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 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 Butler

1.) I have two substantive comments regarding the content of the paper. The first is that I am surprised that the authors do not show any analyses of the heat flow in their model. If nothing else, the authors could show the overall energy balance in their model by showing the incoming and outgoing advected and conducted heat flows at the top and bottom as well as the change in internal energy in the solid and fluid. This would serve as a check on energy conservation in their model but analyses could also be done on smaller scales looking at heat fluxes between the solid and fluid as well as heat fluxes due to advection and conduction. This would serve to help explain the cause of nonequilibrium in their models and I think would add significantly to a reader's understanding.

We now include two extra panels in Fig. 2 showing the three contributions of the heat fluxes demonstrating the importance of advective heat flux by the fluid. Based on these new figures we add a short statement about the neglect of conductive terms which has been done in earlier papers. I am not so sure whether we can learn much more from more extensive heat flow analyses, so this should be sufficient. As for the heat balance, yes, we tested it and make a short statement in the Numerical scheme section. Fig. 1 below shows the global heat balance which is of error order 1 (due to upwind). However, we think it is not necessary to include it in the paper.



Figure 1. Left: Incoming heat flux (conductive + advective) at bottom, outgoing heat flux (conductive + advective) at top, and change rate of internal heat (Del-q) as a function of time of model 1. The different resolutions plot essentially on the same curves. Right: Error of the relative heat balance of these contributions with different resolutions corresponding to 101, 201, and 401 grid points.

2.) My second substantive point regards the length scale in the model. As the authors show in equation 9a, the length scale is (phi delta ds/cs)^0.5 where phi is porosity, delta is the thermal boundary layer thickness, ds is the grain size and cs is a constant of order 1. Since the thermal boundary layer thickness will be small compared with the size of the system for large Peclet numbers and the grain size is very small compare with the size of the system, L will also be very small compared with the size of the system. In estimating G for a mid-ocean ridge the authors get values of 10^-6-0.6 for various settings. Yet, when investigating their models (figures 2-9) the authors only show values of G as low as 0.01. Values of z can only be as large as 1/G. I think that this issue with the length scale should be discussed.

Perhaps this concern arises from the understanding that " d_s is the grain size". In our model we introduce and understand d_s as the average distance between melt filled pores or channels which can be considerably larger than the grain size. Then both delta and d_s can reach some considerable fraction of the system. The same applies for the length scale *L*. We add this statement after introducing the length scale. At various relevant places we indicate that d_s is the grain size *or* channel spacing. In the discussion section 5.3 we clearly introduce d_s as grain size or channel spacing and review values from grain sizes up to channel distances of 300 m. From all these number we obtain the regimes plotted in Figure 4 and everything is consistent. Of course, the regimes extend far into the region of G< 0.01, which we now emphasize more clearly.

3.) Also, as shown in figure 4, the degree of disequilibrium is proportional to G in all cases. In Regime 1, DeltaTmax is G*z where G is small, the maximum value of z will be large. However, in Regimes 2 and 3, when G is small, unless Pe is very large, the degree of disequilibrium is likely to be very small based on the scaling laws in these two regimes since (1-exp(-z)) and M(z) are both of order 1,

I don't fully understand the concern. If you are concerned about the fact that the proportionality to *G* given in the formulas is not equally visible in the three regimes, here some explanation: In regime 1 one does'nt see the proportionality to G because increasing G decreases the maximum value of z which is equal H = 1/G cancelling the G-proportionality. In regime 2 (1-exp(-z)) is of order 1 indeed, and one clearly sees the proportionality to G, in regime 3 M is both a function of G and z, and the proportionality to G gradually disappears. I added two sentences in the text along these explanations.

4.) Issues marked in the text

Thanks, changes are applied appropriately.

4.1) Time scales: I think that the most important question in application to the real earth is "How large is the degree of disequilibrium likely to be?" This has implications for whether the solid rock will melt or the liquid will freeze. Since the authors have calculated a dimensionless DeltaTmax, they should also be able to estimate a magnitude of the temperature disequilibrium.

You are right, that is the main point of the whole paper, and we did'nt give a number. In the section on application to magmatic systems we add a paragraph in which we estimate the degree of thermal non-equilibrium in Kelvin for our example of mid-ocean ridges.

Review Wilson

1.) Non-dimensional Numbers

The non-dimensional parameters controlling the system has now been reduced to Pe, φ and G and only Pe and G in the analytical case. However, models are still presented in terms of H. Even though this is just the reciprocal of G it would be great to use a single labeling system. G and H are also listed in table 1 as dimensional numbers but, I think, only discussed in their non-dimensional form (except maybe when discussing the scale length L). Even though primes are explicitly dropped this might be a little confusing.

Fair enough, but as we cannot abandon *H* completely, we replaced *H* by *G* where possible, but keep *H* where explicitly the height of the model is addressed. Also, the labeling in Fig. 6 has been changed in favor of *G*. In Table 1 we added "mostly non-dimensional" for *G* and *H* and put the dimensions is parantheses.

