I would like to thank the reviewers for their extensive and detailed reviews. Both gave us many suggestions, not only on the scientific part, but also on the written English. I hope that my English will become better in the near future. Anyways, the manuscript was edited, but I had not the time to get professional help. If the manuscript is still not good enough there might be an English editing by the Journal. At least there was one for my last paper published in EGU Solid Earth. If that is not the case, I could find an editing service myself.

In the following you will find the reviewers comments in black and my answers in red. In the marked-up manuscript all changes were also marked in red.

Reviewer 2

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The submitted revised version of the revised manuscript by Dohmen and Schmeling 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 extent while keeping the compaction length constant.

In this latest revision the authors finally address the study's major early design issue. The current version is scientifically sound and brings some insight on the transition without extensively reporting numerical artefacts.

The current manuscript lacks however in quality regarding the writing style which significantly affects readability. You'll find hereafter some ideas on how to improve it. Another solution would be to get the manuscript revised for language by a professional language service or get some advice from a native speaker.

The author should clarify the pressure issue and elimination discussed between equations (3)-(10). I agree it is not the scope of the paper to sort out the confusion and extra complexity introduced by McKenzie (1984), but the current explanation is not consistent. It would be of great help to the reader to have well defined concepts with clear names referring to that are kept constant throughout the entire manuscript. To this end, it would be appreciated to have a clear definition of what is the melt, the fluid, the effective properties, the mixture and other quantities introduced on a non-systematic basis throughout the paper. Then, once this framework and naming conventions are set, define the pressure as it should. I suspect in the current form the pressure called fluid pressure (and missing the subscript f) may not be the pressure of the pore-fluid but a composite fluid pressure defined by McKenzie.

The pressure in our equations is the fluid pressure, where we now added a subscript f. The fluid pressure is balanced by the lithostatic pressure for solid density, the compaction pressure and the dynamic pressure (eq. 5). The dynamic and compaction pressure are both included in the stress tensor (eq. 6), as we state now in its definition. The fluid pressure in our equations should therefore be the fluid pressure and not a composite fluid pressure. I agree that in our description the pressures are not defined individually, which may lead to confusions, but the description should be consistent. The pressure in eq. (3) is the fluid pressure as well and we can use eq. (5) + (6) to achieve eq. (10).
I cannot find any wrong notations in our description, but we now use $\overline{\rho}$ for the mixture density and P_f for the fluid pressure, which should clarify some possible confusions. Additionally, the volume viscosity is now given by ζ, the shear viscosity by η and the dynamic fluid viscosity by μ, because we changed the term bulk viscosity to volume viscosity and adapted our notation.

Based on these considerations, I would recommend this draft for publication after extensive minor revisions (more than just fixing the detailed comments below).

General comments
Hereafter some ideas and suggestions on how to enhance the writing style applicable to the entire manuscript. Sentences should be kept short and as concise as possible. One way of achieving this is to switch to the active voice wherever it is possible.

Action verbs may also help getting straight to the point; consider using more precise verbs rather than "doing".

The text is populated with too much useless linking words. One possibility to fix this would be to remove all parasite linking words, read through the text, and add them back in places where having them missing would seriously alter the meaning or the logical reasoning.

The text currently includes a lot of familiar "spoken" jargon. Consider removing these or replacing them with more scientific and precise concepts.

Regarding the layout and style, it is considered good practice to have inline equations in the text, preferring horizontal fraction expressions to / for better readability. Also, referring to figures and equations in a passive fashion (e.g. "Our results confirm A (Fig.X)" instead of "Figure X shows that our results confirm A."") may often help to increase flow and readability.

Specific comments
l.6 Consider capitalising the "Earth" throughout the text

“Earth” has been capitalized throughout the text.

l.8 solitary porosity waves -> Solitary waves of porosity

I think the term “solitary porosity waves” is more common and is not necessarily less clear. Nothing has been changed.

l.10-11 a mechanism could be dominant, not really a wave or a diapir. Consider a more accurate formulation.

The sentence has been adjusted to:

Thus, the size of a partially molten perturbation in terms of compaction length controls whether material is dominantly transported by porosity waves or by diapirism.

l.16 For enhanced readability I would suggest a full stop after "flux" and starting a new sentence for the melt.

The suggestion was accepted:
If the perturbation is of the order of a few compaction lengths, a single solitary wave will emerge, either with a positive or negative vertical matrix flux. If melt is not allowed to move separately to the matrix a diapir will emerge.

l.37-38 "However, ..." consider simplifying the sentence making it straight to the point.

