1 Major comments

1.1 Major comment 1. Eq. (9) explicitly requires a component-wise Archie assumption and an interpretation in the conductivity domain.

The central electrical model assumes that the three pore classes contribute to the electrical response as a parallel circuit, that is, additively. The manuscript first writes this idea as a parallel resistance relation,

$$\frac{1}{R_0} = \frac{1}{R_e} + \frac{1}{R_h} + \frac{1}{R_v},$$

and then writes it as a resistivity relation,

$$\frac{1}{\rho_0} = \frac{1}{\rho_e} + \frac{1}{\rho_h} + \frac{1}{\rho_v}.$$

After that, the manuscript presents the following model:

$$\frac{\rho_w}{\rho_0} = \phi_e^{m_e} + \phi_h^{m_h} + \phi_v^{m_v}.$$

This last step cannot be obtained by "substituting Eq. (6) into Eq. (8)," as written in the text. Eq. (6) only describes the stress-dependent evolution of the primary fracture porosity. Eq. (6) does not define the relation between component resistivity and component porosity. To derive Eq. (9), the authors must further assume an Archie-type relation for each conductive component:

$$\frac{\rho_w}{\rho_i} = \phi_i^{m_i}, \qquad i = e, h, v.$$

In addition, this model may be better written directly as conductivity contributions, rather than as a conversion from R to ρ. At least, from the formulation using R and from Fig. 2, it is clear that it cannot be immediately converted into ρ.

$$\sigma_0 = \sigma_e + \sigma_h + \sigma_v,$$

with

$$\sigma_i = \frac{1}{\rho_w}\phi_i^{m_i}, \qquad i = e, h, v.$$

Then Eq. (9) is obtained as

$$\frac{\rho_w}{\rho_0} = \phi_e^{m_e} + \phi_h^{m_h} + \phi_v^{m_v}.$$

Actually, this development is physically different from the simple idea suggested by looking at Fig. 2, namely "dividing the specimen into three independent spatial subdomains." If that were the case, the conversion from resistance to resistivity would require area ratios. A more accurate interpretation is that these three components are virtual conductivity contributions, or paths, defined in the same representative domain under the same macroscopic potential gradient. In other words, it is equivalent to considering three patterns of virtual resistance components obtained when no other path exists in the same unit area, and then connecting those contributions in parallel. In this sense, $\phi_i^{m_i}$ is not a simple geometrical area fraction, but an effective conductive contribution of pore class i, including connectivity, tortuosity, constriction, and isolation effects.

From the above, the authors should correct the derivation of Eq. (9), and explicitly state that Eq. (9) combines Eq. (8) with a component-wise extension of Archie's law. The current description that Eq. (9) follows from Eq. (6) and Eq. (8) is mathematically incorrect.

1.2 Major comment 2. The manuscript currently shows curve fitting, not independent model validation, and parameter identifiability is not established.

The manuscript states that the model was validated by substituting experimentally obtained parameters into Eq. (39) and comparing the calculated curve with the measured curve in Fig. 7. However, some parameters, including the parameters in Tables 1 and 2, are estimated purely by data fitting. In this configuration, the comparison mainly tests whether a flexible parameterized function can reproduce the calibration data.

This is especially important because this model contains a relatively large number of parameters:

$$F_0, \quad n, \quad u_m, \quad \phi_0, \quad \gamma, \quad E, \quad m_e, \quad m_h, \quad m_v,$$

whereas the resistivity-stress curves used for fitting are comparatively simple and smooth. Therefore, the apparent agreement between the calculated curve and the observed curve in Fig. 7 may reflect the flexibility of the model rather than independent predictive ability.

The authors should clearly separate the following categories of parameters:

1. parameters directly measured before or during the experiment, such as specimen dimensions, pore-fluid resistivity, independently measured porosity, elastic modulus, or peak stress;
2. parameters calculated from the mechanical peak-point conditions, for example F_0, n, and u_m, if they are really obtained only from Eqs. (36)–(38);
3. parameters fitted to the resistivity curves, for example m_e, m_h, and m_v, if applicable;
4. parameters fixed or constrained during the fitting.

