Dear Editors,

Thank you for the opportunity to revise our manuscript, "Models of buoyancy-driven dykes using continuum plasticity and fracture mechanics: a comparison". We appreciate the constructive feedback from you and the reviewer. We have now addressed all the remaining points and believe the manuscript is significantly improved in its clarity and transparency.

In summary, we have undertaken the following revisions as agreed:

- **Reviewer Comments**: All new specific comments from the reviewer have been addressed, with clarifications incorporated directly into the manuscript.
- **New Compaction Formulation**: We have improved the explanation of our new compaction formulation in Section 2.2.1 by clarifying the role of the Picard iteration scheme in handling nonlinearities and have enhanced the cross-reference to our previous work for full details.
- **Plasticity Model Limitations**: We have added a new paragraph to the Discussion section acknowledging the limitations of the employed plasticity model, placing our work in the context of this active area of research.
- **Numerical Implementation**: We have added succinct information about our numerical framework to the introduction and Section 2.2.4 to make the manuscript more self-contained, with clear signposting to Li et al. (2023) for details.

We trust that these revisions address your concerns and have strengthened the manuscript. We look forward to your decision.

Sincerely,

Yuan Li & Co-authors

Manuscript "Models of buoyancy-driven dykes using continuum plasticity and fracture mechanics: a comparison" – 2nd Revision

1 Review's comments

The revised manuscript shows several improvements and thoughtful clarifications in response to reviewer comments. I appreciate the authors' efforts and recognize their intention to sharpen the focus of the work. However, several substantial concerns from the first review round remain either unaddressed in the manuscript itself or insufficiently justified. Many of the explanations are offered only in the rebuttal, while key modeling assumptions, derivations, and terminology still lack clarity or supporting evidence in the main text. To meet the standards of transparency and reproducibility, these elements must be incorporated directly into the manuscript.

R: We thank the reviewer for their constructive feedback. In response to their specific comments, we have revised the manuscript to directly incorporate the necessary explanations of our modeling assumptions, terminology, and methods. We agree that these changes significantly improve the manuscript's clarity and transparency.

1.1 Specific comments

- Clarification of scope: The authors themselves note that "many of the other comments appear to stem from a misunderstanding of the scope of this manuscript," and indicate that both reviewers required clarification on this point. This strongly suggests that the scope is not sufficiently defined in the manuscript itself. The clarification provided in the rebuttal should be incorporated into the revised text—preferably early in the introduction—to prevent similar confusion for future readers.

  R: We thank the reviewer for this constructive suggestion. We have revised the first paragraph of introduction to clarify that the central goal of this manuscript is to rigorously benchmark our continuum model against linear elastic fracture mechanics (LEFM) as a critical validation step.

- Line 12 – Formation of dykes: A previous comment on Line 12 remains inadequately addressed. The sentence "Magmatic dykes, formed by fluid-driven fracture, are an important pathway for magma ascent across the lithosphere" still implies a singular mechanism, even after the minor rephrasing. To ensure scientific accuracy and neutrality, I recommend splitting the sentence: "Dykes are an important mechanism for magma ascent. They can be formed, among other mechanisms, by fluid-driven fracture." This makes the distinction between the general importance of dykes and the specific process of their formation more precise, without requiring an exhaustive review of alternative mechanisms.

R: Changed.

- Anisotropic permeability and dyke direction: The treatment of anisotropic permeability requires further clarification and revision. In the rebuttal, the authors explain that anisotropy was introduced to limit lateral leakage, not to drive the flow. However, this important clarification is absent from the manuscript itself. Moreover, the influence of permeability anisotropy on dyke orientation is a nontrivial issue. Unless supported by a parametric study (e.g., varying anisotropy and boundary conditions), the manuscript should avoid making deterministic claims about dyke direction. The comment that anisotropic permeability in sedimentary rocks was not modeled also raises further questions: How many distinct permeabilities are present in the model? And how is anisotropy imposed? The sentence "Therefore, to limit leakage through the walls, we introduce an anisotropic permeability" requires clearer explanation and consistency with the modeling assumptions. References such as Rozhko et al. (2007), which demonstrate the role of external conditions in dyke orientation, should also be considered.

  R: We thank the reviewer for this helpful comment and agree that our rationale for using anisotropic permeability required clarification.

  The anisotropic permeability was introduced for a specific methodological purpose: to ensure the simulated dyke maintained a constant width, enabling a direct comparison with the one-dimensional poro-LEFM model. Its function was to prevent artificial widening, not to model the geological controls on dyke trajectory, a topic that falls outside the benchmarking scope of this paper.

  We have revised Section 2.2.2 to state this rationale explicitly, and we hope this clarifies our approach.

- Prescribed initial porosity and dyke emergence: The rebuttal asserts that the dyke is not prescribed but emerges from the PDEs given a specific initial porosity distribution. However, because this distribution is carefully shaped (anisotropic Gaussian) and placed at the base, it still strongly preconditions dyke formation. This explanation must be included in the manuscript. Better yet, presenting a comparison with a case using isotropic or random porosity would provide more convincing evidence for the claim of emergent behavior.

  R: We have revised the manuscript to clarify the role of the initial condition. The new text in Section 2.2.4 now explains that while the initial porosity field is intentionally designed to precondition the dyke's location and orientation such that it resembles the poro-LEFM model in Fig 1, the dyke itself — as a result of plastic failure — is an emergent feature that results dynamically from the governing equations.

- Mode-I vs. mode-II fracture and failure criterion (Line 296–299): The authors acknowledge that isolating mode-I fracture is not possible with their yield criterion, which incorporates both shear and compaction stresses. This important caveat is only included in the rebuttal and should be explained in the manuscript. This needs to be explained in the manuscript, not only in the response to reviewers.

  R: We thank the reviewer for this important point. As suggested, we have added a sentence to the manuscript (Section 3.1) to clarify that our yield criterion incorporates both shear and tensile failure and therefore does not isolate for pure Mode-I fracture.

- Terminology – Overpressure and dilatancy pressure (Line 526): The use of terms like ”overpressure” and ”dilatancy pressure” is potentially confusing. “Overpressure” typically refers to fluid pressure exceeding hydrostatic pressure, not to a generic pressure difference. If the authors choose to use this term, they must define it explicitly upon first use. A clearer solution would be to consistently refer to ΔP as “pressure difference” throughout the manuscript. Likewise, the term ”dilatancy pressure” should be introduced more carefully to distinguish it from similar terms in existing literature.

  R: We thank the reviewer for highlighting this potential for confusion. As suggested, we have revised the manuscript to explicitly define ”overpressure” upon its first use and have been more careful in our introduction of the term ”dilatancy pressure” to clarify its specific role within our formulation.

- Constitutive modeling – Equations (D7) and (D8): The derivation of Equations (D7) and (D8) from the general viscoplastic framework (Equation C7) is still unclear. While the rebuttal provides some helpful interpretation, these explanations must be moved into the manuscript for the benefit of all readers. Clarifying these equations would significantly enhance the rigor of Appendix D.

  R: We thank the reviewer for highlighting this lack of clarity. We have revised Appendix D2 to better explain the decomposition of the viscoplastic work rate. The updated text now clarifies two key aspects: firstly, it explicitly states that the purpose of this subsection is to justify our choice of a small η^{K}. Secondly, it clarifies the origin of Equation (D7), explaining that it is a definitional decomposition and not derived from Equation (C7). We hope this revised explanation makes our justification clearer.