Regarding suggestions made in the Introduction:
The introduction was to large parts rewritten, following most of the suggestions made by the reviewer.

l.43 "On the other hand" suggests "on the one hand" to appear first. Consider removing unnecessary linking words for enhanced clarity.

l.48 Consider being more precise than "transition looks like" which does not sound very scientific. What interests you in the transition regime ?

l.51 Consider being more factual in this last sentence switching subject from authors to end-member scenarios.

l.54 please refine or clarify the concept of "partially molten scenario".

l.56 "and look especially [at] what happens" doesn't sound very scientific. Consider being more precise and avoid using familiar jargon in a scientific publication.

l.62 Be more specific about "more viscous" -> I suspect you mean feature larger viscosity values, e.g.

l.64-65 "Based on ..." sentence's construction is suboptimal making it hard to get the point. Consider revision for more clarity.

l.66 Consider adding "single phase" to "the Stokes equations" ?

l.72-74 "In the present Boussinesq ..." please rephrase this sentence making the comparison clear.

We already revised the part about the Boussinesq approximation and explained it quite detailed. I don't know how to explain it better, but I think it should be already clear enough.

Section 2.1 Revise usage of fluid and melt. The terminology is mixed up and maybe miss-used in some places in this section, giving the reader hard time to follow the mathematical model formulation.

I could not find any miss-used subscripts. Maybe some irritations came along with the density, where we used ρ for the mixture density. We now use $\overline{\rho}$ as it is most common.

l.84 If P is the fluid pressure, consider adding the subscript f to it for consistency.

The subscript f was added to all P throughout the text.

l.85 It is unclear how P, being apparently Pf, would actually include a lithostatic component.

s.a.

l.86 What is P in the Stokes equation for the mixture, Pf or another pressure ? If P stands for Pf here, it is confusing as this would mean fluid pressure balances the mixture buoyancy forces and shear stresses.

The second term of the stress tensor is the compaction pressure, which we now explicitly state in the definition of the tensor:

… and $\boldsymbol{\tau}$ is the effective viscous stress tensor of the matrix including both shear and compaction components…

l.88 Eq. (5) is known as Kozeny-Carman relation. Consider stating it and referring to e.g. Costa 2006: Costa, A., 2006. Permeability-porosity relationship: a reexamination of the Kozeny-Carman equation based on a fractal pore-space geometry assumption, Geophys. Res. Lett., 33(2), L02318.

The Kozeny-Carman relation is now mentioned and cited:
This relation is known as the Kozeny-Carman relation (e.g. Costa, 2006).

l.88 power-law "n" definition seems missing

Yes, it was missing and was now added:
with n being the power-law exponent constant, usually equal to 2 or 3.

l.89 ηf, g and ρ refer to Eq.(4), consider moving these definitions right after the equation they appear in.

The equations and the definitions of the parameters have been slightly rearranged:
The momentum equations are given as a generalized Darcy equation for the fluid separation flow

$$\vec{v}_f - \vec{v}_s = -\frac{k_\varphi}{\eta_f\,\varphi}\left(\vec{\nabla}P_f - \rho_f\vec{g}\right), \tag{3}$$

where ρ_f is the fluid density and P_f is the fluid pressure (including the lithostatic pressure), whose gradient is driving the motion. k_φ is the permeability that depends on the rock porosity

$$k_\varphi = k_0 \varphi^n, \quad (4)$$

with n being the power-law exponent constant, usually equal to 2 or 3. η_f is the melt dynamic viscosity, $\vec{g}$ is the gravitational acceleration. The Stokes equation for the mixture is given as

$$\rho \vec{g} - \vec{\nabla} P_f + \frac{\partial \tau_{ij}}{\partial x_j} = 0. \quad (5)$$

ρ is the density of the melt – solid mixture and τ_{ij} is the viscous stress tensor…

l.90 τij is the mixture, solid, total, melt stress tensor ? Please be more precise.

The description of the tensor is now more precise:
… and $\boldsymbol{\tau}$ is the effective viscous stress tensor of the matrix including both shear and compaction components…

l.94 "simple" does not provide any further information - consider removing it.

"simple" was removed.

l.119-121 Be consistent with the comparison 1 Pa.s - 1e14 Pa.s and then you switch 1e20 Pa.s - 1e16 Pa.s; consider choosing increasing or decreasing values for both.