Furthermore, if nonlinear fitting is used, the fitting problem should be explicitly defined. For example,

$$\hat{\boldsymbol{p}} = \arg\min_{\boldsymbol{p}} \sum_{i=1}^{N} \left[\rho_{\mathrm{obs}}(\sigma_i) - \rho_{\mathrm{model}}(\sigma_i; \boldsymbol{p})\right]^2,$$

where $\boldsymbol{p}$ represents the relevant parameter vector.

The authors should provide quantitative information about the fit and parameter identifiability. At minimum, the manuscript should report residuals as a function of deviatoric stress, RMSE, normalized RMSE, or relative error for each confining pressure.

Especially for m_h, m_v, γ, ϕ_0, and u_m, parameter uncertainty or confidence intervals should also be shown. Because this model is nonlinear, the possibility of non-unique solutions, local minima, or multimodal parameter sets should also be discussed. In general, parameters that contribute through power operations make the nonlinearity very strong, and when different unknowns are simultaneously included inside and outside power operations, the ranges of parameter-estimation errors can mutually become large. The statistical validity of the fit should be evaluated not only by visual agreement but also against measurement variance or estimated observational uncertainty.

Stronger validation could include tests such as comparison with a simpler baseline model, for example a single-porosity Archie-type model with stress-dependent porosity; calibration on part of the loading path followed by forward prediction of the remaining path; or cross-validation in which several parts of the measurement interval are removed. Without such tests, the conclusion that the model “accurately reflects” resistivity evolution is stronger than the evidence supports.

1.3 Major comment 3. The electrical measurement protocol needs a more precise treatment of electrode polarization, contact impedance, and complex impedance.

In the experimental section, the manuscript states that a two-electrode method was used, that the total resistance was measured by a digital bridge, and that a test frequency of 100 Hz was used to avoid electrode polarization. This description is not sufficient. In a two-electrode measurement, the measured impedance is generally

$$Z_{\mathrm{meas}}(\omega) = Z_{\mathrm{sample}}(\omega) + Z_{\mathrm{el},1}(\omega) + Z_{\mathrm{el},2}(\omega) + Z_{\mathrm{contact}}(\omega) + Z_{\mathrm{lead}}(\omega).$$

Here, $Z_{\mathrm{meas}}(\omega)$ is the total complex impedance measured by the bridge at angular frequency ω, and $Z_{\mathrm{sample}}(\omega)$ is the intrinsic impedance of the rock specimen that should be converted into sample resistivity. The two terms $Z_{\mathrm{el},1}(\omega)$ and $Z_{\mathrm{el},2}(\omega)$ denote the electrode-interface impedances at the two electrodes, including electrode polarization. $Z_{\mathrm{contact}}(\omega)$ denotes the contact impedance associated with the electrode-specimen contact, including contact resistance or capacitance caused by electrode material, conductive paste or foil, end caps, surface roughness, saturation state, and contact pressure. $Z_{\mathrm{lead}}(\omega)$ denotes the impedance of leads, cables, connectors, and the measurement cell outside the specimen. I use this decomposition only to state the measurement problem; these terms do not have to be separated in the final model if the authors can show that the non-sample terms are negligible at 100 Hz.

A single measurement frequency does not, by itself, show

$$|Z_{\mathrm{el},1}(\omega) + Z_{\mathrm{el},2}(\omega) + Z_{\mathrm{contact}}(\omega)| \ll |Z_{\mathrm{sample}}(\omega)|.$$

This problem is especially important because the manuscript interprets the resistivity decrease near failure as a change in pore connectivity. Near failure, contact conditions, local saturation, crack-fluid pathways, electrode-sample coupling, and sample geometry may also change. A two-electrode measurement can mix these effects with the intrinsic sample response.

