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 #2

General comments

The submitted manuscript systematically investigates magma ascent dynamics in order to capture the transition from the solitary wave regime to diapirism. The authors explore this transition by varying the relative compaction length of the system - here by changing the model extend dimensions while keeping the compaction length constant. Investigating fluid transport mechanisms in Earth subsurface is of broad interest with applications not only limited to melt in the crust, and thus the study is a welcome contribution. Although the title and abstract sound promising, the study presents several important issues that need to be addressed before to be further considered for publication.

1. The study's design

The authors claim to resolve the transition from solitary wave of porosity to Stokes-like diapiric rise of magma. These two regimes are very different. The solitary waves of porosity occur in two-phase medium, when the fluid has a relative velocity compared to the solid. The diapiric ascent occurs if the fluid has no or very limited mobility with respect to the solid and thus the medium behaves as single-phase. The authors report here briefly the two-phase flow equations they rely on, which permit to resolve the two-phase motion. However, it is unclear what happens in the single-phase flow limit. In this limit, the equations should reduce to the single phase (Navier-) Stokes system. This part is totally absent from the study, both in the physical description (system of equations) and from the numerical implementation. The authors overlooked a study from Scott (1988) investigating a very similar research question, namely "The competition between percolation and circulation in a deformable porous medium". This short communication may be highly relevant and may support or challenge some statement claimed by the authors.

We totally agree with the reviewer here. The equations given in the former version describe the two-phase flow limit but fail in the Stokes limit. Because of that we introduced a new non-dimensionalization that is capable of describing both limits.

The "Governing equations" section was completely rewritten.

We now mention the research of Scott (1988):

This switch from negative to positive mass flux was already observed by Scott (1988), but while he changed the viscosity ratio, we change the radius and keep the viscosity ratio constant. Both describe the transition from a two-phase limit towards the Stokes limit, but in our formulation we are able to reach the Stokes limit while Scott (1988) is still in the two-phase flow regime.

2. The numerical implementation

In this study, the authors rely on numerical modelling to investigates the effect of changes in compaction length, or rather vary the domain size keeping the compaction length fixed. Being a numerical study, the current manuscript seriously lacks in robust model description, numerical implementation, benchmarking. These (non-exhaustive) steps are the basic technicalities one is expected to report when performing numerical experiments. The authors emphasise both in the Abstract and the Introduction the numerical challenges relative to accurately resolving fluid migration in the subsurface. However, no further discussion about numerical method, implementation, benchmarking, sensitivity analysis, etc... is present in the manuscript. The model configuration is poorly described and some basic information such as the numerical grid resolution should be reported in a well-crafted "Numerical Implementation" section well before the final discussion. Although focus should not be on benchmarking, ensuring accuracy of the numerical scheme and related results is primordial in studies like this one. As reported recently by Räss et al. (2019), lack of numerical resolution may lead to erroneous results. I am afraid that part of the results reported in this study are under-resolved, as at least a few tens of gridpoints are needed per compaction length to obtain accurate results. Also missing is the description of the transition from two-phase flow to single-phase flow. How do the authors treat the very small compaction length limit? In this limit, Stokes flow is dominating, and the motion of the fluid pocket needs advection of the solid matrix. There is no information regarding this important point in the manuscript. The governing equations are very cryptic, and it would be very helpful to see the finally implemented closed system of equations that is actually solved numerically, together with information on the numerical scheme that is used.

Yes, the numerical resolution is a major issue, but now, as we revised our statements, the resolution is no longer as big of a problem as before. The small porosity waves we observed in the transition regime would have been most certainly not decently resolve, but now we state that we observe channeling in this regime, based on Stevenson (1989), which are resolvable by our resolution. The channel's wavelength in our models is in the same order as in Stevenson (1989) and the growth rate is explainable as well.

We still added a small chapter about numerical issues to the results, that tells a little bit about the issues observed.

We also give a small introduction on how we solve the equations numerically and, as already stated above, we changed the mathematical description so that we are now able to reach the Stokes Limit.

