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"Nucleation and propagation of brittle fracture as a constrained energy minimization problem"

Summary and recommendation

The manuscript proposes a sharp-interface continuum model for quasi-static brittle fracture under monotonic loading. Its central postulate is that (i) new crack surface may be created only in regions in which a prescribed material "strength surface" has been exceeded and (ii), subject to that constraint and irreversibility, crack evolution is selected by minimization of elastic potential energy plus Griffith surface energy. The paper then gives a restricted two-dimensional mathematical discussion and compares computations with the authors' "Nine Circles of Elastic Brittle Fracture" benchmark suite.

There is an interesting modeling idea here, and I agree that combining a strength criterion with an energetic fracture criterion might be useful in overcoming an important difficulty with regard to classical global-minimization models, namely, their treatment of nucleation. However, in my view the manuscript presently makes claims of generality and predictive status that are substantially stronger than what is mathematically or physically established. The main strength-constrained energetic criterion is also not new to this manuscript: for propagation of large cracks it was already introduced by Lopez-Pamies and Kamarei in 2025, and closely related strength-augmented variational ideas have appeared in the authors' earlier phase-field work. What is new here appears primarily to be the aim to promote those ingredients to a sharp, time-discrete nucleation-and-propagation framework and to demonstrate it on a common collection of examples.

The mathematical contribution, as presently written, is modest and contains issues that should be addressed before it can be regarded as a well-posed and fully-developed fracture theory. In particular, the paper explicitly fails to construct a limiting time-continuous quasistatic evolution, does not establish an energy–dissipation balance through jumps, and does not prove convergence or partition-independence of its discrete scheme. In addition, I see specific problems in Section 4 concerning the pointwise definition and compactness of the strength-violation set, a likely error in Proposition 4.1 as printed, and a mismatch between the abstract minimization problem and the much more restricted crack families used in the numerical examples.

On the materials science side, there is an enormous literature on crack nucleation that the work seems to ignore. Crack nucleation, even in nominally brittle materials involves complex material/processing specific mechanisms that occur at the microscale, including built-in and deformation-induced defects, microstructural features such as grain boundaries and PSBs, stress concentrators, free volume and cavitation, loading/environmental conditions, and other factors. Since the work is intended as a contribution to mechanics, the material-specific predictive ability of the nucleation theory should be carefully assessed through extensive comparisons to the experimental literature.

For these reasons, I do not recommend publication in the present form. For one thing, the caliber

of the authors, with many seminal contributions to their credit both in mathematics and mechanics, is such that they should be able–and expected–to do much better. In my opinion, the manuscript should be substantially reworked in a way that sharply limits its claims, fixes/completes the mathematical formulation, separates a modeling postulate from an established evolution theory, validates experimentally said modeling postulate, and provides genuinely topology-unbiased and independently calibrated validation.

1. What, precisely, is new?

1.1. Mathematical novelty

I do not find a deep new mathematical result in the present manuscript. Section 4 contains two kinds of statements. First, Proposition 4.1 gives a sufficient geometric/energetic condition intended to produce a material length-scale. Second, the paper sketches existence of a minimizer for a very restricted incremental problem in two-dimensional isotropic linear elasticity, under pure displacement loading and with cracks having a uniformly bounded number of connected components. The main analytical ingredients in the latter argument are standard Hausdorff compactness, a compactness lemma taken from Francfort–Giacomini–Lopez-Pamies, and Gołąb lower semicontinuity. The paper does not prove a continuous-time limit, energy balance, regularity, uniqueness, stability with respect to time discretization, or an existence result for the nonlinear finite-elasticity setting advertised in the introduction and abstract.

This is a particularly important distinction because the mathematical literature already contains rigorous constructions of quasistatic brittle-fracture evolutions obtained from time-incremental minimization. Dal Maso and Toader proved a two-dimensional continuous-time quasistatic evolution by passing to the limit in time-discrete minimization with irreversibility [3]; Francfort and Larsen obtained related existence and convergence results [4]; and Dal Maso, Francfort, and Toader treated n-dimensional nonlinear elasticity with quasiconvex bulk energy and time-dependent boundary deformations and applied loads [5]. Larsen subsequently developed an ε-stable local theory precisely to address deficiencies of global minimization at jumps [7]. Threshold-based quasistatic evolution has also been studied rigorously by Garroni and Larsen [6]. Against that background, Section 4 of the present paper should be presented as a preliminary and restricted well-posedness remark, not as a substantial new existence theory.

