Comments by authors to the reviews of “Thermal non-equilibrium of porous flow in a resting matrix applicable to melt migration: a parametric study”

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We thank both reviewers for their thoughtful and constructive reviews. We extensively rewrote the paper according to the comments by the reviewers. In the following we react point by point to the comments which are given in italics.

Review Rudge

1.) … *Reducing the number of dimensionless parameters should allow the authors to greatly simplify their analysis. Instead of the talk of separate variation in the parameters Pe and A, and the Pe-A parameter space, the behaviour can all be boiled down to variations in the single parameter* $\widetilde{Pe}$*. All mention of the separate parameter A can be deleted.*

This is an excellent point! Indeed, we used different lengths for the scaling length and the diffusion length used to define the scaling time. We adopt Rudge’s suggestion and use the diffusion length not only for the scaling time as in the previous version but also for the scaling length *L*. Indeed, we found out that using a modification of Rudge’s scaling length *L* leads to a more elegant non-dimensionalization:

$L = \sqrt{\frac{\phi_0(1-\phi_0)\delta}{S}}$

where ϕ_0 is the melt fraction, δ is the interfacial boundary layer thickness and *S* is the interfacial area density. This scaling length leads to more symmetric heat equations for solid and fluid (new eq. 14, 15), respectively, and to an equation for the fluid to solid temperature difference independent of ϕ_0 (new equation 20)! Thus, for the non-equilibrium temperature difference the problem depends only on two rather than 4 non-dimensional parameters, the Peclet number *Pe* and the non-dimensional initial temperature gradient *G*. The previously used additional parameters *A* and ϕ_0 drop out in equation 20, and *A* drops out of the equations for solid and fluid temperatures (new equ. 4 and 5). We now emphasize that this scaling diffusion length scales with the geometric mean of the pore dimension and the interfacial boundary layer thickness. With the new scaling the heat exchange parameter *A* drops out. However, our calculated numerical models are not obsolete, because the previous variation of *A* and ϕ_0 maps into a variation of *Pe* and *H*, where *H* is the thickness of the model. Indeed, in the original paper we had 4 non-dimensional parameters, *Pe, A,* ϕ and *H* and focused on the *Pe – A* space. With the new scaling, only three of these four parameters are independent, and for the temperature difference only two are independent. Following his suggestion we dropped *A* everywhere and focus on the $Pe - G \ (= \frac{1}{H})$ parameter space. The three different regimes seen in the previous version show up again as three modified regimes. Where necessary we have rerun most models with the new scaling. We dropped all models with $A \neq 1$ but included some important cases with various *H.* Only for section 5.2 (Time scales) we still use the results of the old models mapped to new Peclet numbers and thicknesses *H* but added a number of new parameter combinations.

2. It's not clear to me why the authors chose the boundary conditions they did, and I think these boundary conditions would benefit from a little explanation of how they relate to the Earth problems they're interested in. The authors describe their boundary conditions in lines 145-150. The conditions are a fixed temperature for both phases $T = \Delta T_0$ *at the base of the column, and a fixed conductive heat flux* $\frac{\partial T}{\partial z} = -\Delta T_0/H$ *at the top. On line 148 the authors have missed a minus sign (temperature decreases with increasing height), and the authors are speaking too loosely by calling the top condition a constant ux condition. Because the melt is moving, there are two mechanisms of heat transfer out of the domain: the conductive (or diffusive) heat loss and the advective heat loss due to the moving fluid. The author's top boundary condition fixes just the conductive part. The authors should be clearer in talking about the advective and conductive heat losses, and provide some physical justification for their chosen boundary conditions. The same comment applies to line 331.*

Good point. We now introduce the boundary condition at the top more rigorously, and address the advective flux. We justify the chosen boundary condition being appropriate except for late stages. In chapter 5.1.2 we extend the discussion about the limitations, and test three alternative boundary conditions, including an open boundary. We also justify that the desired temperature differences are well given by the values at the boundary despite the discussed limitations.

3. In the final discussion, I think it is worth writing some of the key results back in a dimensional form to make them more accessible. For example, in the cases where a constant temperature gradient in the solid is a good approximation, the key result is that a dimensional thermal disequilibrium of

$$T_f - T_s = -\frac{\phi(1-\phi)\delta v G}{\kappa S} \qquad (7)$$

develops, where G is the background temperature gradient. The characteristic time scale for the pore-scale thermal equilibration process is

$$\frac{\phi(1-\phi)\delta}{\kappa S} \qquad (8)$$

Equations (7) and (8) are nice simple equations that can be used to get ball-park estimates of the expected thermal disequilibrium for geological problems.

Nice suggestion. We now include an appropriate section at the end of the application section.