The authors should add one or more of the following:

1. multi-frequency impedance spectra before loading, during representative loading stages, and near failure;
2. phase-angle data showing that the measured response at 100 Hz is dominantly resistive;
3. comparison between two-electrode and four-electrode measurements, or justification based on the system geometry and the expected contact impedance;
4. control measurements using fixed resistors or brine-filled standards under pressure;
5. explicit reporting of electrode material, electrode-sample contact method, contact pressure, bridge model, voltage/current amplitude, and data acquisition rate;
6. most simply, the error range between the expected resistivity value of a standard sample, which is necessarily required for the measurement, and its measured value.

The authors cite the previous THMC resistivity apparatus paper by Ren et al. (2023). The present manuscript should either summarize the relevant calibration results from that system or explicitly refer to the section where electrode and contact effects were evaluated.

1.4 Major comment 4. Stage divisions should be defined quantitatively.

The manuscript divides pore-volume evolution into Stages I–IV and resistivity evolution into Stages A–D, and uses these divisions to interpret the underlying deformation and fracture processes. However, the criteria for determining the stage boundaries are not quantitatively defined. It is not clear whether the boundaries are determined by slope, curvature, crack-initiation stress, dilatancy onset, peak resistivity, pore-volume minimum, acoustic emission, or visual/manual inspection of the curves. This point is also related to model validation, especially if a cross-validation type procedure is adopted.

Because the stage interpretation is an important concept in the proposed physical model, the authors should provide explicit criteria for the stage divisions. If the criteria follow previous work, the relevant references and operational definitions should be shown. If the divisions are based on the curves in this study, the boundary stresses should be reported, preferably also with normalized stresses such as σ/σ_p. This would make the interpretation reproducible and reduce the risk of subjective stage assignment.

1.5 Major comment 5. The physical interpretation of negative u_m should be explained.

Table 1 reports negative u_m values for all confining pressures:

$$u_m = -3.60,\ -1.72,\ -1.30,\ -0.45.$$

However, u_m is introduced as the expansion coefficient at peak stress, and the manuscript does not explain the physical meaning of the negative values. In Eq. (29), u_m controls the sign and magnitude of the plastic volumetric strain contribution. Under the usual assumptions

$$E > 0, \quad \sigma'_{1p} > 0, \quad \sigma'_{1s} > 0, \quad \sigma'_1 > \sigma'_{1s},$$

negative u_m seems to imply a negative plastic volumetric strain contribution. This may be consistent with the authors' sign convention for pore compression or volumetric strain, but that convention and the physical interpretation should be explicitly stated.

The authors should explain whether negative u_m represents compaction-dominated plastic deformation, a result of the adopted sign convention, or an artifact of the fitting or parameter-estimation procedure. This clarification is important because u_m is later used in the pore-volume evolution model and affects the predicted resistivity evolution through ϕ_v.

1.6 Major comment 6. The hierarchy of plastic volumetric strain in Eqs. (25)–(30) is ambiguous and should be reorganized.

The derivation from Eq. (25) to Eq. (30) is difficult to trace because the manuscript does not clearly distinguish the level at which the plastic volumetric strain is being defined. In Eq. (25), ε_v^p is introduced as the bulk strain of the damaged part, and the overall volumetric strain increment is written as a volume-fraction-weighted superposition of the undamaged and damaged components:

$$d\varepsilon_v = (1-D)d\varepsilon_v^e + Dd\varepsilon_v^p.$$

With this definition, $d\varepsilon_v^p$ is naturally read as the strain increment of the damaged component at the representative-volume scale. Eqs. (26) and (27) appear to continue this interpretation, by relating the plastic volumetric strain increment of the damaged component to the axial strain increment through Rowe's shear expansion relation.

However, Eq. (28) introduces σ'_{1s}, which is the effective axial stress corresponding to the initial failure of a rock unit. At this point, the meaning of ε_v^p seems to move from the damaged-component-scale strain to the plastic volumetric strain accumulated in a particular failed rock unit from the failure onset at σ'_{1s} to the current effective stress σ'_1. This change of hierarchy is not explicitly stated, and therefore the derivation is difficult to trace.