3. The quality of the reported results

The reported results are interesting but in light of the previous comments, further work would be welcome to refine the Results and Discussion sections. The authors could put some additional efforts in crafting better quality figures. There are missing labels, fonts are very small and hardly readable in some cases, and figure captions display repetitions and miss important details. Also, it may be interesting to report in form of quiver plots the solid and fluid velocity components as those could be directly compared to results obtained by Scott in 1988.

All figures were revised and are now hopefully up to the standards.

To summarise, this manuscript tackles an interesting and not yet fully resolved question, but the study's design, numerical implementation and overall quality should be seriously improved before being considered for publication. Addressing these issues are important as in the current status it is hard for the reader to discriminate between resolved dynamics or numerical artefacts, especially in the transition regime. In the Discussion, the authors provide some insights in the challenges related to resolving the two-phase dynamics for large domains (or small compaction length). There may be a conceptual study design issue there. The authors spell out all the pitfall and they don't, but their study actually reports results that exactly suffer from those drawbacks. and may not be accurate. A potential way to improve the study would be to move a large part of the issues raised in the discussion to the Section 2. For example, the discussion about the numerical grid resolution should appear much earlier. Then, one could discuss the issue, try to solve it. And if results cannot be trusted, then one should identify them and discard them from the analysis.

Detailed comments

l.18: In the current status, these may be numerical artefacts as well. Appropriate benchmarking would be welcome (e.g. running a test setup at various resolutions and reporting the results).

We now state that the "numerical artefacts" mentioned are channels which are resolvable and show its dependence of resolution in a resolution test.

l.21: For accurate results of porosity waves, numerical resolution should always be such to have about 10 grid points per compaction length.

Yes, such a resolution would be desirable, but is hard to reach in many models. Anyways, as we now state channels this minimum resolution criteria is no longer applicable.

l.23-24: True, one should be careful. Please report how you carefully addressed these resolution issues.

See above.

l.47-49: Important question on "what are the numerical implications on modelling magma transport". Within the manuscript, however, these implications are discussed but it appears that the suggestions provided are not followed by the authors themselves.

See above.

Section Introduction: Please update it putting your contribution in light of previous work such as Scott (1988) and other potential studies.

We added a small comparison of our models to Scott (1988):

"Scott (1988) already had a look at a similar scenario. He calculated porosity waves changing the compaction length by altering the shear to bulk viscosity ratio, while we want to change the radius of a partially molten perturbation in terms of compaction lengths but keeping the viscosity constant. While Scott (1988) was not able to reach the single-phase flow endmember due to his setup we can reach this endmember with our description and can show how the transition looks like."

eq.8-11: These are non-intuitive formulation of the momentum balance. What do v_1 and v_2 stand for? Please take some place to better describe the approach.

We completely revised the mathematical description and now explain how we get the momentum balance. This description is hopefully more intuitive. v_1 and v_2 have been explained as well.

Section 2.2: Please complete the model setup.

With the new non-dimensionalization the model setup is hopefully better understandable. We also give a small example as the model series is not really intuitive.

l.85: What value of A do you use in the experiments?

We now mention the Amplitude of the wave:

" … where A is the amplitude equal to 0.03 in our models…"

l.90: This may be problematic as number of grid points per compaction length will decrease with increased nondimensional box size.

Yes, this is a problem, even though we now observe channels. But it is not really possible to keep the resolution of the compaction length constant. From r'=1.5 to r'=100 we would have to increase the resolution with a factor of 66, corresponding to a resolution of 13201x13201. Even when we say we don't need a higher resolution for the bigger radii as the compaction length doesn't need to be resolved as good, we still have very high resolutions with high CPU-times. We also observed that some models become unstable with very high resolutions, which is not explainable by now.

l.92: Can you precise what out and inflow conditions you use for the solid? Majority two-phase flow simulation apply free slip boundary conditions for the solid or porous matrix. Please clarify the model configuration - this is crucial for reproducible science.

