Initial Response to RC2 4 April, 2022
Mousumi Roy

Overall:

"This paper investigates the potential for channelized melt transport into the base of the thermal lithosphere to supply an elevated localized heat flux. A simple modelling approach is used with a series of idealized forcing scenarios. The results include calculations of the scale of the thermal reworking zone and estimates of the overall heat supply. The modelling approach is heavily idealized and so is subject to significant limitations. The writing of the paper was hard to follow in parts. This could be improved by restructuring as a coherent whole without the back-and-forth use of appendices to develop both theory and results. However, the topic is interesting and the modelling is a useful starting point that makes a good contribution to analyzing the problem. Overall, I think the paper should be *accepted subject to minor revisions."*

Thank you very much for your review. I appreciate the thoughtful and constructive comments you have provided; I gratefully acknowledge they will improve the paper and increase its impact and readability.

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.**

General comments

1. Explanation of model, especially relating to the heat transfer and channel spacing: In the specific comments section, I give several suggestions for how I thought the model and results could be explained more clearly. This includes some suggested revisions to the notation. My main concern in this area relates to the modelling of the heat transfer process. Physically, I would think that macroscale heat transfer results from microscale diffusion (i.e. thermal conduction), but the paper states that axial conduction is neglected. But looking at the appendix (C2), k is proportional to an effective thermal conductivity divided by the square of some length scale (d in the equation). The correct choice/s of length scale is the crucial issue (the square is clear from dimensional grounds). The authors say that d is the channel spacing, but elsewhere (L316) say that d is the particle diameter. These are obviously very different. So the relevant length scale needs much better justification and the role of conduction (equivalently diffusion) in the model should be clearer.	Thank you for this comment. As R1 has pointed out also, the grounds for neglecting axial conduction are not valid for some of the models I consider. **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.
2. Simplifications in modelling approach: There are numerous simplifications inherent in the modelling approach. These are generally mentioned in the text but I felt the paper would benefit from	Yes, related to 1 above, I plan to also discuss the relative importance of axial conduction terms in the equations and address how including these affects the overall story of the paper.

more analysis of the relative importance of the various simplifications made. Two related simplifications that seem especially important to me relate to the parameters φ (fraction occupied by channel), the make up of the channels, and thermal (and perhaps chemical feedbacks).	Chemical exchange is ignored here entirely and this is indeed a limitation, but it will be more clearly stated in the revised paper.
It is not entirely clear whether the channels are envisaged as purely liquid, narrow dikes surrounded by entirely solid rock or much wider bodies of partially molten rock, where a channel is distinguished as having a higher melt fraction. In either case, it is clear that the properties of these channels are in practice determined that the operative dynamics and it is a large simplification to just impose them. There must also be feedbacks between any thermal reworking process and the channels themselves but this can't be investigated within this type of model, as the channel properties are just imposed.	As I mention in lines 330-339 in the current manuscript, the channels may be a high-porosity region within a lower-porosity surrounding region. To explore the 'end-member' upper limit to the disequilibrium heat exchange, I consider the case where the channels are purely liquid and walls are solid. This needs to be clarified in the main text and not in an appendix (see response to #3 below). Yes, there are no transport and channelization feedbacks that can be explored in this limited approach (but see lines 50-55; 98-100 in current manuscript). I plan to make this clearer in my revision.
3. Paper structure: Significant aspects of the paper were hard to follow. I was less concerned about appendix A (but also don't see why a few short paragraphs couldn't be included in the introduction). Appendix C develops substantial aspects of the model (including aspects novel or specific to this study) to such an extent that the description in the main text relies heavily on material in the appendix (e.g. the discussion of k, k_f and k_s, which are crucial to the paper). Appendix B is rather more technical, but the meaning of symbols developed there is relied on elsewhere. So it should eitherbe incorporated into the main text, or care should be taken such that all notation is properly defined in the main text at least.	OK, thank you for this comment. I feel your suggestions will greatly improve the flow of the paper and strengthen its impact. This is also in line with R1's comments on the organization of the Appendices and the material in the text.
Appendix D and especially appendix E, given that it is perhaps the most 'realistic' scenario considered, also belong in the main results section. The summary given relies on notation developed in the appendices as well as figures only reported in the appendices. For this style of journal, the back-and-forth between main text and appendices is hard to justify.	Yes.

