write that "this approach enables the simulation of complex fault behaviors, from stable creep to stick-slip instabilities, " (l.8-9), "allows stable creep, stick-slip instability, and dynamic rupture-like behavior to be represented within the same continuum framework" *(. 39-40) and the "full qualitative spectrum from .. stable creep to self-sustaining earthquake cycling and creep". To me and most readers such texts and the full spectrum implies that also slow slip events are simulated. However, also in light of the above, I wonder wether those are simulated correctly? Also since you never use the words to describe or list them. Could you show one panel in Figures 12-13 with non-decaying (similar magnitude) slow slip events? You could also plot stress and strength throughout a few earthquake cycles or slip transients.

- Also what means dynamic rupture-like? Please reformulate in relation to comments below

You explain and justify your slip rate calculation in 5.3.2. You argue that principal stresses axes remain close to fault parallel and fault normal throughout the cycle. This might be a allowable approximation when the fault is defined as a planar structure, but this does not hold during fault growth. Preuss et al., JGR, 2019, shows the evolution of fault stress and friction during fault growth with an invariant RSF formulation, which are significant and can explain fault orientation mismatches in nature.

- To evaluate your slip rate calculation you could evaluate wether you can reproduce the Arthur angle as well.
- Note that Preuss 2019 uses the invariant formulation of van Dinther 2013 and Herrendorfer 2018 (V_slip = plastic strain rate * 2 * dx , where dx is grid size or fault width), which does not suffer from the same limitation in approximation V_slip as you suggest in l. 578.
- Ideally, check your implementation against the Arthur angle, but at least discuss your limitation more adequately and be aware when removing the definition of fault plane latter on.
- This computation is also computationally efficient and allows for the mesh-objectivity under plasticity, as discussed latter on. Hence one could consider adopting this one.
- Alternatively, this implementation could for a predefined planar fault be evaluated in a large-scale benchmark, but this will be discussed in the next point.

* Noise and reproducibility

One of the main technical points in the abstract, and one of three technical breakthroughs presented, is the bit-wise reproducibility. However, this is not demonstrated in the paper. Since your figures 12 and 13 suggest the presence of numerical noise I doubt that at least the earthquake cycle simulations are truly bitwise reproducible. Please repeat identical simulations across multiple runs and relevant parallel execution configurations and compare the complete numerical state using e.g., binary comparison. If reproducibility is not demonstrated or only partially for repeated runs using one fixed hardware and execution configuration, please narrow the claim accordingly.

- Could you also clarify which operation was previously nondeterministic and how the two-stage update removes this nondeterminism?

Your figures 12 and 13 with earthquake cycle results show considerable noise, whose amplitudes can very from 0.5-3 orders of magnitude. A large part of this may well be numerical in nature.

- A part of this in the left and right “creep” panels may be physical as well, hence please show the left and right panels for much short times/displacements, such that about 10 cycles are seen.
- Other parts in the central panel are likely numerical still, as oscillations are prominent in the inteseismic and at the end of coseismic as well. Such oscillations are not present in e.g., the comparable work of Herrendorfer et al., JGR, 2018, which - like many boundary element earthquake cycle simulations (Rice et al. JGR 1993) - show very clean sequences with fully periodic recurrence of characteristic earthquakes, after the first or first few (spin-up) earthquakes.
- Additionally, the small variability in recurrence rate and slip you note, also suggests that there might be something that is not fully appropriately solved.
- Often reducing the grid size can help to achieve a smooth, noiseless evolution. Does that, or anything else, help you to reduce the noise in your simulations?
- Also, is this noise bitwise reproducible?

Reproducibility of plasticity is also affected by mesh-objectivity, which is only discussed in one sentence in relation to another paper with a different implementation and a focus only at rate-dependent plasticity (Goudarzi et al., 2023). However, mesh-objectivity for your long-term geodynamic simulation coupled to surface processes would not be helped by that rate-dependence, yet the fault locations and intensity and maybe even surface expressions could be affected. In various geodynamic codes this is dealt with by adding Kelvin viscoelasticity to regularise the fault width (Duretz et al., GGG, 2019). ASPECT includes a similar type of regularisation, such that this difference could explain a distinct part of the difference between your and ASEPCT’s simulation, which is distinctly smoother and has less faults.

