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Interactive comment on "The effect of effective rock viscosity on 2D magmatic porosity waves" *by* Janik Dohmen et al.

Viktoriya Yarushina (Referee)

viktoriya.yarushina@ife.no

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Bulk and shear viscosities of the porous rocks are dynamic quantities that change in response to pressure, temperature and porosity variations. Various laws for porosity dependence of the bulk viscosity on porosity were proposed in the literature. Some of them differ significantly from each other and sometimes even show opposite trends [Yarushina and Podladchikov, 2015] (Fig. 1). On the other hand, previous research showed that porosity waves change their properties significantly depending on the rock rheology and dependence of bulk viscosity on effective pressure [e.g., Connolly and Podladchikov, 2007; Yarushina et al., 2015]. Thus, it is very important to explore how various expressions for porosity dependence of effective viscosity influence propagation of porosity waves. Manuscript of Dohmen et al. addresses this issue. The authors

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Discussion paper

investigate how porosity waves change their properties based on the effective viscosity relation previously proposed by the authors [Schmeling et al., 2012]. They compare results of 2D simulations with previously published 1D results for different types of the bulk viscosity with a conclusion that exact choice of porosity dependence of bulk and shear viscosities play a minor role on the speed of porosity wave propagation for the cases considered. The manuscript presents new concepts with interesting conclusions and, thus, deserves the publication in the journal.

Some points for improvement:

- References in the manuscript do not sufficiently cover recent work and sometimes misleading. For example, lines 46-51 state "Within supersolidus source regions at low melt fractions melt is assumed to slowly percolate by two-phase porous flow within a deforming matrix (McKenzie, 1984; Schmeling, 2000; Bercovici et al., 2001), followed by melt accumulation within rising high porosity waves (Scott and Stevenson, 1984; Spiegelman, 1993, Wiggins and Spiegelman, 1995; Richard et al., 2012) or focusing into channels (Stevenson, 1989; Richardson, 1998) which have the potential to penetrate into subsolidus regions above to generate dykes."

However, the work of Scott and Stevenson [1984] does not present focusing of the melt into channels. It discusses linear stability analysis, which gives conditions at which flow instability may arise. Subsequently, it was shown by various authors that this instability may result in different 3D shapes including spherical blobs and sills [e.g. Wiggins and Spiegelman, 1995]. On the other hand, formation of 3D channels due to two-phase porous flow within a deforming matrix were demonstrated in [Omlin et al., 2018; Räss et al., 2014].

- Similarly, lines 54 – 57 state that "So far most of the porosity wave model approaches used either equal bulk and shear viscosities, or simple laws in the form of [eqn (1) and (2)]", where equations (1) and (2) represent simple porosity dependent quantities. This is an incorrect statement – most model approaches but not most recent ones.

In fact, much more complicated relations for pressure-dependent weakening viscosity were considered in some recent works of [Omlin et al., 2018; Yarushina et al., 2015]. Moreover, Omlin et al. [2018] study the effect of the ratio of bulk and shear viscosities on the speed of wave propagation. The implication of this to magmatic systems can be important.

- Equations (3) and (4). Do you really need to keep ρ_f and ρ_s here?

- Lines 87 – 93. It is important to emphasize already here that P is the FLUID pressure and τ_{ij} is the EFFECTIVE stress tensor to avoid possible confusion. In 2-phase system there are many different pressures and stress tensors, i.e. solid stress, total stress, effective stress.

- Lines 94 – 101. As authors state earlier in line 84, solid and fluid densities are constant. Thus, it is a bit odd to read about linearized equation of state for fluid and mixture densities in lines 96-97. Mixture density is already linear with respect to porosity: $\rho = \rho_f\phi + \rho_s(1-\phi)$. The real meaning of equations (9) and (10) is introduction of new unknowns ρ_0, which is nothing else than ρ_s and c_f. Introduction of these new parameters is unnecessary and complicates reading a paper, which is already mathematically heavy.

- Lines 102 – 103. Statement "Neglecting capillary pressure at the melt-solid interfaces, the pressure is equal in the melt phase and in the solid phase" is wrong. Difference between solid and fluid pressures does not stem from capillary forces. What authors might want to say here is that parameter P in equations (5) and (6) is the same and is the fluid pressure. This confusion may be avoided if authors will label variables as suggested before.

- Equations (16), (17). Do you mean k_0 instead of k_ϕ here?

- Lines 188-189. Unclear sentence. Please, rephrase.

- Line 196 – 198. "They show that after a short transient time the wave velocity and

amplitude of the evolved porosity wave approach constant values in the limit of infinite resolution for all viscosity laws used." You do not have infinite resolution, please rephrase.