There are, moreover, several technical problems with the argument as currently written; I detail these below. Until those are corrected, even the restricted incremental existence claim should not be treated as established.

1.2. Mechanical and physical novelty

The paper's central mechanical postulate is not new in its propagation component. Lopez-Pamies and Kamarei [11] already proposed that growth of a large crack is permitted only in regions where the material strength surface has been exceeded, and that the crack selected within that region minimizes potential plus surface energy. The current manuscript itself acknowledges this antecedent. Earlier phase-field papers by this group also introduced material strength as an ingredient required to obtain realistic nucleation behavior [8, 9].

The potentially new step here is therefore narrower: the authors attempt to use essentially the same strength-constrained energy competition as a single sharp-interface rule for both nucleation and propagation, including finite crack jumps. That is a potentially useful modeling synthesis, but it should not be described as though the manuscript introduces a new physical mechanism of fracture. The model remains macroscopic and phenomenological. It does not identify a microscopic nucleation mechanism, a defect population, an interface-decohesion law, a microcracking mechanism, or a stochastic weakest-link process. The strength surface is an effective constitutive input and its predictiveness vis a vis crack nucleation in actual materials and conditions is not established in the paper.

1.3. Are the examples representative or selected to look favorable?

The example suite is broad in a phenomenological sense: it includes approximately uniform tension, biaxial loading and torsion; notch-driven nucleation; indentation; the poker-chip problem; and Mode-I/Mode-III propagation tests. That breadth is a real positive feature and a strength of the paper.

However, the computations are not a blind validation of the general crack minimization problem stated in Section 3. In many examples the admissible crack family is chosen using prior knowledge of the experimentally observed topology. The simple-tension and mortar examples explicitly say that the straight through-thickness crack topology is chosen for consistency with experimental observations. The notch, DCB and trousers calculations are restricted to straight or self-similar add-cracks; the indentation problem uses axisymmetric ring/conical cracks; and the poker-chip problem uses axisymmetric penny-shaped cracks. The authors state that certain alternative kinks or curved fronts were checked, but that is not equivalent to minimizing over the unrestricted class of cracks in the formal theory.

More importantly, the uniform-loading examples introduce deliberately weaker regions in order to localize nucleation: e.g. a central region with a 10% lower tensile strength in simple tension, and analogous weak regions in biaxial tension and torsion. Thus the "where" of nucleation is partly supplied as input. Such examples can demonstrate internal consistency, but they do not establish that the theory predicts fracture origin in a nominally homogeneous specimen with a realistic stochastic flaw population.

Finally, the "Nine Circles" benchmark was itself formulated by an overlapping author group, and much of the comparison in this manuscript is to "reference results" from that program and from the authors' earlier papers. This does not invalidate the examples, but it weakens the claim of independent validation. I would describe Section 5 as a useful collection of demonstrations, not as decisive validation of a general theory.

2. Evolution theory of fracture?

Modern rate-independent fracture theory routinely uses time-incremental minimization, irreversibility, and a limiting procedure to construct a quasistatic evolution [3, 4, 5]. However, the present manuscript stops before the evolutionary part is established. The authors explicitly state that they do not know the time-continuous formulation because the dissipation associated with strength and crack jumps is not understood. In the examples they even observe abrupt drops in the quasi-static

energy and note that kinetic or other dissipative mechanisms may be needed after nucleation. Once that is admitted, a sequence of minimizers at selected load levels does not yet constitute a closed evolution law.

At a minimum, a quasistatic rate-independent formulation should clarify the roles of: (i) an appropriate stability condition; (ii) the work of time-dependent loading; (iii) the energy balance or energy–dissipation inequality; and (iv) the jump selection rule. The last point is not cosmetic. Different notions of quasistatic evolution can produce different jump behavior. Larsen's ε-stable theory, for example, adds a path condition through jumps precisely because global energetic minimization can choose transitions that are not locally physically accessible [7]. Visco-energetic and vanishing-viscosity approaches provide other selections. If the present strength constraint introduces an additional dissipation, that dissipation should be an explicit part of the model, not merely noted after the fact.