- It is not clear wether you model is reproducible for different grid sizes. Could you clarify what spatial or temporal regularization controls shear-zone width, and demonstrate mesh convergence or discuss the limitations for your surface processes and earthquake-cycle simulations?
- Since you write here and there, e.g., in L. 551, that your “shear zones are explicitly resolved “ I would expect to see that demonstrated following the previous question, or such formulations implying “resolvedness” need to be updated.

* Physical implementation: issues and lack of clarity

Your description of the physical equations is not clear everywhere, such that the reader can not understand what you did or what is implemented. This is evident from my questions above, but also for inertia:

*** Inertia: description and implementation issues with unboundedness at large slip rates**
It is not clear if, and how, inertia in a physical form is implemented. Some formulations suggest so "Navier-Stokes equations (l. 57)" or "propagation of elastic waves " (l. 675). However, others do not.

- Equation 2.1 uses a fictitious mass-scaled density, and the method was originally designed to damp inertial forces and recover quasi-static equilibrium. Please clarify whether rho_f=rho during coseismic events, whether dynamic relaxation remains active and how this is tuned in relation to adaptive time stepping, and whether the resulting P- and S-wave speeds are physical. If not, the terms "dynamic," "dynamic rupture," and "elastic-wave propagation" should be accurately reformulated. The present formulation may be better described as a dynamically relaxed or mass-scaled explicit approximation.
- Also if you had inertia, you would not need an additional bound as a novel criteria in your adaptive time stepping, which you do in form of a local shear wave speed maximum (l. 586). You explain that you do need that to ensure that the fictitious density remains bounded when slip velocities approach shear wave speeds. However, physically, slip rate remains bounded due to inertia. Hence it is not clear why you need this.
- Without it is not fully dynamic, hence you should not claim earthquake-cycle dynamics is solved (e.g. in the title). And you would also need another energy sink to limit unbounded slip rate growth, which is in the absence of inertia typically done using a quasi-dynamic approximation (Rice et al., JGR, 1993). However, I do not see anywhere you implement that?
- I suspect that you cutoff maximum slip rates with your additional shear wave speed bound in your time stepping, rather than implementing a physical mechanism to limit rupture growth. This numerical trick may well impact your solution, may also contribute to the noise you observe towards the end of the event, and inhibits a proper verification and benchmarking of your solution.
- Since lines 675 and others discuss the simulation of waves and claim issues due to a linear approximation of waves, I wonder what your waves look like. And how you could simulate them without physical inertia. Could you show plots of the (underresolved) wave field in the supplements? And clarify how you simulate those?

The rheological model description varies throughout the paper. Sometimes correctly, but also sometimes less completely. For example, in the abstract you write about RSF being implemented in a MC elastoplastic structure, but latter I also see visco-elasto-plastic or elasto-visco-plastic and full implementations. Could you check all your rheology model statements and try to clarify what you do where?

* Benchmarking

The above issues illustrate the importance of benchmarking your solution also in an earthquake cycle setup.

Quickly reading your text that references to SCEC (State-wide California Earthquake Center) benchmarks of Sequences of Aseismic and Seismic Slip (SEAS) across several pieces of text makes the not careful reader think that you executed them, but you have not. First, could you not refer to those benchmarks if you did not execute them?

Instead you compared wether you would get nucleation, if friction increases and decreases, what maximum slip rates are, that you get recurrence and conclude that "cycle statistics are broadly in line with quasi-dynamic RSF sequence result (Lapusta.2000; Erickson.2020)". I have seen quite some simulations that reproduce these very general features, but in those there still could be many smaller and larger issues. Hence quantification and closer examination matters here.

At the start of your benchmarking section 5.4 you indeed describe that you "must reproduce physically reasonable nucleation, rupture and healing behaviour" (L. 593/4). However, you do not verify of any of the properties wether these are physically reasonable.

I understand that reproducing SCEC benchmark designed for non-volume-based codes using a volume-based code is an endeavour you would not like to start on, and is it not realistic to demand in this paper. However, several aspects can be tested independently and analytically. This involves inter seismic healing and post seismic relaxation, such as addressed in Figure 10 of Herrendorfer et al., (2018).

In addition, your recurrence pattern can be compared to Herrendorfer et al., (2018)'s Figure 4c. Since your setup is very similar to that one, such a comparison should be rather straightforward and very meaningful.
Other more detailed evaluations for the nucleation and propagation are available in there as well, which would be useful for evaluating those processes, but that can be regarded as beyond the scope of this study.