The values now increase for both cases.

l.155 Consider rephrasing this sentence to the active voice for more clarity.

The sentence was rephrased:
All quantities in the other equations are simply replaced by their non-dimensional primed equivalents (eqs. (1), (2), (6), (11), (12), (13), and (14a)).

l.157 "We can now compare..."

The sentence was changed according to the reviewers suggestion.

l.157-159 Long and complicated sentence to state something trivial. Consider revising for enhanced clarity.

I don't think this sentence is too long and complicated, but we changed the sentence a bit and is now hopefully more clear:
We now compare the two limits, where segregation or two-phase flow dominates (solitary wave regime), and where fluid and solid rise together with the same velocity as partially molten bodies, which we identify with the diapir regime.

l.159 Consider replacing familiar expressions with more accurate terms. E.g. "this can be done" -> "we compare the characteristics..."

The suggestion was accepted.

l.168 Why to have italic text here. Consider clearing formatting or implementing corresponding format to the "diapir limit" text.

The italic formation was removed.

l.174 Complete "equ" word

The word is now complete

l.190 Consider replacing big by large and spell out what you mean with "the other way around". This sentence could be formulated as a dual comparison in a concise manner.

The sentence was rewritten:
Smaller amplitudes lead to a switch at a smaller radius and larger amplitudes to a switch at a larger radius.

l.194 Is "Gaussian wave" the most suited terminology for describing your initial condition ? Consider revising it, maybe a "Gaussian profile" or distribution ?

“Gaussian wave” was replaced by “ Gaussian bell-shaped”.

l.199-200 What do you refer to as "model series" ? What do you refer to as "parameter range towards the diapiric regime" ?

The sentence was revised:
In our model series we vary the ratio of Stokes radius to compaction length from 1.8 to 48 to explore the transition from solitary wave towards diapiric regime.

l.200-201 what unit is your 201 x 201 model resolution? I suspect it is the number of grid points. Please add it. What does a "high length-scale ratio" stand for ? Consider being more precise.

The sentence was revised:
The resolution of the models is chosen to be at least 201×201 grid points and was increased for higher ratios of Stokes radius to compaction length so that the compaction length is resolved by at least 3-4 grid points.

l.203 "at the top and the bottom" of what ? What about "At the top and bottom domain boundaries we ..."

The suggestion was accepted.

l.203-205 Be more specific and scientific in the description. Simply state what boundary conditions you implement and for which reason in an active voice.

The paragraph was revised:
At the top and the bottom domain boundaries, we prescribe an out- and inflow for both melt and solid, respectively, to prevent melt accumulations at the top. The segregation velocity of the background porosity φ_0 is calculated using equation (17) without the viscous stress term. The corresponding matrix velocity is calculated using the conservation of mass.

l.208-209 What does the reader get as add-on value knowing that the lateral BCs are "mirroring" ? Why don't you just state "We enforce no flux horizontal boundary conditions." ?

The suggestion was accepted.

l.209 The power-law exponent should be defined in Section 2.1 and n=3 could potentially show up there already if it is constant throughout the study.

The power-law exponent is now defined in Section 2.1, but I think it is still important to have it here in the Model description.

l.210-214 You implement a moving frame. Could you be more precise or drop the shifting information ? Once φmax reaches L/2+dz you shift the entire model down from one dz. But then you repeat this for every next dz travelled by φmax ?

Yes, the information on how we realize this moving coordinate system might be not so important. The paragraph was revised:
To run models for a longer, practically infinite, amount of time we let the models coordinate system follow the maximum melt fraction. This procedure allows us to zoom into the perturbation and follow it, not knowing its velocity and without carrying out any interpolations, which would strongly influence the model.

l.217-218 What about "We discretise the non-dimensional set of equations (xx-yy) using finite-differences on a staggered? regular? grid and solve the system using the FDCON code (Schmeling et al., 2019)"?

The sentence was rewritten:
We discretize the set of equations using finite differences on a staggered grid and solve the system using the code FDCON (Schmeling et al., 2019).

l.250-254 It is motivating to read that benchmarking was carried out. It would be highly appreciated if the statements could be supported by one or two figures in e.g. an Appendix.