Nor is the present scheme a de Giorgi minimizing-movement construction in any standard sense. A minimizing-movement scheme normally includes a metric/dissipative penalty that couples consecutive states and then studies the limit as the time step tends to zero. Here the coupling is essentially irreversibility plus a strength-based admissibility constraint; no dissipative distance or convergence theorem is given. If the authors intend a rate-independent incremental principle rather than minimizing movements, they should say so and formulate the corresponding stability and energy balance.

There is also a discrepancy between the formal discrete rule and the numerical narrative. In (7) and in Section 4, the admissible new crack at time t_k is constrained by the strength-violation set computed from the *previous* state, $V_F(t_{k-1})$ or Σ_{k-1}. In Section 2 and Section 5, by contrast, the text generally says that the load is increased until the strength criterion is violated at the current load, and cracks are then minimized in that current violation region. These are not literally the same algorithm. The former has a one-step lag and can depend on the chosen partition; the latter is closer to an event-driven update. The authors need one precise algorithm and should prove, or at least numerically demonstrate, convergence as the loading increment tends to zero.

Claims such as the DCB crack propagating in repeated 0.08 mm jumps or the trousers crack in repeated 0.28 mm jumps are especially sensitive to this issue. Without convergence with respect to load increment, crack-length sampling, mesh, and candidate-crack parametrization, it is premature to interpret these discrete increments as physical predictions rather than numerical/discretization selections.

3. The restricted incremental existence argument is not presently convincing

3.1. The strength-violation set is not well-defined at the stated regularity

Definition 4.4 takes the displacement in an LD-type space and hence the stress is, at best, an L^2 field. The set

$$\Sigma_{k-1} = \{X \in \Omega \setminus \Gamma_{k-1} : \sigma_{k-1}(X) \notin \mathring{K}\}$$

is then used as a *pointwise geometric constraint* on a one-dimensional crack set. This creates a basic measure-theoretic problem. An L^2 stress is an equivalence class defined only up to two-dimensional

Lebesgue-null sets, whereas an admissible crack is itself a set of zero two-dimensional measure but positive $\mathcal{H}^1$ measure. Changing the representative of σ on a Lebesgue-null set can therefore change whether an entire candidate crack is contained in Σ_{k-1}. As written, the admissible crack class can depend on the arbitrary representative chosen for an L^2 stress field.

Some additional structure is required: for example, enough regularity to obtain a continuous stress away from a controlled singular set, a quasi-continuous/precise representative together with the relevant capacity, or a different definition of the admissible region that is stable under the convergence used in the existence proof. The manuscript presently provides none of these.

3.2. The asserted compactness of Σ_{k-1} is not justified

Immediately after Definition 4.6 the manuscript states that $\Gamma_{k-1} \cup \Sigma_{k-1}$ is compact and invokes Hausdorff compactness. Under the stated assumptions this does not follow. Even after choosing a representative of an L^2 stress, the inverse image of the closed set $\mathbb{R}^{2\times 2} \setminus \mathring{K}$ under a merely measurable stress need not be closed. Hence Σ_{k-1} need not be compact. This is not a minor technicality, because the stability of the constraint

$$\Gamma_{k-1} \subset \Gamma \subset \Gamma_{k-1} \cup \Sigma_{k-1}$$

under Hausdorff convergence is exactly what the direct-method argument needs.

The authors should either prove the required regularity/closedness, redefine the violation set (for example by a carefully justified closure), or redesign the compactness argument. Taking a closure is not automatically innocuous: it can admit crack points at which the strength condition is not actually violated, especially near oscillatory stress fields.

3.3. Indexing and definition of the incremental update

Definition 4.6 asks for the infimum at time t_k over $\mathcal{A}(\bar{u}_k)$ and then says "Denote by (u_{k+1}, Γ_{k+1}) a minimizer." This appears to be an off-by-one error: the minimizer should presumably be (u_k, Γ_k). In isolation this could be a typographical mistake, but in combination with the previous-step strength set it makes the temporal logic of the scheme unnecessarily ambiguous and should be corrected carefully throughout.

