

Interactive comment on “Numerical model of crustal accretion and cooling rates of fast-spreading mid-ocean ridges” by P. Machetel and C. J. Garrido

Anonymous Referee #2

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The manuscript “Numerical model of crustal accretion and cooling rates of fast-spreading mid-ocean ridges” by Machetel and Garrido describes several updates the authors made to their original 2009 model and discusses a number of example calculations for the cooling history of fast-spreading ocean crust. While I find the paper interesting, it needs moderate to major revisions before publication. I come to this conclusions mainly due to three reasons: 1) not too much has changed in the model formulation with respect to the 2009 model and I have a number of potentially major technical comments, 2) the discussion and interpretation of the modeling results should be improved, 3) the text needs revisions.

As GDM is mainly a platform for modeling studies, I will start with discussing the tech-
C861

nical parts of the paper:

The original Machetel and Garrido, 2009 (MG09) introduced a nice modeling framework to study the thermal structure of fast-spreading ridges. Now the authors present an improved version of their model. Unfortunately only the setup is changed so that the sheeted dike layer is better resolved, otherwise there are no major improvements on the technical side. Given the great progress that was made in the geodynamics community over the past years in simulating lithosphere dynamics, I had hoped for more.

That said, I have a number of comments on the model description:

One major concern is that the model is overprescribed. Constant temperature boundary conditions are applied at all boundaries with the half-space cooling solution being applied at the bottom and right margin of the box. Doesn't this put too many constraints on the solution? The whole point of the paper is that the near-ridge crust does not cool according to the half-space cooling model. I think you can actually see the artifacts caused by the boundary conditions on the right boundary (Fig. 3) where the temperature is forced back to the half-space solution. Also the melt emplacement geometry (e.g. melt lens depth) is fixed. If the model had more freedom to evolve (e.g. predict the depth of the melt lens instead of prescribing it, self-consistent heat flow from the mantle, free moho and surface topography, etc.) the different melt emplacement geometries would probably make very different predictions on the ridge thermal structure and required magnitude of hydrothermal cooling. The current setup puts too many constraints on the solution and that's probably why all the model runs look pretty much the same.

What is actually the geometry of the melt lens or is new material only added directly on axis without a horizontal scale? That again influences the solution. I think the description of the stream function boundary condition should be improved in this respect.

Another point is the viscous flow law. It is nice that the authors account for viscosity

variations with melt fraction. However, I am surprised that there is no explicit dependence on temperature (only through the melt fraction). Shouldn't temperature have a first order effect on viscosity? Which flow law is used? The used values seem orders of magnitude too small. The authors should clearly state which flow law is used and put a citation.

What are the benefits of using a stream function approach? Most modern codes use some kind of mixed pressure-velocity formulation, which is somewhat more flexible. In the same direction: why is an ADI solver used instead of a direct 2D solver? Maybe the authors want to discuss their numerical strategy a bit more.

I guess on the left-hand side of eqn. (7) the dT/dt is the material derivative. It's a bit non-standard to write it with a small d instead of a capital D . It should also be clarified in the text that the advection term is hidden inside this derivative.

Speaking of advection, how is advection resolved? I think this should be discussed.

The energy equation includes the latent heat effect of crystallization. But shouldn't there be another term accounting for heating through melt injection? The dykes are, for example, emplaced hotter than the ambient temperature and that should be accounted for.

I generally like the discussion of the modeling results and the implications of melt emplacement geometry for the cooling of young ocean crust. However, I am a bit concerned that the results are basically not benchmarked. Before interpreting cooling rates, I think the modeling results should be compared to some data to check if they are consistent with observations. This is typically done by matching the depth of the melt lens and/or the thermal structure from seismological studies (e.g. Dunn et al., 2000) (or heat flow data). None of this is done in the manuscript.

Do the different melt emplacement geometries require different amounts of hydrothermal cooling? What happened to the findings of Chen 2001 that only limited amounts

C863

of melts can crystallize close to Moho level?

I find the discussion of cooling rates a bit long – especially with respect to the discussion of the modeling results. Why not discuss the actual modeling results in more detail? For example, the reader does not get any answers to the questions on heat extraction from the near ridge crust outlined in the instruction of the text.

Minor comments:

The abstract should be rewritten.

page 2431, line 26: Why cracking temperature of peridotites?

Page 2435, line 5: Advantages with respect to what? Maybe it would be good to actually discuss why the authors use the stream function approach, while most current codes use mixed formulations in pressure and velocity.

page 2436 line 10: '...avoid arbitrary hypotheses on the thermal structure of the underlying mantle'. I disagree. The model would become way better if the mantle flow field were included/modeled (see my comments above).

Page 2440, line 11: all the simulated flows are laminar. Better to rephrase this.

Page 2444, line 7-10: '...all the cases investigated in this paper are finally consistent with geophysical data...' I don't think this has been shown - the authors should actually do the comparison.

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C864