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Technical comments:

4. L33–45 or final paragraph of introduction: Consider referring to body of work relating to thinning of the thermal lithosphere in arc settings (e.g. England and Katz, 2010, https://doi.org/ 10.1038/nature09417, Perrin et al., 2016, https://agupubs.onlinelibrary.wiley.com/doi/10. 1002/2016GC006527 and Rees Jones et al., 2018, https://doi.org/10.1016/j.epsl.2017.10.015.)	Yes, I shall include some of these papers.
5. L51-54: this is a very significant simplification as it precludes any feedbacks between the channels and the process(es) that create them.	Yes. It is highlighted in the beginning of the Discussion also, currently lines 176-186.
6. L58: 'v is transport velocity' needs a bit more explanation (transport velocity of what?). Also I assume from the equations that the solid is not moving but this could be stated more clearly in the text. I don't really understand why you introduce a new symbol $v_{channel}$ when it seems to be the same as v. The cartoon sketch in figure 1 is also a bit unclear as to whether v is the fluid velocity within the narrow channels in the zoomed in circles or some kind of average?	OK, yes, this is a typo. The velocity should be $v_{channel}$ everywhere. I plan to retain the 'channel' to specify that this is the average rate of relative motion of material within and outside channels. I need to clarify this as an average rate
7. Eqs. 1–2: This way of defining k_f and k_s could be clearer. The notation is also potentially confusing as k has different units from k_s and k_f. Suggest changing one of the symbols.	OK, your point about the units being different is very good and I will change the notation so *k* is only used as the effective heat transfer coefficient.
8. Figure 1: These time-dependent forcings have very different total energy inputs which could be emphasized a bit more, perhaps.	OK.
9. L87 & L109: 'across channel walls' sounded a bit strange because the fluid flow seemed to be vertical so there wouldn't be much flow across channel walls, since the walls in the sketch are also near vertical.	I shall clarify; I mean relative motion between material inside and outside channels.
10. L104-112: consider phrasing this discussion in terms of a Peclet number.	Yes, this was also brought up by R1 and I will include this in the revision
11. L136: Think 'duration' was intended rather than 'amplitude.'	Yes, you are correct.
12. L138–: Think that this section would be easier to understand if text from appendix (and especially figures) was included in the main text.	OK, agreed.
13. Figure 2: This is a useful figure. But I think plots against x at a series of t values are also useful complementary way to show the same data.	Yes, I will include this in a revision

14. L174: 10 m.	Yes, I need the units
15. Figure 3: Consider plotting agains the theoretical scaling to collapse all the data on a single line.	I thought about this, but decided against it because the effect of *d* on δ is important to show visually.
16. Figure 4 & L231: I wondered if this velocity range was rather low, for example when compared to typical asthenospheric melt velocities which might be an order of magnitude larger.	OK. I plan to revisit this after correcting the calculations to include the diffusion terms
17. L203–224: Perhaps it would make more sense to consider the overall LAB heat budget rather than one component.	Agreed. But the stated goals of this work are to place limits on this one process, namely disequilibrium heat exchange. I can state this explicitly here again.
18. L305: z is an odd choice of symbol (looks more like a vertical coordinate) and could be defined more clearly.	OK. This is in keeping with some of the previous literature I cite. I can see how this would be confusing though and will think about how I can clarify this.
19. L310: Might benefit from a brief discussion of the numerical methods used.	Yes, agreed. I plan to add a short section on this and on grid resolution tests.
20. L311 & 316: d appears to be used for two different quantities	Yes, this is confusing! It will be corrected by using a different symbol for particle diameter.
21. eqs. C1 & C2: check whether the minus sign is correct. This looks like it should be related to the harmonic mean of two conductivities (it would be with a plus sign). And the equations would be problematic if the term in square brackets were zero.	Yes, absolutely; As also pointed out by R1, another typo – thank you!!
22. L325: Not sure where this range came from originally but I don't think it would be appropriate if the model is intended to be of a porous flow, it sounds more like a pipe flow argument.	OK. I agree that it is tricky in a 'coarse grained' model such as this to connect to microscopic geometry. My intention here is to illustrate what reasonable numbers might be for A and β… as Reviewer 1 suggested, I can connect to some previous work to motivate this better.
23. Figure A3: Could benefit from better formatting to match the standard of the other figures	OK – I am guessing you mean panels (c) and (d) in particular. I will fix this.