For clarification, the authors should introduce a different symbol for the plastic volumetric strain of a failed rock unit, for example

$$\varepsilon_{v,s}^p = \frac{u_s}{E}\left(\sigma'_1 - \sigma'_{1s}\right),$$

where σ'_{1s} is the effective axial stress at which that unit first fails, and u_s is the expansion coefficient assigned to that unit. Then Eq. (29) can be understood as the result of substituting the strength-dependent expansion coefficient

$$u_s = \frac{\sigma'_{1s}}{\sigma'_{1p}} u_m,$$

into the unit-level expression:

$$\varepsilon^p_{v,s} = \frac{u_m}{E\sigma'_{1p}}\sigma'_{1s}\left(\sigma'_1 - \sigma'_{1s}\right).$$

Finally, Eq. (30) should be explicitly explained as the volume-fraction-weighted superposition of plastic volumetric strain contributions from all failed rock units:

$$\varepsilon^p_v = \int_0^D \varepsilon^p_{v,s}\, dD'.$$

This clarification is important because the current notation gives the impression that the same variable ε^p_v represents, at different points in the derivation, the strain of the damaged component as a whole, the strain of a particular failed rock unit, and the total plastic strain obtained by superposition over all failed units. The mathematical intention becomes understandable only after Eq. (30), but the notation and explanation should be reorganized so that readers can follow the transition from the representative-volume description in Eq. (25) to the unit-level integration in Eqs. (28)–(30).

1.7 Major comment 7. The traceability of the mathematical derivation is low for a central model.

The proposed model depends on a long sequence of mathematical transformations from the statistical damage constitutive equation to the final resistivity evolution model. However, several parts of the derivation are difficult to verify from the manuscript as written. I think this is not only a presentation problem. Because the final model directly depends on intermediate quantities derived in Sections 3.2.2 and 3.2.3, insufficient traceability makes it difficult for readers to reproduce or evaluate the model.

In particular, the derivation includes several changes of variables and changes in the hierarchy of variables, such as the transition from the representative-volume description in Eq. (25) to the failed-unit-level expression in Eqs. (28)–(30), the transformation from D to F in Eq. (31), and the use of peak-point conditions in Eqs. (34)–(37). These steps are highly compressed, and in several places the notation appears inconsistent or incomplete. Examples are listed in the minor comments, including the definition of u, the use of incomplete gamma functions in Eqs. (31)–(33), and the apparent omission of the initial-compression correction term in Eqs. (34) and (35).

I listed various issues about equation transformations in the minor comments, but here I give one example as the most serious one. It is Eq. (39), which is the final predictive form of the model. The first line of Eq. (39) appears to follow from the three-porosity Archie-type relation in Eq. (9), but the individual porosity components are not explicitly derived. The term for the elastic pore component seems to be reconstructed as

$$\phi_e = (1-\gamma)\phi_0 - \frac{(1-2\mu)(\sigma_1-\sigma_3)}{E},$$

and this seems to represent the initial non-fracture pore volume minus the elastic volumetric compression under deviatoric loading. However, the transition from Eq. (5) to this expression is not shown, especially the replacement of σ_e by $\sigma_1 - \sigma_3$ and the treatment of the related numerical coefficient. The component

$$\phi_h = \frac{\gamma\phi_0}{3}\exp\left[-\frac{3\lambda(\sigma_1-\sigma_3)}{E}\right]$$

appears to be taken from Eq. (23), but the manuscript does not explicitly state that the primary-fracture deformation ε_s is identified with ϕ_h. The component

$$\phi_v = \frac{2\gamma\phi_0}{3} + \varepsilon^p_v$$

appears to consist of the remaining two-thirds of the initial primary-fracture porosity, and the damage-induced plastic volumetric strain from Eq. (31). However, the origin of the term $2\gamma\phi_0/3$ is not explained, and the substitution of Eq. (31) into ϕ_v is not shown. Because Eq. (39) is the final model used for comparison with the experimental data, the authors should explicitly show how ϕ_e, ϕ_h, and ϕ_v are obtained from the preceding equations. Other equation-specific problems are listed in the minor comments, but Eq. (39) is the clearest example where the lack of intermediate definitions and variable correspondence materially affects reproducibility.