I think adding an Appendix just for a figure of a numerical test is not necessary, as Fig. 2 already shows the important outcome of the numerical test, namely the evolution of the initial perturbation with the resolution used for model series is approximately equal for higher resolutions. A paragraph without a figure about the convergence of velocity and melt fraction amplitude should be enough.

l.257-259 This explanation is very helpful to remind the reader about the experiment design.

l.267 I would reformulate the sentence making such as "We compare the observed solitary wave velocities (Fig3b-e) to equivalent Stokes velocities for a diapir based on eq. (15)". Making more concise sentences without decoration words will strengthen your sayings and clarify the text a lot.

The suggestion was accepted.

l.273-274 "fits quite nicely". Either it fits, or not. If it fits not exactly, select a precise word instead of "quite nice".

The sentence was revised:
These semi analytical solutions are in good agreement to our solitary wave models, as already shown in Dohmen et al. (2019).

l.274-275 Here and in some other paragraphs, avoid using "can". It reads you are not trusting your results or awaiting the reader to approve your claims. It does not read well.

This sentence was changed. And we tried to avoid "can" in other sentences as well:
The velocities in this figure correspond to ratios of solitary wave velocity to initial perturbation Stokes velocity.

l.315-317 Consider switching this sentence to the active voice and making a linear construct, subject, action verb, complement. Also consider assessing whether "Just in the case" is really needed.

The paragraph was revised:
A classical diapir will evolve only in cases with zero compaction length ($r = \infty \cdot \delta_c$), i.e., melt is not able to move w.r.t. the matrix (Fig. 3k). Here, no focusing into solitary waves can be observed and transition time is infinity.

l.345 Consider switching to the active and affirmative voice. Report what you did and not what you think you may have done, e.g. "A quantitative analysis of ...".

The sentence was changed:
The transition from solitary waves towards diapirism on qualitative model observations was so far only based on observations.

l.349-351 "soli" and "dia" not being variables, consider a non-italic font style.

“soli” and “dia” are no longer in italic.

l.369 Scott isn't himself "in the two-phase flow regime". His results suggest it. Consider modifying here and other places in the text where you refer to authors instead of author's results.

The sentence was revised:
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’s formulation (1988) is restricted to two-phase flow.

l.400 "that rise diapiric as a swarm" -> "that rise as a diapiric swarm".

The suggestion was accepted.

l.410-415 The work of Keller 2013 may provide some references to you claims

The paragraph was changed and Keller et al. 2013 was cited and discussed shortly above.

l.416-421 Unclear formulation and complicated sentence construct. Consider revising and simplifying.

The paragraph was rewritten:
Even though most diapirs should, according to our models, disintegrate into numerous solitary waves, not all will inevitably. Within regime (1) solitary waves are possible and most probably expected but the deeper we are in regime (2) the less expected is the disintegration because a long time is needed to build up. In nature, different from our models, they cannot rise for an infinite amount of time.

l.436 Consider changing the title "Other issues" to "Model limitations" or another positively relevant item. "Other issues" sounds like previous material is already an issue which is not the case.

The suggestion was accepted and the subsection is now called “Model limitations”.

l.449 Although solitary waves may be observed on non-resolved resolutions, their properties such as velocity would be non accurate.

l.464 What does a and b stand for or refer to ? Please add precision there.

The explanation for a and b is written in the next sentence. To make it clearer a and b was added to the number of the enumeration:
(1a + b) Solitary wave a and b, (2) solitary wave composite diapirism and (3) diapirism.

l.472-473 "might be no longer resolved properly" -> "may not be under-resolved to allow for ..."

The suggestion was accepted.

Figure captions:

Fig.2:
"The six panels depict..."
"resolutions" -> "numerical (grid) resolution"

The suggestions were accepted.

Fig.3:
A general statement of what the figure is about is missing, e.g. "Melt ascent morphology as function of compaction length".
"a) Initial conditions of the model valid for all cases apart of the change in compaction length".
"b-j) Melt fraction distribution after ... for length scale ratios varying between 2.4 and 48."
"k) Diapiric rise resulting from a compaction length of zero."
"l) Models' transition time as function of length scale ratios varying between 1.8 and 120. ..."
The figure caption was adjusted according to the reviewers suggestions:
Fig. 3: Melt ascent morphology as function of initial perturbation radius in terms of compaction length. a) Initial conditions of the model valid for all cases apart of the change in compaction length. b-j) Melt fraction distribution after $t' = 0.2$ for length scale ratios varying between 2.4 and 48. k) Diapiric rise resulting from a compaction length of zero at $t' = 9$. l) Models' transition time as function of length scale ratios varying between 1.8 and 120. The transition time gives the time after which the main wave has reached a solitary wave status.

