Thanks to the reviewers we were able to greatly improve this manuscript and we could hopefully answer all comments made good enough. It was quite a lot of work we put into this manuscript after receiving the reviews and a big part of it was completely rewritten or changed and quoting it all here in the answer letter would mage it hard to follow. Therefore, I will just briefly explain the biggest changes made here and kindly refer to the new manuscript.

We completely rewrote the mathematical description of the model which now includes the dimensional equations and a small chapter about how these equations are solved. We also use a new non-dimensionalization using the Stokes velocity and the radius of the initial perturbation. This allows us now to describe the Stokes limit, where the old description failed. It might also help to better understand the quite complex model setup. Due to the change in scaling all figures had to be remade and are now hopefully up to the standards and everything is readable.

Regarding the figures, we removed former figure 3 as we think that it didn't give much more information than figure 2 already gave and is quite complicated to understand and describe. Instead we added a new figure containing a resolution test which is described and analyzed in a new chapter called "numerical issues".

The main point in this new manuscript is that we now state that the focusing, we formerly stated are small porosity waves, are channels that build up in front of the wave due to the horizontal stresses occurring there. This new statement is described and analyzed in the results part of the manuscript, where it replaces the argument of the small porosity waves.

In the discussion we now discuss the growth rates of these channels in our models and compare them to Stevenson (1989).

Several other parts in the manuscript had to be changed according to our new statement and are not especially mentioned here but are marked in the updated manuscript in red.

Below you will find the comments made by the reviewers in black and our answers in red.

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.

We do not agree with the English version of Wikipedia. Diapirism is not necessarily related to brittle overburden and to a more mobile buoyant material. We use diapirism in the sense it had been defined and introduced e.g. by Turcotte and Schubert (1981), Simpson, 1989, and many others in the 1970s and 1980s. We specify our (and the common geologic) definition:

"Addressing different melt ascent mechanisms it may be useful to specify our definition of diapirism. Originating from the Greek "*diapeirein*", i.e. "to pierce through", diapirism describes the "buoyant upwelling of relatively light rock" (Turcotte and Schubert, 1981) through and into a denser overburden. In the general definition the rheology of the diapir and ambient material is not specified, both can be ductile as in our case, but often, the overburden is assumed being more viscous or even brittle. Buoyancy may be of compositional or phase related origin, e.g. due to the presence of non-segregating partial melt (Wilson, 1989). Based on these definitions in our case a diapir is a rising, partially molten body or porosity anomaly with zero fluid-solid separation velocity. Mathematically the equations of motion of the two-phase system degenerate to the Stokes equation (see below)."

In the introduction authors describe diapirs as structures that are formed by RayleighTaylor 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".

We do not consider or mention Rayleigh Taylor instability in our paper, per definition a RT instability starts from a stratified and not from a buoyant circular anomaly. But thanks to the reviewer, we specify the Darcy and Stokes type of equations for the both end member now.

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.

Yes, numerical resolution is a major problem in modelling porosity waves and or model setup will inevitably lack in decent compaction length resolution. Anyways, we now state that the peaks we observe in the transitional regime are channels which are still resolvable with our resolution. Channels also explain why the tail behind the leading wave does not get smaller or even vanish but growth with time. We now calculate growth rates and show that they agree with Stevenson (1989).

Still, resolution is a major issue and we added a new chapter about “numerical issues”.

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.

Now we specify how the former equ 11 is derived, which reveals the inherent assumptions. As for the “small fluid viscosity limit” we add:

“In the small fluid viscosity limit the viscous stresses within the fluid phase are neglected, resulting in a viscous stress tensor in the Stokes equation of the mixture (equ. 4), in which only the stresses in the solid phase are relevant. This is evident from the definition of the viscous stress tensor, which only contains matrix and not fluid viscosities. Melt viscosities of carbonatitic, basaltic or silicic wet or dry melts span a range from < 1 Pa s to extreme values up to 10^{14} Pa s (see the discussion in Schmeling et al., 2019), while effective viscosities of mafic or silicic partially molten rocks may range between 10^{20} Pa s and 10^{16} Pa s, depending on melt fraction, stress, and composition. Thus, in most circumstances the small fluid viscosity limit is justified.”

