Review of "Thermal non-equilibrium of porous flow in a resting matrix applicable to melt migration: a parametric study"
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This manuscript presents a simple 1D model of the thermal equilibration of a fluid rising at a uniform velocity through a porous medium. Such models have been extensively studied in the engineering literature, but may be less familiar to the readers of Solid Earth. The authors apply their model to quantify the likelihood of thermal disequilibrium between melt and solid during different stages of melt ascent through the Earth's mantle: from diffuse porous flow, to channelised flow, to rapid ascent in dykes. Their main conclusions are perhaps not surprising: diffuse porous flow is in thermal equilibrium, dykes are not, and channels are somewhere in between. Indeed, this was also one of the main conclusions of a recent 2018 paper by one of the authors (Schmeling et al. 2018 GJI). The simple model presented here leads to some useful analytical expressions for quantifying thermal disequilibrium in geological systems. However, I think the presentation of the theory at the moment is somewhat overcomplicated, and a more streamlined revised manuscript could be produced.

Major points

1. The analysis of the thermal problem can be significantly simplified. At the moment, the results are presented in terms of four dimensionless numbers: Pe, A, ϕ, and H. In fact just three dimensionless numbers are sufficient to describe the system: a modified Peclet number $\widetilde{Pe}$, a modified dimensionless domain height $\widetilde{H}$, and the porosity ϕ.

   The authors in (8) choose to make lengths non-dimensional on L_0, which is a characteristic channel width of the pores. However, the equations are much simpler if one instead chooses to non-dimensionalise on a scale L defined by

   $$L = \sqrt{\frac{\phi\delta}{S}} \tag{1}$$

   where δ is what the authors term the interfacial boundary layer thickness dm and S is the interfacial area density. L is the natural lengthscale associated with thermal equilibration of melt pores. With time scaled by the diffusion time

   $$\tau = \frac{L^2}{\kappa} = \frac{\phi\delta}{\kappa S} \tag{2}$$

the dimensionless equations become

$$\frac{\partial T_f}{\partial t} + \widetilde{Pe}\frac{\partial T_f}{\partial z} = \frac{\partial^2 T_f}{\partial z^2} - (T_f - T_s) \tag{3}$$

$$\frac{\partial T_s}{\partial t} = \frac{\partial^2 T_s}{\partial z^2} + \frac{\phi}{1-\phi}(T_f - T_s) \tag{4}$$

where the modified Peclet number and dimensionless domain height are given by

$$\widetilde{Pe} = \frac{vL}{\kappa}, \qquad \widetilde{H} = \frac{H}{L} \tag{5}$$

where H is the dimensional height of the domain and v is the dimensional velocity of the upwelling melt. (3) and (4) are simpler than the authors (15) and (16) in containing just $\widetilde{Pe}$ and ϕ as dimensionless parameters, rather than the Pe, A, and ϕ that the authors have in (15) and (16). The authors' (15) and (16) relate to (3) and (4) by the transformation

$$\widetilde{Pe} = \frac{Pe}{A^{1/2}}, \qquad z'' = A^{1/2}z', \qquad t'' = t' \tag{6}$$

where the double primes refer to the non-dimensionalisation scheme used in this review, and single primes to that used by the authors.

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.

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 flux 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.

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

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

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

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

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.

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.

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 justified by 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.

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 flow" here.

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.

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.

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?

Typos

1. Line 121. Besides is not right here.

2. Line 273. L'Hôpital not L'Hospital.

3. Line 315. onm

4. Line 373. affected not effected

5. There are a number of places where the English phrasing is a little odd, and the manuscript could be benefit from some additional proof-reading.