

Overall:

“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.”

Thank you very much for your thoughtful and detailed comments. I am very grateful for the time and effort you have devoted to this review—your comments are invaluable and will greatly improve this paper.

I want to post an **initial response**, below, in which I address each of your comments and sketch out an outline of how I would address these in a potential revised manuscript, should the editor allow a revision. I understand that I am to wait until the editor’s decision is made and, **if I am allowed to submit a revised manuscript, I plan to upload a final, more complete version of my responses to the comments, referring to line numbers in the revised manuscript.**

Major comments

<p>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.</p> <p>...</p> <p>... Therefore I strongly recommend to include the diffusive term into the calculations of thermal non-equilibrium and rerun the models.</p> <p>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.</p>	<p>Yes, this is an important critique and you are correct that the model assumptions are invalid when the heat transfer coefficient is too large. I had tested inequalities (1) and (2) you derived using Fourier modes, but for an earlier set of models with smaller heat transfer coefficient, k</p> <p>I am redoing these calculations including the diffusion terms to test the robustness of my interpretations of the TRZ and the overall heat LAB budget for large Peclet numbers. I am aware that the conclusions of the current manuscript may undergo modification. The goal of this paper is to set limits on the importance of disequilibrium heat exchange within the lowermost continental lithosphere, and this should still be possible with the suggested change to the model.</p> <p>To avoid this, I choose dt to satisfy the CFL condition. I plan on describing my numerical methods in more detail in a short section in the revised paper. Currently I am using an explicit leapfrog method with center-time and 2nd order upwind finite difference scheme; see response comment #3</p>
<p>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 ϕ is the porosity and d is the channel distance. From the physics point of view it is proportional to $(1-\phi)/(d\delta)$ where δ is the microscopical thermal boundary layer thickness at</p>	<p>This is a good point. For the timescales of the driving term here, we consider durations that are longer than $1/k_s$, a nominal thermal response timescale for the solid. I plan to touch on this, and the relationship to the microscopic treatment in your 2018 paper, in the revised paper.</p>

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<p>the solid-fluid interface (Schmeling et al., 2018). Only for long period thermal variations δ is of the order of d.</p> <p>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 $a_{sf} \approx A(1-\phi)/d$</p> <p>(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.).</p> <p>A back-of-envelope calculation results in $A=2$ or planar channels, while for cylindrical tubes it is more complicated if written in terms of d (the formula contains square roots of ϕ). 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.</p> <p>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 β values of 10, 8, 6 are assumed for spherical matrix grains, cylinders or slabs, respectively.</p> <p>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 $\Phi=1/\beta+$ 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}/δ where δ 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 δ rather than d in estimating the effective conductance is that δ is time-dependent, and theoretically includes the full thermal history of the two-phase flow. In Schmeling et al</p>	<p>OK, yes will fix this.</p> <p>I shall cite this paper in connection to the discussion of reasonable numbers for A</p> <p>Yes, thank you! – this is a typo in both C1 and C2</p> <p>Yes, I need to address this as a limitation of this way or estimating the effective conductance. I will essentially discuss the caveat that this underestimates the heat transfer coefficient for short term variations, limiting its applicability to thermal driving terms that vary over timescales that are “long” per your 2018 paper.</p>
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<p>(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.</p>	
<p>3.) You don't say how you solve the equations. Please add a short section on the numerical method, grid resolution etc.</p>	<p>I plan to add a section on my numerical solution methods. The code is simple, written in Matlab and uses an explicit leapfrog method with center-time and 2nd order upwind finite difference scheme. The calculation is carried out on a 1D domain (N=5000 elements in most models) with dt chosen to obey the CFL condition (so it depends on z). To test the accuracy of the solution (e.g., when comparing to Schumann's and Kenyon's analytic solutions) I considered grid resolution tests and chose a model size that is appropriate. This optimization will likely need to be revised when the additional diffusion terms are included. I shall include a discussion of this in the revised manuscript.</p>
<p>4.) The Appendices D and E contain very interesting model results. In my opinion they should be moved to the main text.</p>	<p>Yes, both reviewers point out the problematic flow between the main text and the Appendices. I shall move these sections into the main text to improve the flow.</p>
<p>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 ΔT 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 ΔT,...</p>	<p>Agreed. I will use $(T_f - T_s)_{\max}$ instead of ΔT and explain this in the context of the implied disequilibrium heat exchange.</p>
<p>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 ΔT. 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</p>	<p>Yes, again I should more properly use $(T_f - T_s)_{\max}$</p> <p>OK, will check again</p>