Several authors of volume-based codes have been discussing to setup a separate, new benchmark that is designed such that it can be robustly simulated and defined for volume-based codes. The idea is to start from a setup similar to Herrendorfer et al., (2018) and yours. I think it would be useful for your model to join this endeavour. However, until then and in any case, the large-scale cycle pattern of Herrendorfer et al., (2018) can be reproduced independently of the progress on that front.

Another way to evaluate wether you capture essential earthquake-cycle physics and reproduce its nucleation is via comparing / demonstrating that your simulations nucleate a "dynamic" rupture once the nucleation size is reached. How do your nucleation size and the theoretical nucleation size mentioned in L. 643 compare?

- L. 641-644 describes that you resolve the nucleation size because you use 4-5 elements across the fault width of 100m. However, in all other smaller width fault zone applications, nucleation size ALONG the fault is measured, such that ruptures from then on can propagate ALONG the fault. -> also holds? Update text

- Note also that if it were the across fault width that mattered, I would not term your simulation as "well resolved" as it is only just or not even resolved.

I appreciate the **new 'shear-box' benchmark** of a single element that you introduce, which can be used by others as well. However, the aspects discussed above are not tested in there. Also you impose the velocity field, which makes the element-level slip rate projection known in advance. This predefines the slip rate, but you argue in l. 621-622 that you test and validate the nodal-to-element velocity interpolation and the projection onto the maximum-shear-stress direction, i.e., the way in which you calculate the slip rate. I do not think you can verify your slip rate equation in this predefined slip rate benchmark with fixed stress orientation. You test one simple case where key things to test are defined.

Also I do not think based on this benchmark you can claim that "All three benchmark problems show excellent agreement between numerical and analytical results (Fig. 10b-d), validating the RSF implementation. " (L. 620). I agree that this benchmark verifies the local RSF constitutive update for prescribed constant-velocity simple shear. However, "validating the RSF implementation" seems too broad. The test does not verify the coupled system-level behaviour under evolving slip rates, including spontaneous nucleation, rupture propagation, arrest, healing, or earthquake recurrence. I suggest replacing "validating" with "verifying the local constitutive implementation" and clearly distinguishing this test from validation of the full earthquake-cycle model.

You study four models with RSF parameters to generate different types of sequences. All your cases have the fault being one property; either velocity-weakening (VW) or velocity-strengthening (VS). However, you do not have the transition between VW and VS then. It is at that transition where in nature nucleation if preferred, so if you want to study and claim reasonable nucleation you need that. Additionally, that transition is numerically harder to simulate, so you need to show you can do that well with one case. This is also done in all SCEC benchmarks.

If not a full SCEC or a targeted benchmark, then expect at least to verify different aspects as mentioned above. The state-of-the-art in method developments has reached that stage several years ago. Also since you in your abstract write that you build a robust platform, I think robustness should be demonstrated.

* Medium points *

- The abstract and many places throughout the manuscript write about "validation", but you are not checking that the model reproduces the physical system adequately. You do not compare to any laboratory or natural observations or discuss in that direction. You are doing "verification", i.e., wether your equations are implemented and solved correctly.

- Your abstract states “across temporal and spatial scales” and your introduction “to bridge the gap between long-term tectonic deformation and earthquake-cycle dynamics”. However, your simulations are done at fixed time frames; geodynamics OR earthquake cycle, so they do not bridge them by simulating both in the same framework. Could you update your formulations throughout and clarify the link with the latest paper as well? Koelzer et al., (2026) is under review like yours (https://doi.org/10.22541/essoar.15007647/v1), and there geodynamic and earthquake-cycle time scales are bridged and the functioning of earthquake-cycles under in a model that also in the same application shows tectonic deformation is clear.

- You do not describe the change from 2D to 3D as one of the main novelties (you only note it briefly at line 54). However, I think this is an important novelty, which was facilitated by your technical HPC work. Why was that not added as a key novelty?

- Since your off-fault behaviour would always weaken (or strengthen) with slip rate changes, can you still simulate static off-fault plasticity as is studied in dynamic rupture studies?