We now describe the in and outflow more:

"At the top and the bottom, we prescribe an out- and inflow for both melt and solid, respectively, which is calculated analytically for the background porosity. This is necessary because we have a background melt fraction φ_0, that has a certain buoyancy which would lead to an accumulation of melt at the top of the model. We therefore calculate the segregation velocity for background porosity using equation (17) without the viscous stress term. The corresponding matrix velocity is calculated using the conservation of mass."

l.93: What do mirroring boundary conditions refer to?

We now explain the mirroring boundary conditions:

"At the sides we use mirroring boundary conditions, which corresponds to a symmetry axis, where no horizontal flow is allowed."

l.98 + eq.14: Please provide relevant reference for the Stokes velocity?

The Stokes velocity is now introduced earlier in the mathematical description and a reference has been added: Turcotte & Schubert (1982).

l.99: Please justify the choice of the radius you utilise in the Stokes formula.

We added a small justification:

"We use the halfwidth of the initial perturbation as radius for the Stokes velocity. This is reasonable as the amount of melt in the perturbation is approximately equal to the amount of melt in a spheres cut with a sharp boundary of radius r, for what the Stokes equation is valid."

eq.15: Ar not defined

Ar was actually A times r, where A is the amplitude of the initial perturbation and r its radius. With the new description A has been replaced by φ_{max}.

Section 2: Besides the model setup, please report what final equations are implemented in the numerical model. Please also report about your numerical implementation, discretisation, solution strategy; all standard components one is expected to see in a numerical study that would enable reproducible science.

The new mathematical description might now solve this comment, as we now start with the dimensional equations. We also added a paragraph to the numerical strategy.

l.117: This may indeed show lack of numerical resolution.

See above.

l.121-126: No focussing is expected for linear shear and bulk rheology. The focussing you report here may rather be attributed to the still transient state of the model evolution - maybe due to the coarse resolution. To verify this, a higher resolution simulation on a larger domain should be carried out and running until the shape stabilises.

We now state that this focusing is a channel which is able to evolve with the rheology used in this work.

l.128-132: Why to report various analytical values when your simulation was carried out only with n=3, m=1. This only confuses the reader.

Good point. With the revision of the figures we now show just the n=3, m=1 case. With the new depiction it would have been even more confusing.

l.164.167: Internal circulation would be great to see in a figure. It is difficult to assess and acknowledge your findings based on text only.

As this chapter was deleted, we don't mention the internal circulation. Just for interest one could add a vector field to one of the figures, but the waves shown are all to small to see something then. Adding a new figure wouldn't make much sense as it wouldn't be referred to.

l.170-171: How can you neglect the density difference between solid and melt. This should be the driving force.

We neglect the density difference everywhere but in the buoyancy terms of the momentum equations. This is part of the Boussinesq approximation, we now explain in the mathematical description.

l.218: This conclusion should be verified by a higher resolution run.

See above.

Section 3: May need further development upon updated results

The section was partly rewritten and now addresses some of the issues stated above.

l.224-248: Interesting insight but all these hypotheses should be tested within appropriate modelling framework including spatial variations in the suggested material parameter fields and using sufficient numerical grid resolution to allow resolving the smallest features. Also, note that focussing will only occur if there is asymmetry among compaction and decompaction of the porous matrix, i.e. for non-linear rheology.

We now replaced focusing with channeling which is able to evolve with linear rheology. Still we are not able to resolve even the smallest features but the channeling we now state is less affected by the lack of numerical resolution.

l.249-264: Good point, but it seems that this study exactly shows the reported artefacts in the results.

See above.

References

Scott, D. R. (1988). The competition between percolation and circulation in a deformable porous medium. Journal of Geophysical Research: Solid Earth, 93(B6), 6451-6462.

Räss, L., Duretz, T., & Podladchikov, Y. Y. (2019). Resolving hydromechanical coupling in two and three dimensions: spontaneous channelling of porous fluids owing to decompaction weakening. Geophysical Journal International, 218(3), 1591-1616