Manuscript "Models of buoyancy-driven dykes using continuum plasticity and fracture mechanics: a comparison" – Response to RC2

1 General comments

In general, I believe that this work is really interesting and should be definitely published after a moderate revision. Below I summarize a few discussion topics that should be addressed in the manuscript.

R: We thank the reviewer for this positive feedback. Below, we address each specific comment in turn, with our responses indicated by **R:**.

Before addressing the comments, we clarify that this manuscript has one specific aim: to benchmark the poro-VEVP model, published in Li et al. (2023), against the classical LEFM model. This poro-VEVP model (arguably) showed an approach to simulate dyking in a continuum model. However, the analysis by Li et al (2023) did not convincingly resolve all issues for this purpose. The present manuscript shows that this poro-VEVP model does achieve a substantial similarity to the dyke in the classical LEFM model.

2 Specific comments

- The results of poro-VEVP models presented in Section 3.1 demonstrate that dyke is essentially locked to one grid cell across the width. The text only discusses the mesh dependence issues in the context of boundary conditions. However I believe that it's not the only factor that is important in this context. What do you think the intrinsic length scale for the dyke width in poro-VEVP formulation should depend on? What happens if a theoretical dyke width is infeasible to resolve in the numerical model, e.g. in 3D? How would you parametrize such a case? A series of models with varying lateral resolution would provide a valuable insight in this issue.

  R: We have added Appendix E, including a new plot (Figure E1), to illustrate the effect of lateral resolution. This figure shows different results for mesh-dependency tests with either a fixed flux or a fixed flow rate. This difference arises because the dyke width cannot be smaller than one grid cell; thus, the same flow rate leads to different fluxes when varying the lateral resolution. Even with a fixed flux, the plastic dissipation rate is also affected by the lateral resolution.

  However, the propagation speed can be made independent of the cell size by prescribing a fixed flux. This regularisation might originate from Darcy's law, but it is unclear whether this introduces an intrinsic length or time scale. Further studies are certainly required.

Regarding simulating a dyke in 3D, we are unsure how to parameterise such a case to reduce computational cost.

- Slow propagation speed in poroVEVP models was mainly attributed to a relatively large Darcy drag in the fracture. I think this conclusion is generally correct. It was also correctly pointed out that propagation speed can be increased by larger permeability enchantment parameter. The question is what values should be actually used? Perhaps the power-law porosity-permeability dependence is just wrong for the dyke case, or the power-law exponent should be larger. Here is my constructive suggestion: we can probably somehow estimate the effective permeability in the dyke such that Darcy drag would be made equal to the viscous drag of Poiseuille flow under the same fluid pressure gradient.

  R: We have shown the effective permeability at which the Darcy drag equals the viscous drag of Poiseuille flow, albeit indirectly, in Figure 2a. This figure demonstrates that the speed of a poro-LEFM model approaches the speed of an LEFM model as $\phi \to 1$, when the relationship between dyke width, h_0, and mobility prefactor, M_0, satisfies $h_0 = (12M_0\mu)^{1/2}$. This relationship is derived using the cubic power-law porosity-permeability relation. Assuming a constant-width poro-VEVP dyke, this relationship can be used to estimate the effective permeability in that model.

- Despite that reliable analysis of mechanical energy dissipation around the dyke tip was performed, it is still not guaranteed that dyke propagation speed in poro-VEVP models is completely independent of the mesh resolution. It would be again insightful to run a series of models with different resolution to investigate how this affects the propagation speed.

  R: We added Appendix E and Figure E1 to show that the dyke propagation speed is independent of the mesh resolution.

- The viscous rheology currently used in the poro-VEVP models is only porosity-dependent. Would it take a significant effort to incorporate a stress-dependent viscous rheology e.g. power law? For example a complete redesign of the iterative point-wise stress computation algorithm would be necessary in this case.

  R: While this is beyond the scope of this manuscript, we offer a brief comment. The poro-VEVP model incorporates two viscous components: the Maxwell and Kelvin viscous terms. We assume this comment refers to the Maxwell viscous term, as there is currently no physical justification for employing such a complex rheology for the Kelvin viscous term. Incorporating a stress-dependence into the Maxwell viscous component is straightforward and does not require any modification of the point-wise stress computation algorithm. In the momentum equation, this is implemented as a non-Newtonian viscosity dependent on strain rates. During the point-wise stress computation, both the strain rates and viscosity are treated as constants. Therefore, the same algorithm remains applicable.

