

# Review of “Assessing the role of thermal disequilibrium in the evolution of the lithosphere-asthenosphere boundary: An idealized model of heat exchange during channelized melt-transport”

Harro Schmeling

March 29, 2022

In this manuscript the thermal effect of melt infiltration into the base of the continental lithosphere is studied focusing on the thermal disequilibrium between melt and ambient rock. While thermal disequilibrium in porous flow is well studied in more technical literature, only a few papers quantitatively addressed this effect in the recent geoscience literature. Therefore this paper is timely and new. It is shown that indeed thermal disequilibrium may be important under certain circumstances near the lithosphere asthenosphere boundary explaining some observational data. Useful timescales and length scales are provided and are applied to observations. I recommend publication after some revision.

## Major points

1.) A major problem seems to be the neglect of conductive heat flux, i.e. the diffusion terms which are missing in eqs. 1 and 2. This is an assumption which stems from the old Schuman paper (1929), and is subsequently applied, e.g. by Spiga and Spiga (1981). Spiga and Spiga (1981) keep the diffusion term in their non-dimensionalized equations (but neglect it later) which reads in the notation of Roy:

$$\frac{\lambda_f k}{\phi c_{pfluid}^2 \rho_{fluid}^2 v^2} \frac{\partial^2 T_f'}{\partial x'^2} \equiv C \frac{\partial^2 T_f'}{\partial x'^2} \quad (1)$$

where the primes denote non-dimensional quantities. This term and a similar term for the solid temperature needs to be added to the right hand sides of equations B1 and B2. Given some temperature variations with a certain non-dimensional wavelength  $\lambda'$  one can actually check the magnitude of this term with respect to a) the advective term  $\frac{\partial T_f'}{\partial x'}$  or b) the heat exchange term  $T_f' - T_s'$  in eq. (B1). Typically  $\lambda'$  is of the order 10 to 20 according to Figure A2 or A4 or shorter at earlier times. Decomposing the temperature into Fourier series we may insert typical temperature modes  $T_f' = T_{f0}' \sin \frac{2\pi x'}{\lambda'}$  into eq. B1 with the added diffusion term, where  $T_{f0}'$  is the time-dependent amplitude of the Fourier mode. Evaluation of the resulting equation shows that neglect of the diffusive term with respect to a) the advection term or b) the heat exchange term requires

$$\text{a) } C \frac{2\pi}{\lambda'} \ll 1 \quad \text{or b) } C \left( \frac{2\pi}{\lambda'} \right)^2 \ll 1 \quad (2)$$

Inserting minimum and maximum numbers of used parameters from Table A1 of the manuscript shows that conditions (2) are severely violated. Only for the most extreme parameter combinations of  $k = 10^{-5} \text{ W/(m}^3\text{K)}$  and  $v = 100 \text{ mm/yr}$  the left hand sides on (2) are of order 0.1, in all other parameter combinations they are larger and violate the conditions. One can also write  $C$  in terms of Peclet numbers based on the length scale  $d$ :

$$Pe_{\kappa_f} = \frac{v d}{\kappa_f}, \quad Pe_{\kappa_{eff}} = \frac{v d}{\kappa_{eff}} \quad (3)$$

where  $\kappa_f$  or  $\kappa_{eff}$  is the thermal diffusivity of the fluid or the effective interfacial boundary layer, respectively. Both Peclet numbers are probably very similar and can be merged to a characteristic Peclet number  $Pe$ . Using the equations given in the paper the relations (2) can be rewritten as

$$Pe \gg \sqrt{\frac{A(1-\phi)}{\phi} \left(\frac{2\pi}{\lambda'}\right)^n} \quad (4)$$

where  $A$  is a constant defined in the paper (2, 4, or 6) and  $n = 1$  based on relation (2 a) or  $n = 2$  for relation (2 b). From eq. (4) it follows that the neglect of the diffusion term is justified for Peclet numbers of order 1 to 10 and larger, while the Peclet numbers used in the paper are between  $3 \cdot 10^{-6}$  to 0.3.

Therefore I strongly recommend to include the diffusive term into the calculations of thermal non-equilibrium and rerun the models. Note that due to numerics you probably have some numerical diffusion in your model which may be of similar order as the neglected diffusion term. Thus, you should do some resolution tests.

2.) The critical parameter is the heat transfer coefficient  $k$ . Already in Fig. 1  $k$  is given as proportional to  $(1 - \phi)/d^2$  where  $\phi$  is the porosity and  $d$  is the channel distance. From the physics point of view it is proportional to  $(1 - \phi)/(d \delta)$  where  $\delta$  is the microscopical thermal boundary layer thickness at the solid-fluid interface (Schmeling et al., 2018). Only for long period thermal variations  $\delta$  is of the order of  $d$ .

