Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-124-RC1, 2020

Interactive comment on "Magma ascent mechanisms in the transition regime from solitary porosity waves to diapirism" *by* Janik Dohmen and Harro Schmeling

Anonymous Referee #1

Received and published: 6 August 2020

The submitted manuscript presents parametric study of porosity wave propagation in viscous porous rocks. The novel aspect of the manuscript is the investigation of the effect of compaction length on the evolution of rising porosity waves. This is a welcome contribution since influence of material parameters and the size/geometry of the source region remains unclear. However, paper has several major drawbacks that need to be addressed.

Authors claim that they consider transition from porosity waves to diapirism. Here, I see a major conceptual problem. As often in geosciences, different terms got confused and mixed up. As I could grasp from the text, by diapirs authors understand wide

structures, while porosity waves are assumed to be narrow structures. This is already in contradiction with e.g. Wikipedia's definition of diapir, which reads as "A diapir,... is a type of geologic intrusion in which a more mobile and ductily deformable material is forced into brittle overlying rocks. Depending on the tectonic environment, diapirs can range from idealized mushroom-shaped Rayleight-Taylor-instability-type structures in regions with low tectonic stress such as in the Gulf of Mexico to narrow dykes of material that move along tectonically induced fractures in surrounding rock." Thus, according to Wikipedia all structures produced by the authors would fall into diapir category.

In the introduction authors describe diapirs as structures that are formed by Rayleigh-Taylor instability, which is commonly considered to be due to interaction of two immiscible fluids, whose behavior is described by Navier-Stokes equations. Porosity wave instability is described by Darcy law in combination with Navier-Stokes for solid. In other words, these are two different systems of equations. However, authors solve only porosity wave system of equations and thus Rayleigh-Taylor instability is not even considered in the paper. This is all very confusing for the reader and needs sharpening of the introduction and model description section. I would even suggest changing the title as diapirs in the sense of Rayleigh-Taylor instability are not even considered in the manuscript. I would suggest something more to the point, like "The effect of compaction length on solitary porosity waves and its implications for magma ascent mechanisms".

Another problem of the paper is the reliability of the presented simulation results. When changing the compaction length, authors produce porosity waves of different radius. Eventually, they become very narrow. We know from previously published research that numerical codes treating porosity waves are very sensitive to the resolution, so that several grid points are required for accurate results [Rass et al., 2019]. Thus, convergence of numerical results at higher resolution needs to be checked before acceptance of the paper. This is especially important for r'>10. We see from results presented in the first row of Figure 1 (low values of r') that porosity waves are circular blobs as expected. Other results exhibit some tails below the circular wave that authors interpret as flow focusing. However, these are exactly the results that may suffer from lack of resolution. Besides, tails behind the major porosity wave were repeatedly reported from 1D and 2D numerical models [Connolly and Podladchikov, 1998; 2000; Rass et al., 2019]. These disappear when simulations are left for longer time periods and waves and allowed to propagate further from the source region. I expect that if authors will allow their waves to run longer, they will see that eventually perfectly circle blobs detach from the cloud. Thus, observed pattern is not a flow focusing as such but just an initial smearing of the fluid propagation front. Eventually secondary waves could form from the remaining cloud.

Some detailed comments:

Section 2.1. The described above possible confusion with terminology requires extra care when describing your governing equations. You really need to explain what the similarities and differences in the description of both instabilities are and what exactly is included into your equations. Please describe here underlying assumptions of the model of Dohmen et al. What kind of simplifications assumed in this model? I think that a very brief approach of referring to Dohmen et al. is inappropriate here.

Lines 50-55. List of principal notations would help the reader, given that you have a lot of quantities with complicated indexes, such as δ_{c0}. Why not just δ? Why Darcy velocity has complicated index v_{sc0}, why not just v_D? Why permeability has index k_ϕ and not just k? Are you using k for something else? Please consider carefully, how to make notations simpler.

Equation 5. It is a bit odd to see ρ_s as an independent scale here together with 3 other scales (for length, velocity and viscosity). In principle, you can have only 3 independent scales in this problem. When you use them, you'll just get some non-dimensional parameters such as sedimentation rate in your system of equations.