I recommend that the authors improve the traceability of the derivation by adding intermediate equations, explicitly defining variable transformations, and using distinct symbols for quantities defined at different levels, such as representative-volume quantities, failed-unit quantities, and volume-fraction-weighted total quantities. These revisions would greatly improve the reproducibility and readability of the model without necessarily changing its physical basis. If the manuscript text becomes too complicated, and if the format allows it, it would be desirable to trace the equations in an Appendix.

1.8 Major comment 8. Specimen number, repeatability, and specimen-to-specimen variability should be clarified.

The manuscript reports tests under four confining pressures, but it is not clear how many specimens were tested at each confining pressure, whether the curves in Figs. 4–7 are single-specimen results or representative curves, and whether repeatability was evaluated. This information is important because the confining-pressure dependence of parameters such as

$$\phi_0, \quad \gamma, \quad E, \quad u_m, \quad m_h, \quad m_v$$

can be affected by specimen-to-specimen variability.

The authors should state the number of specimens tested under each condition and, if available, provide variability estimates. If only one specimen was tested for each confining pressure, the authors should present the pressure-dependent trends more cautiously as observations from the tested specimens, rather than as statistically established confining-pressure effects.

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2 Minor comments

1. **Archie's formula should include its assumptions and possible prefactor.**
   Eq. (1) is written as $F = \rho_0/\rho_w = 1/\phi^m$. In many applications, $F = a\phi^{-m}$ is used, where a is a tortuosity or lithology-dependent coefficient. The authors should state why $a = 1$ is assumed. Because the limestone contains 9.3% illite, the possible contribution of surface conduction should also be discussed, even if it is finally neglected.

2. **The notation for stress variables is inconsistent.**
   The manuscript uses σ_e, σ_{ij}, σ'_{ij}, σ_p, P_c, and P_w. A notation table would improve clarity. In particular, σ_p in Figs. 4–7 seems to mean deviatoric stress, probably $\sigma_1 - \sigma_3$, but this should be explicitly defined.

3. **The unit label in Fig. 7 appears inconsistent.**
   Fig. 7 labels the horizontal axis as σ_p in kPa, whereas the text and other figures use MPa-scale deviatoric stress. This should be checked and corrected.

4. **Stage labels should be unified.**
   Pore-volume evolution uses Stages I–IV, whereas resistivity evolution uses Stages A–D. The correspondence, for example Stage A $\leftrightarrow$ Stage I and Stage B $\leftrightarrow$ Stage II, should be explicitly stated. The quantitative criteria for the boundaries are treated as a major issue above, but the notation itself should also be made consistent.

5. **The experimental section should report loading rate and data acquisition rate.**
   Strain rate or stress loading rate affects drained conditions, pore-pressure equilibration, crack growth,

and electrical measurements. The sampling interval of resistance, strain, and pump-volume data should also be provided.

6. **Specimen variability should also be reflected in the figure and table presentation.** In addition to clarifying the number of specimens tested, the authors should state whether Figs. 4–7 show single-specimen curves, representative curves, or averaged curves. If repeated tests are available, tables and figures should include variability or uncertainty estimates.

7. **Fluid chemistry and pore-water resistivity are missing.** Because the model uses ρ_w, the manuscript should report brine concentration, measured ρ_w at 20°C, possible temperature correction, and whether ρ_w changed during the experiment.

8. **Several equations need typographical and notational correction.** Eqs. (26) and (27) use θ_u and φ in a way that may indicate a symbol mismatch. Eq. (39) is difficult to read, and should be rewritten with all variables defined immediately below it. Eq. (14) also needs clearer presentation, as described in Minor comment 12.

9. **Figure captions should specify whether curves are raw data, smoothed data, or fitted curves.** Figs. 4–7 show smooth curves and symbols, but the manuscript does not state whether the data were filtered or interpolated. This is related to the interpretation of the observed maxima and stage boundaries.