2.1) In Appendix C the heat transfer coefficient is discussed in more detail. There seems to be a confusion about the constant  $A$  in the equation for the specific surface area  $a_{sf} \approx A(1 - \phi)/d$  (note that you should use the symbol “ $\approx$ ” or “ $\cong$ ” as “approximately equal” and not “ $\sim$ ” as “proportionally” as you do correctly in the notation of  $k$  in Fig. 1.). A back-of-envelope calculation results in  $A = 2$  for planar channels, while for cylindrical tubes it is more complicated if written in terms of  $d$  (the formula contains square roots of  $\phi$ ). Instead, for cylindrical tubes it can be written as  $a_{sf} \approx 4\phi/d_f$  where  $d_f$  is the tube diameter. But correctly, it is 6 for spherical or other grains embedded in the fluid phase. In Chevalier and Schmeling (2022) we discuss some of these relations.

2.2) In eq. C1 and C2 the minus sign should be replaced by a plus (Dixon and Cresswell, 1979, eq. 29; Stuke (1948), eq 57). For  $\beta$  values of 10, 8, 6 are assumed for spherical matrix grains, cylinders or slabs, respectively. Adopting Dixon and Cresswell’s arguments means that short period effects (higher temporal modes as considered in Stuke, 1949) are neglected. This results from their assumption of taking Stuke’s (1949) heat transfer coefficient (eq. 57 in Stuke 1949) with  $\Phi = \frac{1}{\beta} +$  higher temporal orders but then neglecting these higher orders. With this assumption you get the effective conductance (your eq. C2). In my understanding, accounting for these higher orders is physically equivalent to taking the effective thermal conductivity  $C_{eff}$  and then defining the effective conductance by  $C_{eff}/\delta$  where  $\delta$  is the microscopical thermal boundary layer thickness. By neglecting the short term higher orders one implicitly assumes that the thermal boundary layer thickness has reached the order of  $d$ . Only then the appropriate  $k$  is given by  $C_{eff}a_{sf}/d$ . In other words, in your choice of  $k$  you underestimate short term interfacial heat exchange. The problem with choosing  $\delta$  rather than  $d$  in estimating the effective conductance is that  $\delta$  is time-dependent, and theoretically includes the full thermal history of the two-phase flow. In Schmeling et al (2018) we studied this effect in detail and showed that choosing  $\delta = d$  describes the thermal non-equilibrium only for intermediate term evolutions, not short period thermal variations (e.g. Fig. 8 in that paper). For  $\delta < d$  the heat transfer coefficient  $k$  will be larger than yours, so you probably overestimate thermal non-equilibrium for short term thermal variations. My recommendation: As it is quite common in literature to use the  $\delta = d$  assumption for simplicity you should keep this assumption and address and discuss this point.

3.) You don’t say how you solve the equations. Please add a short section on the numerical method, grid resolution etc.

4.) The Appendices D and E contain very interesting model results. In my opinion they should be moved to the main text.

5) Discussion. In section i) you introduce the term “disequilibrium heating”. This term should more rigorously be defined. In this section (e.g. Line 208) you estimate the heat budget due to disequilibrium heating by multiplying the excess infiltration temperature  $\Delta T$  by  $k$  to get a volumetric heat generation rate. According to eq. (2) you should use the disequilibrium temperature difference  $T_f - T_s$  rather than  $\Delta T$ , to get an estimate of the contribution of disequilibrium heating. From your figures 2, A2 and A3  $T_f - T_s$  is of the order of  $10^{-2}$  to  $10^{-1}$  of  $\Delta T$ . These temperature differences are also consistent with the estimates of the magnitude of thermal non-equilibrium by Chevalier and Schmeling (2022) for Peclet numbers of order 0.1. Thus the estimated LAB heat budget in line 208 should be one to two orders of magnitude smaller. Yet, the advective heat transport by infiltration flow in thermal equilibrium might be larger and would be nice to be estimated here. Perhaps these estimates do fit better to the estimates of the heat released by crystallization you estimate in line 222.

6) Line 258, 260. Here you speculate about rheological weakening due to disequilibrium heating. Again, assuming 100 K as a possible temperature increase is a probably an overestimate given that the disequilibrium temperature difference  $T_f - T_s$  is one to two orders of magnitude smaller than  $\Delta T$ . And: I have checked the activation energies and volumes of Hirth and Kohlstedt (2003) and I don't get your factors of order 1/62. I get something like 1/20 at most for constant stress, and 1/3 for constant strain rate. Given the smaller temperature difference of order 10 K reduces this effect even more to a factor 1/1.3 or something like this, which is still worthwhile to mention.