Fig.4:
Consider replacing "show" by "refer to"

"show" was replaced by "refer to".

Fig.5:
"The upper row panels depict..."
"... from left to right, respectively"
"The bottom row panels depict the corresponding ..."

The suggestions were built in:
Fig. 5: The upper row panels depict the solid and fluid mass fluxes of a horizontal line cutting through the maximum melt fraction at timesteps where the main wave has just reached the status of a solitary wave. These timesteps are $t' = 0.02; 0.068; 0.155; 0.416$ from left to right, respectively. The bottom row panels depict the corresponding melt porosity fields. All quantities shown are non-dimensional.

Fig.6:
A general statement of what the figure is about is missing.
"a) Solitary wave (blue) and ..."
"The dashed lines highlight the transition in the regimes."
"b) Ratio of maximum... in the entire model."

The suggestions were built in:
Quantitative parameters as function of initial perturbation radius in terms of compaction length. a) Solitary wave (blue) and diapir (red) partition coefficients for several initial perturbation radii. b) Ratio of maximum fluid velocity to maximum absolute solid velocity in the entire model.

Reviewer 3

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I would like to thank the authors for thoroughly addressing the issues raised in the previous round of revisions. The revised work now clearly demonstrates that the numerical method produces valid and well-resolved results, introduces a more appropriate dimensional analysis to explain the dominant process regimes, and (mostly) avoids interpretations not clearly supported by the model results. As such, the manuscript is now close to a publishable state.

However, a few issues remain to be addressed.

One of the main concern at this stage is the quality and conciseness of language used throughout. I'm afraid, the text is currently of a quality and style of writing not suitable to publication in an international journal. The instances of incorrect grammar, convoluted and repetitive descriptions, and informal or otherwise inappropriate expressions are too many to usefully point out in detail. I strongly urge the authors to apply thorough copy-editing throughout and in places substantial rewriting to the text.

While the early introduction of the concept of compaction versus diapir length scales and segregation versus collective flow speeds is very welcome, I recommend a general rewrite of the Introduction where the broader context and the specific question or hypothesis of the research are more clearly explained, and where the end-member regimes and their contrasting length and speed scales are more systematically introduced and discussed in light of the literature. At present, the text in some places appears incomplete (e.g., no mention that the research is motivated by partial melt transport in the asthenosphere) and in other places disjointed or oddly sequenced.

I recommend the authors consider condensing some of the lengthy and at times repetitive descriptions and discussion of the results. In a number of instances, the authors repetitively point out features that are clearly visible in the figures without adding more information to the discourse. The clarity of characterisation of regimes between compaction wave- and diapir-dominated is obfuscated by repetitive discussion and unclear or inconsistent terminology. Similar arguments are stated multiple times in different contexts, but no clear and consistent terminology is introduced to characterise the regimes or the main metrics used to characterise them. For example, in one instance the authors refer to the diapirism regime as "the solitary wave composed diapiric uprise regime". I believe it should be possible to rewrite the Results section to condense and clarify the observed model behaviour and the following regime classification using concise descriptions and clear and consistent terminology. It would allow the interesting results to be understood more clearly.

Finally, in the Discussion the authors attempt to argue that larger diapirs will generally break up into trains of smaller compaction waves, and that these could be expressed in pulsating volcanic activity. I'm afraid I find this line of argumentation as it is currently stands entirely implausible. Firstly, the authors fail to clearly discuss that their analysis only applies to porous flow of melt within generally partially molten domains at low melt fraction. Previous work has shown that considering more realistic rheologies (e.g., exponential melt-weakening, non-Newtonian or visco-plastic matrix viscosity, decompaction weakening) all lead to substantial modification of melt transport such that the classical picture of compaction waves versus melt diapirs might not apply at all, or at least not everywhere in the partially molten mantle. These limitations should be clearly pointed out and taken into account when discussing implications. Secondly, there are a number of complex processes at play in transferring melt arriving from mantle melt source into the lower lithosphere to volcanic centres at the surface. The ones that come to my mind are subject to their own time and length scales which will likely buffer out or entirely overprint any periodicity of melt transport in the mantle. In my view it is therefore not plausible that the model results presented here have a direct bearing on observed volcanism at the surface. They may still be relevant for some partially molten domains in the mantle or ductile crust, and that is where I would see the main contribution of this work to our present understanding.