10. **Data and code availability should be strengthened.** The manuscript should provide the raw stress-strain, pore-volume, and resistivity data, together with the scripts or equations used to calculate Tables 1–2 and Fig. 7. This would allow readers to reproduce the parameter estimation and evaluate uncertainty.

11. **The meaning of the resistance components in Eq. (7) should be clarified relative to Fig. 2.** Fig. 2 may be read as if the rock specimen is divided into spatially separated subdomains, and R_e, R_h, and R_v are the resistances of those separated parts. If read in that way, each term requires explicit area fractions and geometrical factors. However, Eqs. (8) and (9) seem to require a different interpretation. That is, the three components share the same representative domain and contribute additively to the effective conductivity. Under this interpretation, each R_i is a hypothetical resistance component obtained when only pore class i is considered in the whole specimen-scale domain, while the other pore classes are masked or ignored. The manuscript should state this explicitly. For example, it may write: "R_e, R_h, and R_v denote independent resistance components evaluated by considering each pore class separately in the same specimen-scale domain, rather than resistances of geometrically separated subdomains." This clarification would keep the intuitive value of Fig. 2 while avoiding a misleading circuit interpretation.

12. **Eq. (14) should first present the general Hooke-law relation, and the restricted triaxial form should be derived only after Eq. (15).** The manuscript introduces Eq. (14) as the generalized Hooke's law for undamaged rock microelements under triaxial loading. However, the current equation seems to be already restricted to the conventional triaxial-compression setting and to the strain component related to deviatoric loading:

$$\sigma_1' = E\varepsilon_1' + \sigma_3',$$

or equivalently,

$$\sigma_1' - \sigma_3' = E\varepsilon_1'.$$

This restricted relation may be acceptable as a limited form for the later derivation of Eq. (16), if ε_1' is explicitly defined as the axial strain component caused by deviatoric stress. The problem is the order and clarity of the derivation. Since Eq. (14) is presented as the starting point in the method, the general isotropic linear-elastic relation should first be shown. For example,

$$\varepsilon_1' = \frac{1}{E}\left[\sigma_1' - \mu(\sigma_2' + \sigma_3')\right],$$

or equivalently,

$$\sigma_1' = E\varepsilon_1' + \mu(\sigma_2' + \sigma_3').$$

The restricted relation used later should be obtained after introducing the triaxial-loading condition, the no-horizontal-damage assumption in Eq. (15), and the effective-stress relation in Eq. (13). In the current presentation, Eq. (14) seems to incorporate the restrictions of Eq. (15) before Eq. (15). This also makes the definition of Poisson's ratio μ immediately after Eq. (14) look unnatural, because μ does not appear in the displayed equation. The authors should reorganize this part of the derivation so that non-specialist readers can distinguish the general Hooke-law statement and the later triaxial specialization.

13. **Eq. (21) should use a proper dummy integration variable and explicitly connect the cumulative Weibull probability to the damage definition in Eq. (11).**
Eq. (11) defines the damage variable as the ratio of the number of failed microelements to the total number of microelements:

$$D = \frac{N_t}{N}.$$

Eq. (17) then introduces $P(F)$ as the probability density function of microelement strength. Later the manuscript writes Eq. (21) in a form equivalent to

$$D = \int_0^F P(F)\,dx.$$

This notation is mathematically confusing. If the integration variable is x, the integrand should be $P(x)$. If the strength-threshold variable is denoted by f, it should instead be written as

$$D(F) = \int_0^F P(f)\,df = 1 - \exp\left[-\left(\frac{F}{F_0}\right)^n\right].$$

