Reviewer Report

The manuscript investigates several viscous–plastic (VP) sea-ice rheologies constructed around a linear Mohr–Coulomb-type stress envelope and different flow-rule formulations. The authors compare existing formulations with two new variants based on teardrop and parabolic-lens potentials, focusing primarily on localization angles and numerical convergence.

There is potentially useful numerical work here. However, I do not think the manuscript is suitable for publication in its present form. There are interconnected problems with the theoretical framing, the positioning relative to established plasticity and localization theory, the interpretation of observational and numerical evidence, and the mathematical and computational implementation of the new formulations. Correcting these issues would materially alter both the interpretation and the claimed novelty of the paper, while some of the new numerical results would need to be recomputed.

1. Foundational mechanics

The introductory presentation of Mohr–Coulomb requires some qualification. For the isotropic two-dimensional criterion considered here, the linear form in terms of σ_I and σ_{II} is legitimate. However, $\mu = \sin\phi$ in this transformed relation is not the plane friction coefficient $\tan\phi$, and the intercept should not be identified directly with the plane cohesion without the corresponding transformation. This two-dimensional form should also not be read as a general invariant representation of Mohr–Coulomb. In three dimensions, Mohr–Coulomb retains Lode-angle dependence; extending the same linear relation to I_1 and $\sqrt{J_2}$ instead produces a Drucker–Prager-type surface.

The laboratory motivation is also stated too generally. The cited sea-ice experiments support Coulomb-like faulting in particular brittle regimes, but the observed failure mode changes outside those regimes. They therefore motivate a frictional description in a limited range of pressure and other conditions, rather than establishing a universal Mohr–Coulomb strength law for sea ice.

More seriously, the manuscript conflates normality with coaxiality. Coincidence of principal stress and strain-rate directions (coaxialty) is a statement about tensor orientations in physical space; normality concerns the direction of plastic flow relative to the gradient of a surface in stress space. The distinction is particularly clear in the present formulation: Eq. (7) makes the deviatoric stress tensor proportional to the deviatoric strain rate, and therefore coaxial, while several of the rheologies are deliberately non-normal. Coaxiality cannot therefore be used to infer associated flow, as the text suggest.

The description of the flow rule as linking the yield curve to "post-failure" deformation is also misleading. It conflates yielding with failure/localization and suggests a conventional yield-surface/plastic-potential structure that several of the proposed VP formulations do not actually possess.

This is particularly evident for MC-E. The manuscript itself states that the implemented MC-E formulation is not constructed from an independent Mohr–Coulomb yield function F and elliptical plastic potential G; rather, η is modified to obtain a Mohr–Coulomb-shaped stress envelope while ζ is inherited from the elliptical rheology. This may be a legitimate VP constitutive construction, but it should not subsequently be interpreted as though it were a conventional F–G plasticity model.

A related point is that the apparent-viscosity representation is more than a mere change of notation. The regularized VP relation replaces the singular/set-valued rigid-plastic limit by a single-valued high-viscosity approximation, and the details of this approximation can affect both convergence and localization (as studied in the literature, see my next point).

Finally, corners and vertices are treated mainly as numerical nuisances. But they are well-recognized constitutive features. Nonsmooth and multisurface plasticity provides standard ways of treating such states, while non-associativity and vertex structure can themselves affect localization.

2. Positioning relative to established localization and numerical literature

The localization discussion is framed mainly through the Coulomb, Roscoe and Arthur angle constructions, while the broader localization/bifurcation literature is largely absent from the interpretation. The effects of friction, dilatancy, non-associativity, constitutive tangent structure and vertices on shear-band formation have been studied extensively, including by Rudnicki and Rice, Vardoulakis, Vermeer, de Borst and co-workers. Particularly relevant is Lemiale, Mühlhaus, Moresi and Stafford (2008), already cited in the manuscript, which considers shear banding for plastic models formulated as incompressible viscous flows. Given that localization angle is the principal mechanical observable in this manuscript, this body of work should form part of the theoretical context rather than the problem being presented largely as a competition among three angle formulas.

There is also established literature on nonlinear solution of plasticity represented through effective/apparent viscosities. Studies including Spiegelman et al. (2016), Mehlmann and Richter (2017), Duretz et al. (2018), Rudi et al. (2020), and Shih et al. (2023) demonstrate the strong dependence of convergence on regularization and nonlinear solution strategy, including well-known limitations of Picard-type iterations. This is directly relevant to the interpretation of the convergence results presented here, and should be considered before directly relating the convegence issues to constitutive features.