4. Proposition 4.1 and the proposed length scale require substantial revision

There appears to be a concrete error in Proposition 4.1 as printed. Remark 3.6 defines K as a closed star-shaped strength domain containing 0 in its interior. Equation (14) optimizes over ∂K, but equation (15) contains

$$\min_{(\lambda_1,\lambda_2)\in K} \left(\lambda_1^2 + \lambda_2^2 - 2\nu\lambda_1\lambda_2\right)$$

in the denominator. Since $(0,0) \in \mathring{K}$, that minimum is zero (the quadratic form is positive definite for the stated range of ν). The right-hand side of (15) is therefore infinite/undefined, and the

proposition is vacuous as written. Presumably K should be ∂K, but the authors need to correct the statement and recheck the derivation.

Even after that correction, the physical meaning of the result is weaker than the discussion suggests. The sufficient competitor used to derive the estimate is $\Gamma = \partial\Omega$. Thus the argument establishes that it can become energetically favorable to separate the body from its entire boundary constraint, paying $G_c \operatorname{Per}(\Omega)$ and eliminating the bulk elastic energy. This is a boundary-debonding/detachment competitor, not an interior bulk crack nucleation construction. The issue is especially conspicuous because Section 4 has deliberately dropped the "unbreakable boundary" condition used elsewhere in the model. A material length scale derived from this competitor should not be interpreted as a general intrinsic nucleation length without further argument.

If the authors want a bulk-nucleation result, they should restrict cracks away from the loading boundary and construct an admissible interior crack competitor. It would then be useful to determine whether the claimed scaling survives and how it depends on geometry and on the chosen strength surface.

5. Finite elasticity: the manuscript's generality claims greatly exceed its analysis

The Abstract and Introduction say that the theory applies to "any elasticity (linear or nonlinear)." At the purely formal level one can of course write the functional for a nonlinear stored energy. That is not the same as having a mathematically meaningful minimization problem. Even without fracture, existence in finite elasticity requires nontrivial hypotheses concerning coercivity, lower semicontinuity/quasiconvexity or polyconvexity, orientation preservation, and often global injectivity or non-interpenetration. Cracks and contact make these issues harder, not easier.

The particular finite-elastic model used for the rubber raises an additional issue. The nonlinear elastic calculations in Section 5 use the compressible Neo-Hookean energy

$$W(F) = \frac{\mu}{2}(I_1 - 3) - \mu \log J + \frac{\Lambda}{2}(J-1)^2, \qquad I_1 = \operatorname{tr}(F^T F) = |F|^2, \qquad J = \det F > 0.$$

It is important to distinguish this model from the poorly behaved Saint-Venant–Kirchhoff model. In fact, the above energy has some quite good mathematical properties. It may be written as

$$W(F) = \frac{\mu}{2}|F|^2 + h(\det F) - \frac{3\mu}{2}, \qquad h(J) = -\mu \log J + \frac{\Lambda}{2}(J-1)^2,$$

and

$$h''(J) = \frac{\mu}{J^2} + \Lambda > 0.$$

Thus, for positive material constants, the stored-energy density is polyconvex (and in particular rank-one convex) on $\{\det F > 0\}$, and $W(F) \to +\infty$ as $\det F \downarrow 0$. The difficulty is therefore not that the constitutive density is an obviously pathological finite-elasticity model. Rather, the difficulty is that these properties alone do not provide the well-posed three-dimensional fracture minimization problem required by the manuscript.

A particularly serious point is the quadratic growth in F. In three dimensions this is a $p = 2 < 3$ growth regime in which finite-energy cavitating Sobolev deformations are not excluded. This can be seen directly from the standard radial cavitation map

$$y(X) = \frac{r(R)}{R} X, \qquad R = |X|, \qquad r(R) = (R^3 + a^3)^{1/3}, \qquad a > 0.$$

This deformation opens a cavity of finite radius a at the material point $R = 0$. Nevertheless,

$$\det \nabla y = 1$$

away from the origin, while the tangential stretches behave as $O(R^{-1})$. Consequently,

$$\int_B |\nabla y|^2 \, dX < \infty$$

in three dimensions, since the singular contribution behaves locally like

$$\int_0^{\varepsilon} R^2 \, R^{-2} \, dR < \infty.$$

For the very Neo-Hookean energy used in the manuscript, the volumetric terms vanish for this example because $J = 1$, and hence the total bulk elastic energy remains finite. This is the classical mechanism behind the mathematical theory of cavitation in nonlinear elasticity; see, for example, Ball [**?**].