In a clearer notation, the random strength threshold and the stress-dependent driving value are distinguished. For example, let F_s be the random strength threshold of a microelement, and let $F^* = F(\boldsymbol{\sigma}')$ be the value of the Mohr-Coulomb-type failure-driving measure at the current stress state. Then Eq. (21) can be written as

$$D = \Pr\left(F_s \le F^*\right) = \int_0^{F^*} P(F_s)\,dF_s.$$

The phrase "From its definition" immediately before Eq. (21) is also too abrupt. Eq. (11) defines D as a number ratio, whereas Eq. (21) expresses D as a cumulative probability. The text should add a short explanation connecting these two expressions. One possible explanation is as follows: if $P(F_s)$ is the probability density function of microelement strength thresholds,

$$\int_0^{F^*} P(F_s)\,dF_s$$

represents the fraction of microelements whose strength thresholds are less than or equal to the current value F^*. Under the statistical-damage assumption that microelements fail in order from low strength thresholds to high strength thresholds as F^* increases, this cumulative fraction is equal to the fraction of destroyed microelements N_t/N. This statement makes the transition from Eq. (11) to Eq. (21) understandable for readers who are not specialists in statistical damage theory.

14. **The physical meaning of the $F < 0$ branch in Eq. (22) should be clarified.**
    Eq. (17) introduces F as a Weibull-distributed microelement strength. In this role, F is a non-negative strength threshold. However, Eq. (18) uses the same symbol for a stress-dependent Mohr-Coulomb-type quantity:

    $$F(\boldsymbol{\sigma}') = \sigma_1' - \sigma_3' - (\sigma_1' + \sigma_3')\sin\theta.$$

    This stress-dependent quantity can be negative under low deviatoric stress or nearly hydrostatic compression. Such a case should not be interpreted as negative microelement strength. Rather, it means that the present failure-driving measure has not reached the non-negative strength-threshold domain where damage begins. The authors should explicitly define the damage variable as a piecewise function, for example:

    $$D(F^*) = \begin{cases} 1 - \exp\left[-\left(\dfrac{F^*}{F_0}\right)^n\right], & F^* \geq 0, \\ 0, & F^* < 0. \end{cases}$$

    This clarification avoids confusion between the random strength threshold F_s and the stress-dependent driving value F^*, and makes the case division in Eq. (22) physically transparent.

15. **The algebraic transition from Eq. (27) to Eq. (28) should be made traceable.**
    The derivation from Eq. (27) to Eq. (28) is difficult to trace in the present form. This seems to be partly caused by inconsistency in the definition of the expansion coefficient u. In Eq. (27), the coefficient multiplying $d\varepsilon_1$ contains

    $$\tan^{-2}\left(\frac{\pi}{4} + \frac{\theta_u}{2}\right) = \frac{1}{\tan^2\left(\frac{\pi}{4} + \frac{\theta_u}{2}\right)},$$

    but the following text seems to define u using

    $$\tan^2\left(\frac{\pi}{4} + \frac{\theta_u}{2}\right).$$

    For Eq. (28) to follow from Eq. (27), u should probably be defined as

    $$u = 1 - \frac{\sigma_1}{\sigma_3}\tan^{-2}\left(\frac{\pi}{4} + \frac{\theta_u}{2}\right).$$

    Furthermore, the derivation of Eq. (28) seems to use the incremental relation between axial effective stress and axial strain,

    $$d\sigma_1' = E\,d\varepsilon_1,$$

    or equivalently,

    $$d\varepsilon_1 = \frac{1}{E}d\sigma_1'.$$

    However, this substitution is not explicitly stated. Adding this intermediate step, together with a corrected and consistent definition of u, would greatly improve the traceability of the derivation from Eq. (27) to Eq. (28). The notation for the sliding friction angle should also be unified, because θ_u and φ_u appear to be used for the same quantity in this part of the derivation.

16. **The incomplete gamma functions in Eqs. (31)–(33) should be explicitly defined.**
    In Eqs. (31), (32), and (33), the quantities A and B are described as gamma functions. However, a two-variable incomplete gamma function must be distinguished by its integration interval. The lower incomplete gamma function, namely the first incomplete gamma function, is defined on $[0, x]$. In contrast, the upper incomplete gamma function, namely the second incomplete gamma function, is defined on $[x, \infty)$:

$$\gamma(s,x) = \int_0^x t^{s-1}e^{-t}\,dt,$$

$$\Gamma(s,x) = \int_x^\infty t^{s-1}e^{-t}\,dt.$$

In the present derivation, the relevant operation is finite-range integration over microelement failure strengths up to the current failure-driving value. Therefore, the intended function seems to be the lower, that is, the first incomplete gamma function. This can be confirmed by expanding the transformation from Eq. (30) to Eq. (31). However, this point is not immediately clear from the current notation. In particular, the manuscript writes $\Gamma(s,x)$, which is often used for the upper incomplete gamma function. The authors should explicitly state whether A and B mean the first or second incomplete gamma function. Alternatively, they should provide the defining integral of the "gamma functions" used in Eqs. (32) and (33). This short clarification would greatly improve readability in the closed-form expression of Eq. (31) and prevent ambiguity.

