

Review of Revision 1 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

I thank the author for the detailed responses to the reviewers' comments. I think that the revised version of the manuscript has been improved in important respects. The structure of the paper is now much better. However, I still think the physical description of heat transfer could be improved (point 1) and think the expression of the scaling behaviour of the TRZ width, which was added in revision, needs reworking (point 2). Overall, given the strengths of the paper pointed out in our earlier reviews, I think the paper should be *accepted subject to minor/technical revisions*.

II. SPECIFIC COMMENTS

[In all the following comments, I refer to line numbers in the tracked changes version of the revised manuscript.]

1. **Explanation of model, especially relating to the heat transfer and channel spacing:** I want to return to this issue as I think could be more clearly described. In the full physical system, heat will be transferred by advection and diffusion (and latent heat, if there were phase change). The diffusion will occur both in the vertical (along channel) and horizontal directions (across channel, from the hot channel into the colder surrounding rock). In revision, an extra term was added to equations (1) and (2), representing vertical diffusion. This is absolutely fine. However, generally you expect horizontal diffusion to be much more important than vertical diffusion (because the horizontal length scale is smaller than the vertical scale). In a 1D (vertical) model, you cannot represent horizontal diffusion explicitly. Instead, in this type of model, the effect of horizontal diffusion is represented by the heat transfer term involving K .

I think that the text of the paper should make it much clearer that this term involving K arises from horizontal diffusion. At present, the first paragraph explaining K (starting L132) emphasizes that K is a proxy for the geometry of the channel wall interface. The block of text added starting on L173 starts to address the crucial issues. Based on dimensional and physical arguments, you would expect

that the timescale of heat transfer to be proportional to the square of a boundary layer dimension (since thermal diffusivity has units length²/time). Then you assume (on the grounds explained around L180), that the boundary layer dimension is proportional to d , which gives you essentially equation (4). So I would recommend rewriting L132–183 to start with the essential physics (horizontal diffusion) and assumptions first, before moving on to the details are the channel geometry. I would try to limit switching between K , $K_{f,s}$ (which have different units to K , something I found confusing at first, and would ideally be avoided), and C_{eff} as far as possible. L119 was also quite confusing in that it talked about diffusion being ignored and used the symbols $D_{f,s}$ which don't seem to appear elsewhere.

2. **Thermal reworking zone width scaling:** the expression given on L14, added in revision, is dimensionally inconsistent. The RHS doesn't have the same units as the LHS. I do not think the final result should be expressed with a term like $(\tau/d)^n$, given that τ/d is not dimensionless. Ideally, you want expressions like equation (14), where a dimensionless quantity is raised to some power. In the main results section, around L318, you have $\delta \sim \tau^n$, which doesn't have any dependence on d . But then the abstract (L14) and conclusion (L397) give a proportionality d^{-n} . It would be good to explain the dependence on d and to try to write the final result in dimensionally consistent groupings. (A theoretical justification for the $n = 1$ scaling would be very good if possible, but I appreciate this might potentially not be straightforward.)

III. TECHNICAL CORRECTIONS

3. **L14:** $n \approx 2$ (remove word 'is').
4. **around L86:** I would say that a key limitation of this type of study is that the channel properties are imposed, rather than emergent dynamically. This simplification is well described later in the paper, but probably should be mentioned somewhere in the introduction more explicitly.
5. **L110:** perhaps should define x (particularly as x is often used a horizontal coordinate) and t .
6. **L202:** perhaps avoid extra spaces in, e.g., T'_f .
7. **L207:** I'm not entirely sure what ζ means (in particular, it is not entirely clear how it has been defined. Perhaps give a formula. I also think the choice of notation is a bit unusual.
8. **L281:** missing space before 'years.' (I noticed some other examples of this too.)
9. **Table 1:** missing link to appendix.