- Since you have a complex workflow with certain parts of it operating on CPU’s and others on GPU’s, are your timings and performance affected by availability of either resource? Does that mean that your code performs better on certain nodes that have an optimal balance between CPU and GPU architectures, such as available on the Grace Hopper?

- Your adaptive time stepping claims to resolve te state variable’s evaluation (l. 585), but you only have a time step criteria on the slip rate. Herrendorfer et al., JGR, 2018 suggests that also two criteria for are needed to resolve state weakening and healing.

- Your references and state-of-the-art description are missing another geodynamic earthquake cycle model; Sobolev et al., GGG, 2017. They also use a finite element model with plasticity (albeit Drucker-Prager), where only the friction coefficient is extended with RSF and a state evolution to simulate elastoviscoplastic earthquake cycles. Since this is the most similar model to yours (also see points below), could you clarify the novelty w.r.t. that model/paper?

* Minor points *

- At various locations you write about state-dependent restrengthening (e.g. l.72), but state evolution also weakens during earthquakes. -> clarify
- Why do you compare the Grace CPU and EPYC CPU in Figure 3, whereas the A100 and H100 performed better in Figure 2? -> Add justification
- L. 474: The references cited here do not all quantify off-fault plastic work in finite-thickness fault-zone models. Several instead concern fault-interface slip or field-scale hydromechanical evolution. Limit to those actually using finite-thickness fault zones or “plastic work” only if energy

dissipation is explicitly calculated. Petrini et al., Tectonophysics, 2020 is worthwhile in this finite thickness - off-fault plasticity - earthquake context.

- L. 476: In this introduction 5.1, is it fair to here only cite your own work?
- L. 477: Here, but also at other locations, the novelty of the current RSF implementation w.r.t. Tong and Lavier, 2018 and Lavier et al., 2021 is not always clear. -> add a few words on adding state evolution here and maybe elsewhere
- L. 492: You may want to discuss finite and heterogeneous fault zone in subduction zones (e.g. in relation to your old work) and wether fault geometries are well-known at depth here.
- L. 517: van Dinther et al., (2013)'s model is also elastic, not only viscoplastic; how else can earthquakes stick and slip?
- L. 518-522: Actually, Herrendorfer (2018)'s formulation is also a highly non-linear viscous one, because that allows it to match fundamental RSF assumptions with stress = strength. This is apparent in its supplements, but it is better explained in Koelzer et al., (2026).
- L. 549: Goudarzi et al. (2023) does not use only a single-cell-wide zone, but also has multiple cell shear zones, where the paper argues under which conditions these are resolved and how different implementations thereof compare.

References (not in manuscript)

Duretz, T., de Borst, R., & Le Pourhiet, L. (2019). On finite thickness of shear bands in frictional viscoplasticity, and implications for lithosphere dynamics. Geochemistry, Geophysics, Geosystems, 20, 5598–5616. https://doi.org/10.1029/2019GC008531

Petrini, C., Gerya, T., Yarushina, V., van Dinther, Y., Connolly, J., & Madonna, C. (2020). Seismo-hydro-mechanical modelling of the seismic cycle: Methodology and implications for subduction zone seismicity. Tectonophysics, 791, 228504. https://doi.org/10.1016/j.tecto.2020.228504

Sirorattanakul, K., Larochelle, S., Rubino, V., Lapusta, N. & Rosakis, A. J. Sliding and healing of frictional interfaces that appear stationary. *Nature* **639**, 947–953 (2025).

Koelzer, A., Mhina de Vos, Taras Gerya, Ylona van Dinther (under review, 2026) Bridging the Gap Between Millions of Years and Milliseconds: Modeling Earthquake Sequences and Splay Fault Rupture in Subduction Zones. ESS Open Archive. DOI: https://doi.org/10.22541/essoar.15007647/v1

Rice, J. Spatio-temporal complexity of slip on a fault. Journal of Geophysical Research 98, 9885–9907 (1993).

Preuss, S., Herrendörfer, R., Gerya, T., Ampuero, J. & Dinther, Y. Seismic and Aseismic Fault Growth Lead to Different Fault Orientations. *J Geophys Res Solid Earth* **124**, 8867–8889 (2019).

Sobolev, S. V. & Muldashev, I. A. Modeling Seismic Cycles of Great Megathrust Earthquakes Across the Scales With Focus at Postseismic Phase. Geochem Geophys Geosystems 18, 4387–4408 (2017).