17. **Eqs. (34) and (35) appear to omit the initial-compression correction term used in Eqs. (36) and (37).**
   Eqs. (36) and (37) include the corrected peak-strain term

   $$E\left(\varepsilon_{1p} - \frac{\gamma\varphi_0}{3}\right) + \sigma_3 - P_w.$$

   However, Eqs. (34) and (35) use

   $$E\varepsilon_{1p} + \sigma_3 - P_w$$

   or

   $$E\varepsilon_1 + \sigma_3 - P_w,$$

   and do not seem to include the initial-compression correction term $\gamma\varphi_0/3$. Because the manuscript explicitly states that Eq. (34) is obtained when $\varepsilon_1 = \varepsilon_{1p}$, ε_{1p} seems to represent the peak axial strain itself, not a corrected strain variable. Therefore, if Eqs. (36) and (37) are derived from Eqs. (34) and (35), the same correction term should also appear in Eqs. (34) and (35). The authors should check these equations and make the notation consistent.

18. **Corrections applied to pore-volume measurements should be clarified.**
   The manuscript states that, because the sample is saturated, the change in seepage pump volume was used to represent pore-volume change. This is reasonable as a first-order measurement principle. However, the authors should clarify whether corrections were applied for system compliance, fluid compressibility, tubing and piston deformation, jacket deformation, and possible drainage transients during rapid failure. This point is especially important because the pore-volume change curves are used for interpretation of model parameters and stage divisions. If these effects were calibrated and confirmed to be negligible, a short statement would be enough.

19. **The definition and interpretation of ϕ_0 should be clarified.**
   The limestone porosity is reported as 0.75%, but Table 1 shows $\phi_0 = 0.66\%, 0.60\%, 0.56\%, 0.52\%$ for the four confining pressures. If ϕ_0 is defined as the initial porosity when the rock is not under stress, it should not systematically vary with confining pressure, except when different specimens have different initial porosities or when this parameter is actually effective porosity after hydrostatic consolidation. The authors should distinguish independently measured porosity from fitted or condition-dependent effective porosity.

20. **Uncertainty should be reported for the fitted cementation indices in Table 2.**
   The manuscript states that m_e remains 1.73 under all test conditions, indicating that confining pressure has little influence on conductive connectivity of pores. However, no uncertainty, confidence interval, or fitting error is reported for m_e, m_h, or m_v. The authors should report fitting uncertainties, or at least residual metrics for parameter estimation. Without these, it is difficult to evaluate whether the observed changes in m_h and m_v, or the constancy of m_e, are statistically meaningful.

21. **The offset between the resistivity maximum and pore-volume minimum should be quantified.**
    The observation that the resistivity maximum c' appears before the pore-volume minimum c is physically important, because it supports the interpretation that connectivity changes affect resistivity before the maximum pore-volume change. However, this offset is discussed only qualitatively. The authors should quantify the offset for each confining pressure. For example, they should report $\sigma(c) - \sigma(c')$, the corresponding normalized stress difference, or the stress ratios $\sigma(c')/\sigma_p$ and $\sigma(c)/\sigma_p$.

22. **Model-based interpretations of fitted cementation indices should be stated cautiously.**
    The discussion interprets the increase in m_h and m_v with confining pressure by fracture compression, reduced connectivity, and fracture inclination. This interpretation is plausible, but these parameters are obtained from fitting, and uncertainty estimates are not provided. The authors should either support these interpretations by independent observations, or present them more cautiously as model-based interpretations.