Minor points

1. I think it's best to have a single letter variable, such as δ*, representing the boundary layer thickness, as the author's choice of dm could be confused as a product of two other variables.*

Agreed and changed

2. Line 232. The authors refer to “this surprisingly good agreement" between their numerics and the analytical approximation. I don't think they should be that surprised by the agreement! The analytical approximation is exploiting the large separation in time scales in the problem, with a fast timescale for pore-scale thermal equilibration, and a slow timescale for heat to be advected across the whole domain. The approximation could be formally justifed by a multiple-scale analysis.
We replaced “surprisingly” by “very good”. But we cannot confirm that the large separation of time scales is the reason. Some tests with similar or reversed timescales (Pe =1, H = 1 or Pe = 0.1 and H=1, respectively) give similarly very good agreement (not included). We don’t carry out a multiple-scale analysis.

3. I don't understand the difference between the channels and dunite system marked in Figure 4. To me, the dunite system is a manifestation of channels.

O.k., we distinguish between the onset of channeling and late stages. This is done in the text and we now write “early channels” in Fig 4.

4. Line 9. “by Darcy flow" - I'm not sure if that is a correct statement. Darcy's law is never used in the manuscript; a constant melt upwelling velocity is simply prescribed. Perhaps delete “by Darcy ow" here.

Agreed. Replaced by “porous flow”

5. Line 56. As the authors note, these two-temperature equations have been very well studied in the engineering literature. It may be worth also citing some of the work of Kuznetsov (e.g. Kuznetsov 1994 Int J. Heat Mass Transfer 37 5050-5055) and Spiga and Spiga 1981 Int J. Heat Mass Transfer 24 355-364.

Thanks for the suggestions, included now.

6. I think the presentation of the heat equations can be streamlined a little. I'd start by writing (4) and (5) and not write (1), (2), or (3). It may be worth noting that these energy equations are for constant pressure.

We prefer to start with (1) – (3) because then it is clear that the equations still allow for variable material properties (even c_p !). We mention the variable properties now (not c_p explicitly) and mention the constant pressure.

7. Figure 2. It would be good to have some indication as to what times the different lines in panel a correspond to. Perhaps add a legend?

We have redrawn the figure completely with the new scaling and added the times

Typos

Typos corrected and English improved.

Review Wilson

1.) Scaling

As John pointed out the number of parameters could be reduced with a redefinition of the length-scale L0. This would have a huge impact on all the subsequent sections, making the conclusions much more tractable. I would encourage the authors to take a small extra step and redefine the length-scale such that:

$$L_0{}^2 = \frac{\delta}{S} \tag{1}$$

where I have adopted John's notation and used δ*= dm and have dropped the* φ *dependence. The equations then become:*

$$\frac{\partial T_f}{\partial t} + Pev \cdot \nabla T_f = \nabla^2 T_f - \frac{1}{\varphi}\left(T_f - T_s\right) \tag{2}$$

$$\frac{\partial T_s}{\partial t} = \nabla^2 s + \frac{1}{1-\varphi}\left(T_f - T_s\right) \tag{3}$$

with controlling parameters:

$$Pe = \frac{L_0 v_0}{\kappa_0} = \sqrt{\frac{\delta}{S}}\frac{v_0}{\kappa_0} \qquad \textit{and} \qquad \varphi \tag{5}$$

This has the small additional appeal of removing the φ*-dependence from Pe (or A) and having independent controlling parameters.*

This is an alternative scaling, but still the length scale is implicitly dependent on φ because *S* depends on φ. As we want to use melt channels, tubes or melt films as typical melt geometries one can write $S = c\,\varphi/d_f$ with d_f as melt pore dimension and *c* =2 or 4 for channels, films or tubes. Motivated by your and Rudge's suggestions about scaling we came up with another more elegant alternative which makes the two heat equations for solid and fluid more symmetric with respect to each other, and which leads the porosity to drop out the equation for the temperature difference. See also our reply to comment 1 by Rudge. With this scaling the length scale is essentially equal to the geometric mean of δ and d_f or d_s which Rudge calls a natural length scale for thermal melt equilibration. We make this point in the revised manuscript.

2.) Boundary Conditions

The top boundary condition is frequently referred to as a constant flux condition but it appears to only set the diffusive part of the condition and ignores any discussion of the advective component. This has large implications for the behavior at the top of the domain, where most of the discussion is focused because it is where the largest (or most persistent) non-equilibrium conditions are seen (at least in the case with a linear initial condition). The persistence of the disequilibrium is a product of the boundary conditions however, with the solid temperature being prevented from further equilibrating with the fluid by its flux being fixed. I was a little confused by the physical motivation of this boundary condition where fluid is (presumably) allowed to escape but the solid temperature gradient is fixed and would like to have it *discussed or justified more at its introduction. The authors do later vary the boundary condition in the discussion and supplement but again don't offer physical motivations for the options and again don't discuss the advective flux component*.