This observation creates a conceptual difficulty for the proposed fracture functional that should be addressed explicitly. The model assigns the surface cost

$$G_c \mathcal{H}^2(\Gamma)$$

only to the explicitly introduced crack set Γ. A cavitating Sobolev deformation, however, can create a finite cavity in the deformed configuration while the singular set in the reference configuration is only a point and one may formally have $\Gamma = \varnothing$. In that case the deformation has created new physical free surface while the surface term in (6) assigns no corresponding Griffith cost. Thus the proposed admissible class should either exclude such cavitating deformations by a precise mathematical condition, or the energy should be augmented so that free surface generated by singular deformations is properly accounted for. The phrase "satisfies material impenetrability" in (7) does not, by itself, resolve this issue, nor does the usual local condition

$$\det \nabla y > 0 \quad \text{a.e.}$$

resolve it. Positivity of the Jacobian is a local orientation-preservation condition, not a complete global non-interpenetration or non-cavitation condition. Indeed, the radial map above has positive Jacobian away from its singular point. If the authors intend a stronger global-invertibility, INV-type, or non-cavitation condition, it should be stated as part of the admissible class and its compatibility with the compactness argument needed for existence should be demonstrated.

This issue is especially relevant to the examples chosen in the paper. The poker-chip calculation is performed precisely for a nearly incompressible rubber under a strongly nonuniform stress field with a large hydrostatic tensile component near the specimen centerline. This is one of the canonical physical settings in which cavitation is relevant. Consequently, it is not enough to prescribe an explicit penny-shaped crack family and compare its energy with that of a smooth elastic solution.

One should also explain why admissible singular elastic deformations that open cavities are excluded, or else include them among the competitors and charge the associated newly created surface. Without such a specification, the nonlinear minimization problem being solved numerically is not evidently the minimization problem stated in (5)–(7).

Accordingly, I would separate two claims that are currently conflated. The particular Neo-Hookean density used for rubber is a respectable polyconvex finite-elastic energy; this is a point in its favor. But the well-posedness of the corresponding three-dimensional fracture problem, with cavitation, global non-interpenetration, crack formation, and surface creation all treated consistently, does not follow from polyconvexity and is not established in the manuscript.

Ball's classic work gives important positive existence results under polyconvexity and suitable growth hypotheses [1], but it also makes clear how far the general theory remains from arbitrary constitutive behavior. In his 2002 open-problems article, Ball writes, in the relevant discussion of quasiconvex energies, that

> "none of the existence theorems for minimizers of integrals of general quasiconvex functions apply to elasticity"

under the physically relevant determinant behavior being discussed there [2]. This quotation should not be misread as saying that finite elasticity has no existence theory; Ball's own polyconvex theory is a foundational counterexample to such a blanket statement. The correct conclusion is that a claim covering arbitrary nonlinear elastic energies is mathematically unjustified without precise structural hypotheses.

The present manuscript simply writes "material impenetrability" as an admissibility requirement, but does not define the function space, orientation constraint, global injectivity/non-interpenetration condition, or crack-face contact condition needed to make that requirement precise. In the nonlinear rubber examples these questions are not peripheral: large deformation and possible contact are part of the intended regime.

There is also a frame-indifference issue in the definition of strength at finite deformation. Throughout the paper S is the first Piola–Kirchhoff stress, yet the isotropic strength surface is described in terms of "principal nominal stresses" or eigenvalues of S. A first Piola stress is generally nonsymmetric and is a two-point tensor. Under a superposed spatial rotation Q, it transforms as $S \mapsto QS$; its ordinary eigenvalues are not objective invariants (and for a general nonsymmetric real matrix need not even furnish three real "principal stresses"). Remark 3.6 asserts frame indifference but does not resolve this problem. The authors need to specify an objective finite-strain stress measure and transformation rule, or give a rigorous definition of what they mean by the principal nominal stresses and prove that $F(S)$ is frame indifferent.

It is particularly relevant that Dal Maso, Francfort, and Toader already developed a quasistatic variational crack-growth theory in n-dimensional nonlinear elasticity under explicit hypotheses on a quasiconvex bulk energy and on the loading [5]. The present paper could engage with that result at a technical level instead of making a formal claim of arbitrary nonlinear applicability.