The omission of this literature also affects the novelty claim. The dependence of localization orientation on friction, dilatancy/non-associativity and constitutive structure is established mechanics, and the authors' own earlier work has already demonstrated an influence of non-normal flow on sea-ice fracture angles. The new contribution here is therefore primarily the particular VP formulations and their numerical implementation.

3. Observations and previous numerical studies are given stronger constitutive interpretations than they support

The field observations alluded to in the text reveal an angle discrepancy, but they do not identify its constitutive cause. Attributing that discrepancy specifically to the elliptical yield curve (and then using it as evidence that a Mohr–Coulomb yield curve is better justified) is a huge leap in reasoning, and requires additional assumptions not established by the observations.

The interpretation of Hutter et al. (2022) illustrates this issue. Their comparison found excessive LKF intersection angles across models, but also showed that rheology, numerics, resolution and forcing all influence the simulated LKF properties. Those results do not isolate the elliptical yield curve as the cause of the angle discrepancy.

Similarly, the reconstruction in Ringeisen et al. (2023) does not uniquely identify a yield surface. The reconstructed object is explicitly interpreted as a possible yield curve or plastic potential, and its construction relies on assumptions about the relationship between the observed angle distribution and the corresponding curve. It is at best motivating possible constitutive forms rather than direct observational identification of a Mohr–Coulomb yield surface.

There is a related issue of independence. Friction parameters inferred from observed LKF angles using a Coulomb-type angle relationship cannot themselves serve as independent observational validation of that same relationship. The independent laboratory evidence should be distinguished from parameters inferred through the assumed Coulomb construction.

The historical discussion of earlier MC-type sea-ice models suffers from the same general issue: outcomes of particular constitutive and numerical implementations are repeatedly generalized to Mohr–Coulomb mechanics as a constitutive class.

4. Interpretation of the numerical results

Figures 5–7 demonstrate that changing the implemented VP formulation changes the orientation of the localized deformation features. The strongest conclusion supported by these calculations is therefore that the localization angle is sensitive to the complete VP constitutive mapping.

For MC-E, for example, changing the ellipse parameter e changes Δ, ζ, η, and consequently the complete rate-to-stress relation. Figure 6 therefore demonstrates sensitivity to the MC-E formulation as a whole; it does not isolate the effect of an independently specified plastic potential in the conventional sense.

The proximity of some calculated angles to the Arthur angle is interesting, but it remains an empirical observation. No localization or bifurcation analysis is presented that identifies the Arthur construction as the governing mechanism. The further speculation that the Arthur angle might represent the most efficient dissipation is not supported by the analysis. Under the usual assumptions of classical convex plasticity, maximum plastic dissipation is associated with normal flow; it does not by itself provide a mechanism for selecting an intermediate Arthur angle.

The interpretation of Fig. 8 should also be more cautious. Residual stagnation of the chosen Picard-based solution procedure demonstrates numerical difficulty but does not establish that the governing problem is ill-posed. The numerical literature cited above provides several alternative explanations related to nonsmooth apparent-viscosity plasticity, regularization and nonlinear linearization. Actual ill-posedness is a separate mathematical question.

Likewise, the larger spread of measured angles for MC-TD and MC-PL is not itself a measure of convergence. The absence of a clear band in two particular calculations also does not establish the more general statement that both Coulomb and Roscoe angles must be defined for localization to occur.

Given that localization in regularized VP formulations can depend on discretization and regularization, some mesh/regularization sensitivity of the reported angles would also be important.

Overall, the numerical experiments support substantially narrower conclusions than statements such as obtaining "realistic failure patterns" or fixing a persistent problem of VP models.

5. Derivations and implementation

The appendices contain a number of algebraic and conceptual inconsistencies. Individually some may be straightforward to correct, but their number is concerning given that the constitutive derivations are central to the manuscript.

In Appendix A:

- The opening description gives $\sigma_I = 0$ in divergence, whereas the equations give the tensile state $\sigma_I = T = k_t P$.
- Equation (A4) has the wrong sign for the pressure term relative to Eq. (7).
- Equation (A5) contains an erroneous factor of two.
- Equation (A10) has the wrong sign in the shifted σ_I term if it is to produce Eqs. (A12)–(A14).
- Equation (A10) also contains $\dot{\epsilon}_{II}$ itself. It is therefore not a conventional independently prescribed stress-space plastic potential, but a rate-dependent construction designed to reproduce the desired constitutive response.
- The statement that the tip states cannot be represented using a plastic potential is too general; nonsmooth/multisurface formulations can represent vertices.