Good point. We now introduce the boundary condition at the top more rigorously, and address the advective flux at its introduction. There we justify the chosen boundary condition being appropriate except for late stages. In chapter 5.1.2 we extend the discussion about the limitations, and test three alternative boundary conditions, including an open boundary. We also justify that the desired temperature differences can safely be picked by the values at the boundary despite the discussed limitations.

3.) Analytical Justification

In section 4.1 the authors simplify their model to seek the temperature difference around a fixed solid temperature profile. The description of this simplification could be improved. For a start I believe the solid temperature gradient has a sign error on line 210 $\partial T_S/\partial z = -\Delta T_0/H$ *(the subsequent equations are correct) and, as far as I could see,* ΔT *has not been defined at this point.* ΔT_0 *was previously used for the temperature drop across the domain (and* ΔT_{max} *for the peak non-equilibrium temperature difference). Beyond that more clearly stating that this fixes the solid temperature gradient and seeks a solution in this state would make reading this section easier (the authors do try to clarify that the steady state behavior they are seeking isn't the same as that in the "full" model).*

Thanks! We corrected the sign error and dropped $\Delta\mathrm{T}$ which was not defined, indeed. We don't mix up dimensional and non-dimensional quantities in that section anymore. At two places in section 4.1 we added "fixed" to the temperature gradient in the solid.

4.) Discussion

The discussion section rigorously describes a number of different scenarios and variations on the previous results. It covers an impressive range of topics though section 5.1.5 seems redundant and could be removed. It would be hugely enhanced by reducing the complexity and only considering a two (with presumably only one important) parameter model. Currently a full understanding of the section depends on several figures that are only available in the supplementary material, which are not particularly well captioned. If it were possible to improve the labeling and move them or a subset of them to the main text that would enhance the discussion significantly. In particular, though the color coded time evolution works well when lines are close together, it fails to clearly show the order of the solutions once significant jumps occur between them. It would also be good to know what (non-dimensional) time-ranges are being plotted in all figures.

Agreed: we dropped section 5.1.5. As with the new scaling the quantity *A* disappeared , sections 4.3.1, 4.3.2, 5.1.4 could be dropped. From the supplementary material sections sections 1.4, 2.1, 2.2, and 5 have been dropped. The remaining figure captions are improved. The Figures have been redrawn with better coding the times of the curves (starting curve is bold now), and all times are mentioned in the captions now. We moved all figures but one from the supplementary material to the paper.

5.) Minor Points

There are quite a few small typos throughout the paper (such as extra or missing spaces) that should be corrected. Below I just list a few things that caught my eye:

. line 72: “allowing easily to decide" → “that allow the easy determination of"? done

. start of section 2.1: a constant porosity and fluid velocity are already stated here but aren't used in the following derivation, instead being repeated at the end of the section (where they are used) o.k. done

. line 86 and throughout where appropriate: “resumes to" → “becomes" done

. equation 9a: κ_0 *is introduced but isn't defined until equation 11 and even then is related to* κ_f *and* κ_s*, which also haven't been introduced before. I think a statement that* $\lambda_f = \lambda_s = \lambda_{eff}$ *might be necessary first.* We refer to Table 1 and explicitly state equal thermal properties done

. line 134: “is to emphasized" → “is to be emphasized" done

. start of section 2.3: the domain height, H is introduced before it is used, perhaps consider adding to the first sentence: “are solved in a 1D domain of height H" done

. line 148: sign error in “flux" boundary condition done

. line 152: extra space in “R2018b)" done

*. lines 169-171: the sentence “As the fluid temperature increases" could do with rewriting for clarity.*done

*. line 177: “boundary conditions are applied" → “boundary conditions at z = H are applied"*done

. line 210: sign error in solid temperature gradient and ΔT *introduced without definition* done

. line 224: missing space between “for" and “$(T_f - T_s)$*"*done

. line 231: “For 91% of the models" - does this include those models with a different initial condition or those with varying material properties? no, for these no analytical solution exists. Reformulated. done

. line 245: I think “complex" could be confusing here, maybe try “complicated" done

. line 273: “L'Hospital" → “L'H^opital" done

. line 287: extra space after “dykes" done

*. line 315: “onm" → “on"*done

*. line 340: “As initial condition" → “As an initial condition"*done

. section 5.1.4: redefining A for this section is potentially a little confusing. Hopefully A is no longer necessary anyway but, if not, perhaps in earlier sections the constant version could be A_0 *so that the variable wouldn't have to be re-used.* obsolete

. section 5.1.5: this seems redundant and could be removed done

. line 428: it might be nice here to clarify that this is the steady state of the full model, not the analytic model. The analytic model is independent of time. Added “numerical model” done

. supplementary material line 23: this looks like it's the solution of Eq. (19) from the main text not the homogeneous Eq. (S2) done

. line 46: “into Taylor series" → “into a Taylor series" obsolete, done

. Figure S4 caption: spacing and punctuation errors done