6. Treatment of body forces and tractions

The manuscript correctly identifies a genuine difficulty with dead loads: once a crack detaches a component that is not kinematically anchored, a nonzero resultant force or moment can drive rigid translation/rotation and send the potential energy to $-\infty$. This is an important coercivity issue.

However, the historical statement in the introduction that the variational approach has an "apparent inability to handle non-zero Neumann boundary conditions and/or body forces" is too broad. The 2005 paper of Dal Maso, Francfort, and Toader explicitly proves an existence result in nonlinear elasticity with prescribed boundary deformations and applied loads depending on time [5]. The precise issue is not that variational fracture cannot contain loads; it is that particular load classes, especially genuinely dead loads acting on components that can detach and escape by rigid motion, can destroy coercivity. The manuscript should state that distinction accurately and explain exactly which load assumptions in the existing existence theories exclude the pathology considered here.

There is also a tension between the theoretical limitations and the demonstrations. Section 4 proves at most a pure-Dirichlet, no-body-force/no-traction incremental statement, while Section 2 prominently uses force control and self-weight as examples supporting the general theory. Those examples lie precisely in the regime for which the authors say their minimization may have infimum $-\infty$ and no general existence theory. Numerical success for a restricted crack family does not resolve that foundational issue.

7. What is the "strength surface"? Defined, yes; established as an intrinsic material property, not yet

Definition 1.1 gives an operational definition of the strength surface: the strength of an elastic brittle material is the collection of critical stress states at which fracture occurs under monotonically increasing, spatially uniform stress, and the boundary of that set is denoted $F(S) = 0$. That definition is useful and should be credited as such.

What remains unclear is whether this object has the deterministic, specimen-independent constitutive status that the subsequent theory assigns to it. Brittle strength is notoriously controlled by flaw populations and stressed volume/area. In ceramics and glasses, fracture-controlling flaws may be surface cracks, pores, inclusions, edge flaws, microcracks, or other processing defects, and measured strength is commonly treated statistically. The relationship between flaw populations, component size, and strength distributions is well established in the materials literature [12, 13]. The manuscript itself acknowledges random spatial variations in strength when explaining why nucleation location is uncertain, and then deliberately inserts weaker regions in several examples.

The authors therefore need to state whether $F(S) = 0$ is intended as (a) a deterministic property of an ideal homogeneous continuum; (b) an effective mean/quantile surface associated with a standardized specimen size and flaw population; or (c) a random field $F(X, S, \omega)$. These alternatives have different consequences. If strength is stochastic and size-dependent, then a single deterministic star-shaped surface is not by itself a complete material description for nucleation in an arbitrary structure.

The phrase "crack nucleation" also risks conflating a macroscopic event with a microscopic mechanism. For the materials considered here, the continuum model is best viewed as coarse-graining the actual

defect physics. In brittle glasses and ceramics, initiation is commonly associated with pre-existing flaws and their statistics; in heterogeneous materials, interfaces, inclusions and microstructural defects can control the fracture origin. The authors do not need to resolve all microphysics, but they should make explicit that the strength surface is a phenomenological macroscopic closure, not a mechanistic theory of crack nucleation.

A related concern is the proposal in the final section to infer the complete strength surface by inverse calibration from nonuniform fracture tests. Such calibration can be useful, but it creates an obvious validation problem: the same class of fracture experiments cannot simultaneously determine the unknown strength surface and serve as independent evidence that the fracture theory is predictive. Calibration and validation data should be separated.

8. Numerical examples do not solve the stated minimization problem

The formal theory minimizes over crack sets Γ, but the computations generally minimize over one or two geometric parameters describing a preselected crack family. This is a much smaller problem. In several cases the paper effectively asks: among straight cracks of an experimentally anticipated orientation family and bounded by the computed strength-violation region, which length/angle has lowest energy? Success in that reduced search does not demonstrate that the unrestricted crack functional has the same minimizer.

This point is central because the claimed advantage of the theory is that it predicts not only *when* but also *where* and along what path cracking occurs. If the candidate topology and much of the path are supplied in advance, the strongest part of that claim has not actually been tested.

I therefore believe the paper needs at least one genuinely unbiased computational experiment. For example, the authors could use a sharp-crack topology-optimization method or a demonstrably converged diffuse regularization that allows multiple competing nucleation sites, branching, curved cracks, mother/daughter cracks, and topology changes without prescribing the observed failure mode. The inputs (elasticity, toughness, and strength data) should be calibrated from separate experiments, and the predicted fracture load, origin, path, and final topology should then be compared with a held-out experiment.