The criticism about this paragraph in the discussion is justified and we therefore dropped it.

I further point out a number of minor suggested clarifications and corrections in the annotated manuscript attached to this report.

Tobias Keller (Tobias.Keller@glasgow.ac.uk)

Introduction

The introduction was to large parts rewritten, following most of the suggestions the reviewer made.

L67

The headline was changed to „Methods”.

L74-76

The suggestions of the reviewer were accepted.

L85

The suggestion was accepted.

L86

The notation was adapted and is now following the reviewer's suggestion.

L89

The mixture density is now given by $\bar{\rho}$.

L90

The full definition of the viscous stress tensor as given by the reviewer is now given.

L91

s.a.

L92

Yes, we agree that "bulk viscosity" might not be the best name. But we do not fully agree that compaction viscosity is the best name, so instead we will us "volume viscosity", which should be good name. The term "compaction viscosity" implicates that this viscosity solely affects the compaction, but it is also affected by the shear viscosity.

L151

The definition of the compaction length of the reviewer was accepted.

L158

The term "batch melting" was abandoned.

L164

Yes, omitting the geometric prefactors would make this analysis simpler, but later in the analysis when the switch is calculated these play a role. With prefactors the switch is at a ratio of 48, without at 28. The lines in Fig. 1 get noticeably shifted to the left and no longer reflect the results of our models so nicely. We therefore keep the prefactors.

L164

Thank you for pointing this out. Due to that I could eliminate a typo in this equation. $\frac{v_{sgr}}{v_{st}} \sim \frac{\delta_c^2}{r^2}$ and not the other way around, as stated before. Furthermore, the compaction length is in fact the constant compaction length for the background porosity of our model. Therefore, we should notate that and use δ_{c0}. The typo was eliminated, and the compaction length was correctly notated.

L204

This part of the sentence was erased as it does not help the reader to understand the model setup.

L212

The part about how the tracking coordinate system is carried out was erased due to simplifications stated by the other reviewer. The part about the zooming was referring to our earlier model setup where the perturbation was smaller in the model box. It would most certainly irritate the reader and was erased anyways.

L239

The suggestion was accepted.

L243

It was added that they converge for resolutions higher than 51x51.

L246

The suggestion was accepted.

L273

The sentence was rewritten, following the suggestions of the reviewer:

These semi analytical solutions are in good agreement to our solitary wave models and differ only by 3-5% percent in velocity, as already shown in Dohmen et al. (2019).

L286

The misspelling was corrected.

L295

The misspelling was corrected.

L318

The suggestion was accepted.

L328

The suggestion was accepted.

L343

The misspelling was corrected.

L355

The sentence was rewritten so that it now says that the sign changes, which corresponds to upward flowing matrix:

With increasing radius C_{dia} increases until it changes its sign, and the matrix flows upward, at $r \approx 20 \cdot \delta_c$. It eventually becomes bigger than C_{soli} at $r = 36 \cdot \delta_c$ and then approaches 1 for bigger radii.

L366

“He” was replaced by “they”.

L371

The suggestion was accepted.

L380

The suggestion of the reviewer was accepted and the second regime is now just call “diapirism-dominated regime”.

L385

The third regime was replaced by the endmember of the second regime.

L388

We now state that this is only true in the present model and cite Keller et al. (2013) to give an exception:

In every other case, in the present model, where fluid is able to move w.r.t. the solid, at some point all diapirs will evolve into a swarm of solitary waves which can be infinitely small compared to the initial perturbation. However, this is expected to happen only after a long distance of diapiric rise. In cases where the size of solitary waves is comparable to the perturbation (e.g. regime (1)) this will occur sooner and in cases, where solitary waves are much smaller, later. Their observation is mostly limited by resolution. For models that allow for the diapir to grow (e.g. Keller et al., 2013) they may not dissolve into solitary waves, as it approaches the single-phase limit.

L411

The concerns of the reviewer are correct. The scenario of a magma chamber was abandoned and replaced by “a partially molten region within the mantle”.

L427+428

s.a.

L436

We changed this subsection a bit and changed its title to “model limitations”. We added a small discussion about the internal circulation of diapirs smearing out the emergence of solitary waves and connected the paragraphs to allow for some kind of “story”. The subsection was kept.