Lines 50-55. List of principal notations would help the reader, given that you have a lot of quantities with complicated indexes, such as δc0. Why not just δ? Why Darcy velocity has complicated index vsc0, why not just vD? Why permeability has index kφ 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 ρs 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.

As we completely revised our mathematical description, this problem is hopefully solved. We still stick to k_φ and δ_c as these notations are commonly used but we got rid of the zero notations.

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?

See above.

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.

This was probably also caused due to the confusion with the non-dimensionalization. But we are in fact changing the radius of the emerging solitary wave. When we double the characteristic compaction length of the model the solitary wave will be also double the size in [m] as the wave will be the same size in terms of compaction length.

Now with the new description it might be easier to understand the model setup. We also gave a small example.

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

As we now state that they are channels and not porosity waves the number of grid points per wave is no longer that important, but we also have a look at the resolution with a new figure with different resolutions. We see that the channels are resolvable as long as the grid size is approximately in the order of the compaction length.

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

This equation is the commonly used Stokes equation, which is now referenced with (Turcotte & Schubert, 1981).

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.

See above.

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

Yes, this sentence was a bit odd and is no longer part of the new mathematical description.

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.

This might be a confusion due to the old description of the theory, as it wasn't able to describe the Stokes limit. With the new description we now are able to describe it and it might be now clear that we get diapirs in the sense we stated above.

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.

See above.

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

Now with the channels we clearly observe focusing in the sense that melt gets accumulated in a smaller horizontal area.

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.

See above.

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.

The figure has been revised and now shows just the relevant case of n=3 and m=1.

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

See above.

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.

This is part of our Boussinesq approximation, where all density differences are dropped but in the buoyancy term of the momentum equations.

We now describe this approximation in our mathematical description.

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

Yes, this is true, but the melt porosities would be way higher if we would allow for further focusing. Also our model starts with very low melt porosities as these models are more robust, but we would observe the same behavior for higher porosities.

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.

The sentence has been changed to:

"But this upward weakening might not be strong enough to lead to the focusing needed for the nucleation of dykes"

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.

We did some simple tests with several different layers that have different shear and bulk viscosities and solitary waves passing through them as port of another project. This test was just very simple and not enough to show in this paper but shows exactly what we describe here, a focusing. We did not, however, perform simulations with varying background porosity as this model setup might be even more complex. The case of differing viscosity should however have the same effect, as it also changes the compaction length.

The sentence has been changed to:

"In the hypothetic case of a porosity wave reaching the top of a magma chamber, the background porosity might decrease which would most certainly lead to focusing, because the compaction length will decrease, and eventually, when reaching melt free rocks, the melt rich fingers may stall as in our models at $r > 50 \cdot \delta_c$ and the rising melt will accumulate and enter the pure diapirism regime"

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.

The dashed lines in the original figure are now the colored ones. They are calculated semi-analytically using the program provided by Simpson & Spiegelman (2011) and there is therefore no analytical equation to describe these curves.

The former colored lines were calculated analytically with the mentioned equation for the Stokes sphere, but this information is now no longer needed.

The caption of the figure was changed to:

The dashed line marks the velocity of the Stokes sphere ($\mathrm{v}' = 1$). The colored lines show the velocity of a 2D solitary wave, calculated semi-analytically by Simpson & Spiegelman (2011), in our non-dimensionalization, based on the radii shown in the legend.

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 φ0, 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.

We now got rid of figure 3 as it was quite complex and did not really give any more information that is not already in figure 2. Sensitivity to the parameters kept fixed is a whole different story. Changing φ_0 should lead to minor changes in the results as we used simplified viscosities. In Dohmen et al. (2019) we have a look at the behavior of SWs for different background porosities. They play a major roll with the more complex, lower viscosities, used there. Changing *n* would probably change the results, but it would need much more time to get a similar work with n=2.

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.