2.) Initial & Boundary Conditions

The dependence on G' (or H') really emphasized to me how dependent these solutions are on the initial and boundary conditions selected (I guess most problems are!). Though some discussion has been added to section 2.3 about the need to specify boundary conditions in general, this isn't really the kind of physical justification for this specific problem that I was hoping for. This model imagines an initially stagnant fluid in thermal equilibrium with a solid with a (Cartesian) conductive temperature profile. The fluid then moves and while the upper boundary allows advective outflow of heat in the fluid it maintains a fixed thermal gradient in both the solid and fluid while doing so. (All of this is much more clearly stated in the updated manuscript, thank you.) The analytical model then seeks the maximum thermal disequilibrium, which occurs in the early stages of the evolution of the model and at the top of the domain. I understand that these decisions were made to make the analytical solution possible but if there is a physical scenario in which this is likely then it would be great to discuss it in section 2.3.

The authors demonstrate numerically the effects that different initial and boundary conditions might have in section 5.1.2. This only seems to consider applying the same conditions to both the solid and fluid temperature. Have the authors considered applying different conditions to the two fields? This may make a more physical scenario possible. For example the fluid temperature could use the open boundary condition, mimicking fluid escape, while the solid could use a Robin condition relating the gradient at the top boundary to an imagined crustal conductive profile above the domain depending on the temperature at the top boundary, e.g. $\frac{\partial T}{\partial z} = \frac{T_{surf} - T}{h_{crust}}$. Admittedly this would introduce an extra parameter for the "crust" thickness h_{crust} and the surface temperature T_{surf} .

Good point, thanks. We did'nt think of applying different boundary conditions to the fluid and solid. We now include this possibility in section 2.3 as a possibility, particularly the suggested open condition for the fluid and the Robin lid condition for the solid. Yet I am not sure whether such a Robin condition is physically better in view of relatively rapid temperature variations: It is unphysical to instantaneously change the slope within the complete lid. The temperature in the imagined lid can only vary on the diffusive timescale of the lid, which is much longer than all time scales in our model if the lid is thicker than *H*. We have done some models with the proposed boundary conditions (open for the fluid, Robin lid

for the solid) and include a paragraph describing that for thick lids (> *H* as supposed for our scenarios) the temperatures evolve generally similar to our flux condition. And we explain the differences. In Fig. 2 here we show this series of models, but don't want to expand the paper even more by including another figure with a fifth boundary condition.



Fig. 2. Models with an open boundary condition at the top for the fluid and a Robin boundary condition for the solid representing an imagined lid of thickness H_{lid} . The columns show models with same parameters as those of Fig. 6 in the paper. The rows contain models with different lid thicknesses.

3.) Time Evolution & Steady State

I appreciated the authors' efforts to clarify the time evolution in the figures by giving some time increments and distinguishing the lines a little. In many cases the total time was listed for all models. Could the authors confirm that in the time evolution figures (e.g. Figure 6) the steady state solution (as opposed to just the solution at an arbitrary final time) is included in all subplots?

We confirm that all models with G = 0.1 have been run close to steady state, which is reached at about t = 100, while the cases with G = 0.01 have not. They would need to be run until t = 10000, i.e. 10 to 20 times longer than we did. We don't think it is necessary to rerun those models because we don't learn too much about thermal non-equilibrium which is reached much earlier during stage 1. We include statements about reaching steady state or not where appropriate.

4.) Even More Minor Points

There are still quite a few small typos throughout the paper that should be corrected in the editing phase. Below are a few things that caught my eye:

- line 31: "On small scale" → "On a small scale"
- line 67: "or more general" \rightarrow "or more generally"
- *line 113: Perhaps introduce a new paragraph after "Primed quantities are non-dimensional."?*
- *line 179: is the second statement about resolution only necessary for a few of the numerical cases presented in figure 3?*
- equations 21, 22, 23: G appears inconsistently italicized in these three equations (see line 331 also)
- equations 32 & 33: the non-dimensionalized prime is placed inconsistently throughout the variables in these equations, sometimes as a subscript and also as a superscript
- table 1, last line: "reference. respectively" → "reference, respectively"?
- figure 3: could figure S1 be incorporated into figure 3a to avoid having to reference a supplemental figure? Also, while figure 3 is necessary to show the numerical results could it reference figure 4 (and vice versa) as it (I believe) represents slices through the latter?

Thanks, we have included all corrections and suggestions. As for eq. 32 & 33, they are all superscripts. Hope this will be correctly displayed after editing.

Another point not noted by the reviewers but by the authors

In section 5.1.3 the formulation addresses different physical properties of the two phases. In the previous version we stated that this formulation adds three new non-dimensional numbers. In fact, modifying the scaling by combining the non-dimensional effective thermal conductivity with the length scale introduces a new alternative Peclet number and reduces the number of new non-dimensional numbers to two rather than three. Thus, equations (32) and (33) are now rewritten in terms of this modified scaling. The shown models and discussion are not affected as we assume $\lambda_{eff}' = 1$ so that the new and old Peclet numbers are identical.