- Lines 236-237. "In Fig. 3 the dispersion curves of a model with a larger initial width than the resulting solitary wave. . ." Initial width of what? Please, be clear.

- Lines 246-248. Please, explain new variable m. Reading of the paper will be significantly easier if you would provide expressions for the bulk viscosity that was used for comparison with your simulations.

- Section 3.2. For each values of parameters that you discuss, it would be nice to see how far different bulk viscosities lie from each other.

- Lines 267 – 275. Authors compare their numerical solution with published analytical solution obtained in a small porosity limit and conclude that there is a deviation from analytical solution at larger values of amplitude. There could be various reasons for this deviation, including numerical error and limitations of the analytical solution as porosity grows higher. To rule out the second reason for discrepancy, it would be useful to compare results of numerical simulations, which are free of the assumption of small porosity, with full analytical solutions such as in [eqn (38), Yarushina et al., 2015].

- Authors conclude that agreement with analytical solution is reasonable. However, comparison was made only for moderate porosity waves with amplitudes below 14, where porosity within the channel reached only 7%. Waves with higher amplitude, which bring more significant amount of melt, will deviate much stronger. The melt fraction in the mantle maybe mostly below 7%. However, for results to have a more general application, the investigated porosity range could have been extended.

- Lines 403-406. "This can be explained by the different scaling which was used by Richard et al. (2012). If the same scaling is used, we get the same behavior. In contrast to Richard et al. (2012) we observe a narrowing effect of the waves for larger

background porosities, which cannot be explained by scaling. As Richard et al. (2012) used a 1-D model". Richard et al. used different expression for compaction length that contained porosity and thus, comparison, of course should take this into account. Please, be a bit more specific on what you mean by "different scaling". It would be useful to show some illustration to what kind of differences you see between your solution and previous result of Richard et al.

- Line 415. "exceed about 20% of the disaggregation values." It is better to use exact values here. Your disaggregation values are internal model parameter.

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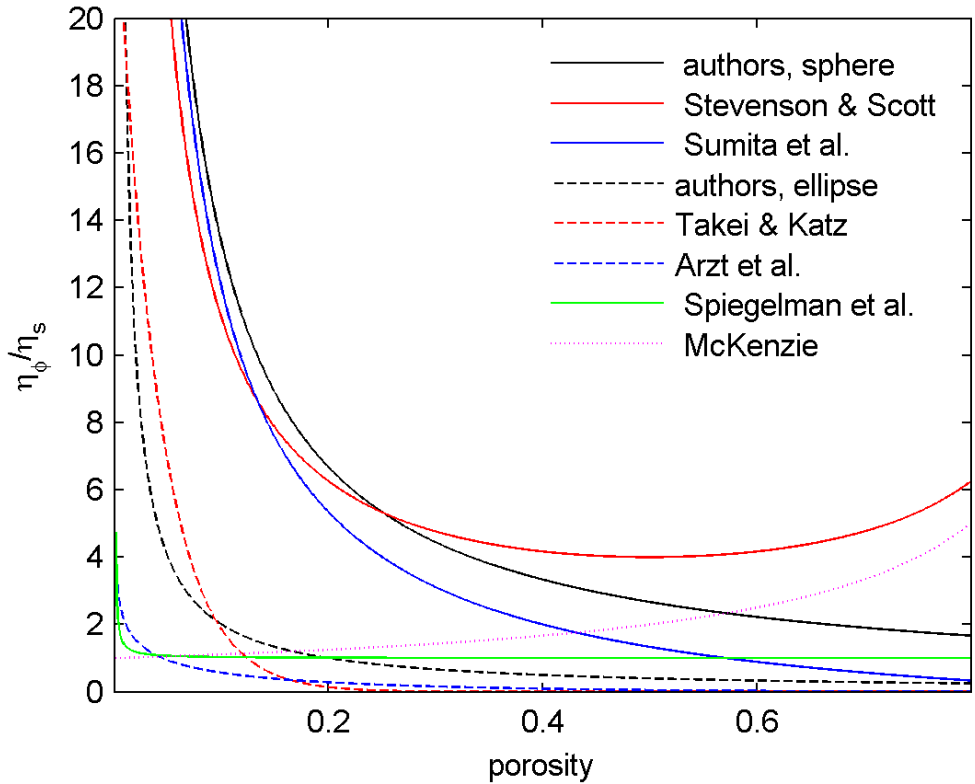

Fig. 1. Figure 1. Compilation plot of selected effective viscosities presented in the literature (from Yarushina and Podladchikov [2015])