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"

I. GENERAL COMMENTS

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*.

II. SPECIFIC 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.

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

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). 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.

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 either be incorporated into the main text, or care should be taken such that all notation is properly defined in the main text at least. 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.

III. TECHNICAL CORRECTIONS

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`.)

5. **L51-54:** this is a very significant simplification as it precludes any feedbacks between the channels and the process(es) that create them.

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?

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.

8. **Figure 1:** These time-dependent forcings have very different total energy inputs which could be emphasized a bit more, perhaps.

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.

10. **L104-112:** consider phrasing this discussion in terms of a Peclet number.

11. **L136:** Think 'duration' was intended rather than 'amplitude.'

12. **L138–:** Think that this section would be easier to understand if text from appendix (and especially figures) was included in the main text.

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.

14. **L174:** 10 m.

15. **Figure 3:** Consider plotting agains the theoretical scaling to collapse all the data on a single line.

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.

17. **L203–224:** Perhaps it would make more sense to consider the overall LAB heat budget rather than one component.

18. **L305:** z is an odd choice of symbol (looks more like a vertical coordinate) and could be defined more clearly.

19. **L310:** Might benefit from a brief discussion of the numerical methods used.

20. **L311 & 316:** d appears to be used for two different quantities

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.

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.

23. **Figure A3:** Could benefit from better formatting to match the standard of the other figures.