Line 73. Please discuss small fluid viscosity limit. What are the typical viscosity values for solid magmatic rocks and for melt? What effects your simplified equations ignore?

Equation 11. Please comment here whether eqn (11) is a consequence of a usual Darcy equation or it follows some other governing law, e.g. Navier-Stokes? Which terms are omitted/presented?

Lines 85 - 90. You do not vary the radius of anomaly. The radius of your anomaly has always the same size. In the non-dimensional world, it is always w'=0.05L'. In the dimensional world it is always w=0.05L. What you are really looking at is the effect of lighter/heavier fluid in a more/less permeable rock, which will naturally have porosity waves of different size The description given in this para is very confusing.

Lines 90-91. Please comment how many grid points you have for the thinnest porosity wave.

Equation 14. Please explain this equation or provide reference for it.

Line 102. "As this radius and the maximum melt fraction change strongly during the run of a model" This just indicates that you did not reach steady-state wave propagation. See comment above.

Lines 105-107. I do not understand what you are trying to say here.

Section 3.1. This definition is very arbitrary. You do not have any diapirs in your model. You only have porosity waves of varying width. As we know, the speed of porosity wave depends on it size and thus you would have bigger and smaller waves travelling with different speed. It is interesting to compare those to the speed of diapirs, but they do not become diapirs here.

Line 109. "The transition from porosity wave to diapirism: Varying the initial wave radius" You do not vary initial wave radius, only compaction length, which is different.

Line 114. It is too early to talk about focusing at this depth. Your waves will become circular when they will propagate higher.

Lines 115-125. Porosity waves are very sensitive to resolution. How many grid points do you have per porosity wave for your runs at r'>=20? All discussions for these runs are meaningless as you clearly run into a problem of not resolving a physical process properly. For all figures with r'>=20 you need to show convergence at higher resolution.

Lines 128-134. What is the point of giving analytical cases that do not correspond your simulations? You have only n=3 and m=1. All these extra cases and lines only confuse reader without much useful information.

Line 133. Again, here I see a big issue with terminology and conceptual understanding. You do not have diapirs. Porosity within your model is never higher than 6 times the background, which is 0.5

Lines 170-175. I do not see how this is relevant for your simulations and porosity waves. It is precisely the difference in solid and fluid densities that drives evolution of porosity waves.

Line 233. "This could lead to the propagation of magma-filled cracks" Again, remember that max porosity in your simulations is 3

Lines 235-236. "But this effect might not be strong enough to lead" Which effect? Considered in your manuscript or in the paper of Connolly and Podladchikov? Unclear sentence.

Lines 238-239. Did you perform simulations with varying porosity/permeability or is this a hypothetical scenario you are describing? Please refer to simulations with varying/layered media.

Figure 2. Explain whether the colored lined are obtained from your numerical simulations or equations (14) - (15). You also need to provide somewhere equation used for dashed lines and comment on the parameters used in this equation.

Figure 3. It is a very interesting idea to compare Stokes and porosity wave velocities. This is one of the central points of this study and therefore much more careful description is needed here. Which equations and which parameters did you use for both? What is the sensitivity of these equations to parameters that are kept fixed (e.g., n or ϕ_0, etc). Obviously, your model did not reproduce any of the analytical velocities. Given the issue of resolution described above, you need to confirm your results at higher resolution. Letters on this figure and figure 4 are unreadable. Please increase the font.

References:

Connolly, J. A. D., and Y. Y. Podladchikov (1998), Compaction-driven fluid flow in viscoelastic rock, Geodin Acta, 11(2-3), 55-84, doi:Doi 10.1016/S0985-3111(98)80006-5.

Connolly, J. A. D., and Y. Y. Podladchikov (2000), Temperature-dependent viscoelastic compaction and compartmentalization in sedimentary basins, Tectonophysics, 324(3), 137-168.

Rass, L., T. Duretz, and Y. Y. Podladchikov (2019), Resolving hydromechanical coupling in two and three dimensions: spontaneous channelling of porous fluids owing to decompaction weakening, Geophys J Int, 218(3), 1591-1616, doi:10.1093/gji/ggz239.

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