The paper should also report systematic convergence/sensitivity studies. At present I do not see enough information to determine whether the reported finite crack jumps and preferred paths are stable with respect to mesh size, load increment, angular sampling, crack-length sampling, the dimensions of the artificially weak region, and the thickness of the "unbreakable" boundary neighborhood. The last item is particularly important because the manuscript admits that strength is exceeded near grips/corners in some examples but then suppresses fracture near loaded boundaries through an ad hoc reinforcement rule.

9. Specific points that could be addressed in a revision

1. State the novelty much more narrowly. Separate (a) the previously published strength-constrained propagation criterion, (b) the new proposed extension to nucleation, and (c) the limited mathematical results actually established here.

2. Replace the phrase that crack "evolution is dictated by minimization" by a precise time-discrete

formulation, and explain what would be required for a time-continuous quasistatic evolution. State explicitly that this limit is currently open for the proposed strength-constrained model.

3. Give a mathematically well-defined strength-violation set at the regularity used in Section 4. Address the dependence on representatives of an L^2 stress and prove the closedness/compactness needed for Hausdorff convergence.

4. Correct Definition 4.6's indexing and explain whether the admissible crack at t_k is constrained by the previous stress state or the current trial stress state. Reconcile the formal scheme with the numerical algorithm.

5. Correct Proposition 4.1. As printed, the minimization over K in (15) includes the origin and makes the denominator vanish. If ∂K is intended, say so and rederive the result. Also distinguish boundary detachment ($\Gamma = \partial\Omega$) from bulk crack nucleation.

6. For nonlinear elasticity, specify admissible stored energies, coercivity and lower-semicontinuity assumptions, orientation preservation, non-interpenetration/global injectivity, and crack-face contact. Otherwise remove or substantially qualify the claim that the theory applies to "any" nonlinear elasticity.

7. Reformulate the finite-strain strength criterion so that frame indifference is explicit. The use of eigenvalues/principal values of the first Piola stress requires justification and, as written, is not objective in general.

8. Revise the discussion of loads and prior variational fracture literature. In particular, distinguish noncoercive dead-load detachment from the broader treatment of time-dependent applied loads in existing existence theories.

9. Recast the strength surface as an effective macroscopic constitutive object and discuss flaw statistics, specimen size/stressed volume, surface versus bulk flaw populations, and calibration uncertainty. Explain whether spatial strength variability is deterministic or stochastic.

10. Provide at least one validation in which crack topology and nucleation location are not inserted through a restricted ansatz or prescribed weak region. Separate calibration experiments from validation experiments and report convergence with respect to all numerical discretizations.

10. Minor and presentational comments

The manuscript frequently uses language such as "complete spectrum," "satisfactory answer," and "failure to resolve even a single circle disqualifies a theory." Given that the benchmark is newly introduced by an overlapping author group and that the current calculations employ reduced crack ansatzes, this rhetoric is too strong. More neutral language would improve the paper.

The term "brutal propagation" is used repeatedly. If the intended meaning is a finite jump in a rate-independent evolution, it would be better to use standard terminology (jump discontinuity, unstable extension, dynamic rupture, etc.) and distinguish these possibilities. A finite jump in a quasistatic state variable is not automatically the same physical event as rapid inertial fracture. More importantly, coarse terms such as "brutal" are not acceptable in formal settings.

The paper should distinguish carefully among first Piola, Cauchy, second Piola, Kirchhoff, and any other stress measures used in defining strength. In the linearized section these distinctions collapse, but they do not in finite elasticity.

Finally, the manuscript's numerical minimization language should always specify "within the tested crack family" unless an actual global minimization over the full admissible crack class has been performed.

Recommendation

The manuscript contains a potentially interesting macroscopic modeling device, but in its present form it does not establish the general fracture evolution that the title, abstract, and conclusions suggest. The strongest physical criterion has substantial overlap with the authors' 2025 work; the new mathematical analysis is restricted and, as written, has concrete gaps/errors; and the numerical examples rely heavily on experimentally informed crack *ansätze* and, in several cases, prescribed weak regions. I therefore recommend rejection in the present form, in the believe that the authors are capable of substantially improving the manuscript with a modicum of effort.

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