- The smooth plasticity model used in this work raises a few concerns. First of all, hyperbolic approximation for the Mohr-Coulomb yield surface in the meridional plane was initially introduced by Abbo and Sloan (1995), two years earlier than in Carol et al. (1997). Please acknowledge that fact. Second, from the definition of the flow potential it follows that this model does not handle the non-associative case with dilatation angle approaching zero.

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  To summarize, it is quite clear that plasticity model should assume uncoupled description of the shear and tensile failure modes which is not the case for the hyperbolic approximation. Even the original ad hoc model of Carol et al. (1997) is not a good candidate since it is not smooth and not convex (see the first plot). Please discuss this rather important issue in the text.

  R: We greatly appreciate this detailed examination of the plasticity model used in Li et al. (2023) and this manuscript.

  We have added citations to the original sources for the hyperbolic yield surface concept where it is first mentioned on page 2: "(e.g., Abbo and Sloan, 1995; Carol et al., 1997)".

  We agree that the plastic potential function in the original Carol model can be non-smooth and non-convex for certain parameter choices. We thank the reviewer for pointing this out. However, this also depends on the choice of function for the dilatancy coefficient. Our poro-VEVP model uses a different function than the Carol model, so under these conditions, this issue may not impair the model's capabilities. We will address this concern further below, demonstrating that it is not a problem in our model.

  This comment also questioned why we chose the dilatancy coefficient to depend on porosity only, as

$$c_{dl} = \exp\{(-\phi_c/\phi)\}.$$

  We are working with a two-phase model that imposes incompressibility of the individual phases. An essential assumption in this context is that dilatancy can only occur where mobile fluid is present. Solid grains in regions without fluid thus cannot undergo dilatancy. A naive approach to incorporate this assumption is to prescribe a porosity-dependent dilatancy coefficient. The simplest approach is to allow full dilatancy ($c_{dl} = 1$) if sufficient fluid presents, and no dilatancy ($c_{dl} = 0$) otherwise. Computationally, a function to facilitate a smooth transition for c_{dl} is desirable. Therefore we choose this exponential function where ϕ_c is a parameter controlling the scale of ϕ over which the transition of c_{dl} occurs.

This comment notes that the choice of ϕ_c affects c_{dl}, with examples of $\phi_c = 10^{-1}$, 10^{-2}, and 10^{-3}. While this parameter can indeed impact c_{dl} and thus affect the numerical results, we mitigate this impact by choosing a very small value, $\phi_c = 10^{-6}$. With this choice, $c_{dl} = 0.9$ and 0.98 when $\phi = 10^{-5}$ and 10^{-4}, respectively. Therefore, the transition of c_{dl} occurs in the region with porosity three to four orders of magnitude smaller than the porosity in the main region inside of the dyke. Thus, $c_{dl} \approx 1$ in almost all regions with sufficiently large porosity. In other regions with very small porosity, the value c_{dl} does not affect the results in this manuscript because plastic failure does not occur due to the high plastic effective compressive stresses in nearly zero-porosity regions. Because c_{dl} is effectively constant in the regions of interest, our plastic potential function remains smooth and convex in the relevant parameter range.

However, we agree that this plasticity model could be improved to better capture both shear and tensile failure modes within a single framework. In this plasticity model, dilatancy could be related to stresses or shear strain rates, potentially relevant to shear failure in shallow regions (where opening pore space is readily accommodated by inflow of water and/or air, or even density change of air). We intend to explore this possibility in future work.

To further clarify the choice of porosity-dependent dilatancy, we have added the following text to Appendix C: "Note that we choose c_{dl} to depend on porosity. This choice contrasts with the stress-dependent formulation used in [**?**], which studies cracks in an engineering context. In our model, $c_{dl} \approx 1$ everywhere the porosity is not vanishingly small, and $c_{dl} \approx 0$ in non-porous regions. The exponential function is chosen to provide a smooth transition between these two states. "