### Minor points

7.) Line 308: you may note here that  $1/z$  is the dimensionless channel velocity (but see also comment 13).

8.) Line 334. Are  $\phi_{in}$  and  $\phi_{out}$  identical to  $\phi_f$  and  $\phi_s$ , respectively? Then you should use same symbols.

9.) Line 337. You choose  $A$  and  $\beta$  independently, but they are geometric parameters for spheres, tubes and spheres. Particularly  $\beta$  is defined for solid spheres, cylinders and plates, while  $A$  is defined for fluid tubes, etc.

10) Line 340 to 345 or section 2: Please specify the boundary conditions more rigorously, for both  $T_s$  and  $T_f$  at  $x = 0$  and at the other side of the domain. You should clearly state that  $T_s'$  is also raised to 1 while you increase the influx temperature of  $T_f'$ .

11.) Line 363. Delete “migration”

12.) Line 366 – 367 and line 143 – 149. The difference between the disequilibrium front velocity of Kuznetsov (1984) and your eq. 3 is puzzling and should be discussed. Is it due to different scaling? Although both, Kuznetsov's and your eq. 3 are given as dimensional equations? Or is it an effect of using perturbation theory versus full solution of the PDE's? Or is it a misprint in Kuznetsov? Anyway, how did you derive and justify eq. (3)?

13.) Fig. A2c causes confusion. From the x-label or figure caption we have

$$x'_{front} = \frac{1}{z} t'. \quad (5)$$

This implies that the disequilibrium front has the non-dimensional velocity  $1/z$ . But the fluid velocity may be written as

$$v_{channel} = \frac{x_f}{t} \quad (6)$$

where  $x_f$  is the position of a fluid particle. If we substitute  $x_f$  and  $t$  using the non-dimensionalization rules one gets

$$v_{channel} = \frac{x'_f v_{channel} k_s}{t' k_f} = \frac{x'_f}{t'} v_{channel} Z = v'_{channel} v_{channel} Z \quad (7)$$

with  $v'_{channel}$  as non-dimensional fluid velocity. After elimination of  $v_{channel}$  from both sides we have

$$v'_{channel} = 1/Z \quad (8)$$

which is in contradiction to eq. (5). Can you help me (and potential readers)?

14.) Line 383. Sentence strange, probably delete one of the “is” or insert “which”

15.) Line 390 – 391. Which “blue lines”? Do you mean the dashed lines or the double arrows?

16.) Line 391: “wavelength” probably to be replaced by “period”

17.) Line 149. I don’t see the strong function of  $k$  in Fig. A4.

18.) Line 150 – 159. You clearly describe the exponential decay of disequilibrium. Could you elaborate a bit on the decay rate for the step function case?

19.) Line 163: delete one of the parantheses “)” in the first *tanh* term

20.) Conclusion: Here I suggest to repeat the meaning of the abbreviations CLM, TRZ again

## References

Chevalier, L. and Schmeling, H.: Thermal non-equilibrium of porous flow in a resting matrix applicable to melt migration: a parametric study, Solid Earth Discuss. [preprint], <https://doi.org/10.5194/se-2021-149>, in review, 2021.

Dixon, A. G. and Cresswell, D. L.: Theoretical prediction of effective heat transfer parameters in packed beds, AIChE Journal, 25, 1979.

Hirth, G. and Kohlstedt, D. L.: Rheology of the Upper Mantle and the Mantle Wedge: A View from the Experimentalists, Geophysical Monograph Series, 138, 83–105, 2003.

Kuznetsov, A. V.: An investigation of a wave of temperature difference between solid and fluid phases in a porous packed bed, International Journal of Heat and Mass Transfer, 37, 3030–3033, 1994.

Schmeling, H., Marquart, G., and Grebe, M.: A porous flow approach to model thermal non-equilibrium applicable to melt migration. Geophys. J. Int., Volume 212, Issue 1, 1 January 2018, Pages 119–138, doi: 10.1093/gji/ggx406, 2018

Schumann, T. E.: Heat transfer: a liquid flowing through a porous prism, Journal of the Franklin Institute, 208, 405–416, 1929.

Spiga, G. and Spiga, M.: A rigorous solution to a heat transfer two-phase model in porous media and packed beds, International Journal of Heat and Mass Transfer, 24, 355–364, 1981.

Stuke, B., Berechnung des Wärmeaustausches in Regeneratoren mit zylindrischen und kugelförmigen Füllmaterial, Angewandte Chemie, B20,262, 1948.