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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.	Yes.
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Minor points:

7.) Line 308: you may note here that $1/z$ is the dimensionless channel velocity (but see also comment 13).	Yes, $1/z$ is indeed the dimensionless channel velocity – see also response to #13
8.) Line 334. Are ϕ_{in} and ϕ_{out} identical to ϕ_f and ϕ_s , respectively? Then you should use same symbols.	These are only identical if we take the ‘end member’ case where the channels are pure fluid and walls are solid. I was trying to say here that this does not need to be the case, but will clarify
9.) Line 337. You choose A and β independently, but they are geometric parameters for spheres, tubes and spheres. Particularly β is defined for solid spheres, cylinders and plates, while A is defined for fluid tubes, etc.	Yes, you are correct. I do this to investigate how the estimates of k are affected by a range in A and β – will clarify in revision
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' .	Yes, your point here and #13 below clearly show that the boundary and initial conditions need to be clarified.
11.) Line 363. Delete “migration”	OK
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)?	I think that this arises because he is using an analytic (perturbative) approach whereas I am solving the full system. I will explain this better, but using the velocity estimate in Kuznetsov as a starting point (a first guess), I find (empirically) that Eqn 3 best describes the velocity in my models. I will explain this in revision, but I am afraid that I don’t have a derivation.
13.) Fig. A2c causes confusion. From the x-label or figure caption we have $x'_{front} = (1/z)t'$. (5) This implies that the disequilibrium front has the non-dimensional velocity $1/z$. But the fluid velocity may be written as $v_{channel} = x_f t$ (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 $v_{channel} = x'_f v_{channel_kst} / k_f = x'_f t' v_{channelZ} = v'_{channel_vchannelZ}$ (7)	The confusion lies here: the initial conditions needed to be better described (currently in Appendix D): For $t < 0$, there is material flowing in the channels, at $v_{channel}$, but the channels are at the same initial temperature as the walls, $T_0 = T_s = T_f$. At $t = 0$, the temperature of the material entering the channels at $x = 0$ is perturbed. So, what I mean by x_{front} is not the location of the disequilibrium front . Instead it is the location of the in-channel material that entered the domain at $x = 0$ at $t = 0$. This material moves at the speed $v_{channel}$ relative to the walls and at a later time t , it is located at $x = x_{front} = v_{channel} t$. Therefore, there is no

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<p>with $v'_{channel}$ as non-dimensional fluid velocity. After elimination of $v_{channel}$ from both sides we have $v'_{channel} = \frac{1}{z}(8)$ which is in contradiction to eq. (5). Can you help me (and potential readers)?</p> <p>14.) Line 383. Sentence strange, probably delete one of the “is” or insert “which”</p> <p>15.) Line 390 – 391. Which “blue lines”? Do you mean the dashed lines or the double arrows?</p> <p>16.) Line 391: “wavelength” probably to be replaced by “period”</p> <p>17.) Line 149. I don’t see the strong function of k in Fig. A4.</p> <p>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?</p> <p>19.) Line 163: delete one of the parantheses “)” in the first <i>tanh</i> term</p> <p>20.) Conclusion: Here I suggest to repeat the meaning of the abbreviations CLM, TRZ again</p>	<p>contradiction, and $1/z$ is indeed the dimensionless velocity of material in the channels. I think the word “front” here is causing the confusion. In the revised paper, I will change this to something like x_{pert} to more clearly indicate that this is the location of the material that entered with a perturbed temperature.</p> <p>OK</p> <p>Thanks for catching a typo: these should refer to the double arrows (they used to be blue in a previous version)</p> <p>No, it is wavelength: I mean the peak-to-peak distance (at a fixed time)</p> <p>Ok, it should refer to the functional dependence on d, which in turn strongly controls k</p> <p>The sentence is confusing... the exponential decay I am referring to is a spatial decay as a function of distance along the transport direction. So, the successive peaks in the blue curve on figure A2b should go down by roughly the same factor as they are their distance apart is roughly the same. I will check this and report it quantitatively in the revision.</p> <p>Yes</p> <p>OK</p>
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