In Appendix B:

- Equations (B7) and (B9) are mutually inconsistent and contain a missing factor of four. Eqs. (B12)–(B13), in contrast, are consistent with the corrected expression for λ.
- Equation (B11) contains another factor-of-two error in the relation for σ_{II}.
- The text gives the wrong sign for the centre of the elliptical potential.
- The sign of Δ_c in Eq. (B15) is reversed; as printed, the proposed cap can produce negative viscosities in compression.

The interpretation of $\lambda < 0$ also requires care. In conventional plasticity, a negative plastic multiplier normally signals that the assumed active plastic branch is inadmissible and invokes the corresponding loading/unloading or complementarity conditions, rather than simply defining a physical negative-viscosity branch.

There is also a sign error in Eq. (36): substitution of the manuscript's own definitions gives a $+Pk_t$ contribution, whereas the printed equation contains $-Pk_t$. The principal TD/PL implementation uses the positive sign.

I also examined the supplied MITgcm implementation. Two issues directly affect the new formulations.

For MC-TD, the implemented compression cap contains the wrong pressure coefficient. With $c_y = (2 - k_t)/3$, the stated cap requires the coefficient $(1 - c_y)$, whereas the code uses $(1 - 2c_y)$. For the parameters used in the manuscript this can make the cap shear viscosity negative, and the subsequent `MIN` operation can select this branch. This directly compromises the interpretation of the MC-TD convergence results and should be corrected before those calculations are relied upon.

The MC-PL implementation contains a separate serious issue: the variable `cyc` is used in the compression-cap calculation without being assigned in the MC-PL branch. In addition, with the supplied default pressure-replacement factor active, the final shear relation follows the parabolic-lens envelope and loses the explicit dependence on μ (it cancels out!), rather than retaining the intended linear Mohr–Coulomb stress envelope. These issues need to be resolved before the MC-PL results can be considered reliable.

There is also a discrepancy in the MC-PS regularization. The manuscript describes a smooth square-root regularization with a transition around $10^{-9}\,\mathrm{s}^{-1}$, whereas the supplied configuration leaves the corresponding smooth-regularization option disabled and uses the hard-maximum form, with the default threshold of $10^{-10}\,\mathrm{s}^{-1}$ unless overridden. Given that numerical convergence is one of the principal comparisons in the paper, this difference is relevant.

6. Scope and novelty

After removing the broader mechanical interpretations, I see the main defensible contribution as a comparison of several particular VP implementations incorporating a linear Mohr–Coulomb-type stress envelope in an idealized compression problem, showing that changes in the complete constitutive/apparent-viscosity mapping alter both localization orientation and nonlinear-solver behaviour.

That can be a useful numerical contribution. However, the underlying influence of friction, dilatancy/non-associativity, constitutive structure and vertices on localization is established mechanics, and the influence of the flow rule on sea-ice localization angles has already been demonstrated in previous work. The principal novelty therefore resides in the particular new VP formulations and their implementation. These are precisely the parts of the study for which the derivations and supplied code raise the most serious concerns.

More generally, the manuscript would benefit from a clearer choice of scope. If the intention is primarily to present and compare particular VP sea-ice formulations, the broader claims about plasticity, failure and localization should be substantially restrained. If the authors wish instead to draw more general conclusions about the underlying mechanics, those conclusions need to be developed with a some level of attention to the established literature on plasticity, bifurcation and localization.

Additional technical comments

- The terminology of yielding, failure, fracture, localization and "post-failure" deformation should be used more carefully; these are not interchangeable concepts.
- The statement that failure can occur only when the slopes of the yield curve and plastic potential lie within $[-1, 1]$ should be restricted to the particular construction used here. It is not a general localization condition.
- The pointed TD and PL potentials require a proper nonsmooth treatment or controlled regularization. Moving the operating point away from the tips by increasing k_t, or hard-clipping the constitutive expressions near the tips, does not resolve the underlying constitutive issue.
- The units and value of $P^\star$ in Eq. (12) should be checked. In the stated strength relation $P^\star$ is stress-like; the conventional Hibler value is approximately $27.5\,\mathrm{kN\,m^{-2}}$, not $27.5\,\mathrm{N\,m^{-1}}$.

Relevant additional references

The following are examples of studies directly relevant to the issues raised above that are not presently included in the manuscript's discussions.

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