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

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Dear colleague,

It seems that is is not possible to keep the italic blue font formatting we used to answer to your comments but you will find in the supplements a PDF file gathering our detailled comments and the proposed new version of the paper.

First, we want to thank you for the thorough editorial works done on the first version of the manuscript. We have taken into account or have answered positively to most of your comments concerning the opportunity to publish the new code; the discussion and interpretation of the results and the revision of text. Following several of your advices

several parts of the paper have been rewritten, several figures have been modified and new explanations have been added to better justify or improve results presentations. In particular we reduced significantly the discussion about the petrological cooling rate that we have more clearly separated from the numerical results presentation (gathered in section 5). You will find below the detailed answers for each of your comments, which are recalled in blue italic. Thanking you again, for the help you brought us improving the scientific content of our paper, we hope that this new version will find your approbation for publication.

Sincerely yours, Philippe Machetel and Carlos Garrido

P.S. Please could you find at the end of these pdf answers file the proposed new version of our paper

???????????????????Global comments: The manuscript "Numerical model of crustal accretion and cooling rate 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 GMD is mainly a platform for modeling studies, I will start with discussing the technical part 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 improvement no 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.

»»»»»»»»»>Since this last publication, several referees have asked us to take into account sheeted dyke layer and horizontal melt injection at the upper lens level (instead of a vertical one) as conditions to calculate realistic mid ocean ridge temperature fields. Indeed, the lack of such layer, combined with the vertical injection at the upper lens level, induces in certain runs, near the ridge axis; unrealistic counter rotating cells in the shallow part of the crust (i.e. see Machetel and Garrido, 2009, Fig. 4). This effect was not a numerical artifact but a consequence of the mass conservation equation and of the vertical direction of heat advection at the ridge. It was absolutely not possible to correct these effects with the previous model. The introduction of a sheeted dyke layer required a new model with in depth modifications of subroutines since the computation grid itself was concerned by changes in arrays index due to rows and columns adding and displacements at the upper lens level. Therefore, in spite of apparent similarity for the input files that describe the physical mechanisms and properties of the crust, the internal structure of the code has been deeply modified. The changes that have resulted from these modifications achieved to convince us it was justifying publishing a new version of the code instead of a dull and boring list of modifications.

?????????????????????That said, I have a number of comments on the model description: One major concern is that the model is overprescribed. Constant temperatures 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. . .

»»»»»»»»»»>The choice using half-space cooling thermal boundary conditions has precisely been done to limit prescriptions on thermal crustal surrounding. To our knowledge, there is no way to get direct measurements of particular thermal or dynamic states of the oceanic crust at the MTZ transition zone. With our approach, which is in

that sense complementary of those of certain other authors, we try avoiding any specifications that would represent particularization, but follow global geodynamic agreement considering half cooling law as a first order of oceanic lithosphere (and therefore its crustal part) thermal behavior. Furthermore, in this study we are more interested on the thermal and dynamic properties near the ridge axis, where the effects of the lateral boundaries conditions are weak in front of the effects of accretion structure and hydrothermal cooling. Indeed, far from the ridge, the crustal evolution is mainly driven by thermal conduction and laminar motions, while, near the ridge, higher vertical thermal gradients are consistent with hydrothermally enhanced heat extraction. Perhaps the intermediate area, where the far ridge and near ridge regimes gather should not be called artifact (since this word may also suggests an error that should be corrected) but the results of a situation that also exist in Nature for the phenomenon governed by partial derivative equation system but that are actually modified by the effects of their boundary condition. We have modified the text in the new version of the paper to better explain our approach.

?????????????????????. . .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. . ..

»»»»»»»»»»>This remark asks the question of the influence of finite length horizontal cracks starting from the ridge axis and propagating through an already partly cooled crust. In other words, what is the real offset of melt injection if it is not exactly the ridge axis. This problem cannot be directly taken into account by the present version of the code that assumes that melt injections through lenses and sills occur horizontally at the ridge axis. However, we have added in the text a description of how the internal boundary condition are set and explained how it is possible to modify subroutine "computation_grid". Only three classical situations are described in details in the paper: 1) the Gabbro Glacier, 2) the mixed MTZ and shallow lenses, and, 3) the superimposed sill configuration. They can be considered as benchmark cases to initiate numerous situations by users if they want to apply internal conditions to explore the effects of injection geometries.

????????????????????. . . 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.

»»»»»»»»»»>The iterative process used in our approach couples temperature and motion by successively solving the energy equation, the vorticity equation and the stream function equation. Therefore viscosity, which explicitly depends on temperature through Eq. 10 and 11, evolves with it all along the computing process. The shape of the viscosity vs temperature curve (Fig. 2) is not given in terms of explicit exponential function but its shape is given by a ïĄĞ function, which sharpness (60°C) around a threshold temperature (1230°C) has been chosen thank to its agreement with the Kelemen and Aharonov (1998) results for crystallization (Fig. 1). As a result of the third series of cases that have been computed, the increases of two to three orders of magnitude of viscosity contrasts between the weak and strong end-members do not induce decipherable changes on the solutions (see text and the relative positions of the cross and solid curves of Fig. 6). However, we have taken your comment in to account by giving better descriptions of the procedure in the text and figures.

???????????????????What are the benefits of using a stream-function approach? Most modern codes use some kinds of mixed pressure-velocity formulation, which is somewhat more flexible...

»»»»»»»»»»>The stream-function approach ensures a mathematical checking of the zero divergence condition for the velocity field. It also allows accurate, easy to operate

local prescriptions of discharge conditions for melt injection. Indeed, thanks to the mathematical and physical meanings of the stream-function, the difference of values between two points measures the flux of melt that join the model through that section. Furthermore, the stream-function contour maps reveal the tracer trajectories allowing direct visualizations analogous to virtual smoke flow visualization (e.g. Von Funck et al, IEEE transactions on visualization and computer graphics, vol 14, n°6, pp. 1396-1403, 2008). Sentences and this reference have been added in the paper and figures captions to better explain these reasons explaining why we have chosen a vorticity-stream-function formulation to constrain the melt intrusion and present the results.

???????????????????. . .In the same direction: why is an ADI solver used instead of a direct 2D solver? May be the authors want to discuss their numerical strategy a bit more.

»»»»»»»»»»>ADI solvers are appreciated for their convergence properties in case of elliptic solving (as it is particularly the cases for the stream function and the vorticity function). They are also easy to use with half-implicit scheme for the non-linear term of the vorticity equation due to the temperature dependent viscosity and the advection terms of the energy equation. Furthermore, the tri-diagonal shapes of solving matrix makes it easy splitting of computational domains into sub areas allowing simple encoding of the internal conditions prescriptions on temperature and stream-functions. The discussion of the numerical strategy has been significantly improved in the new version of the paper.

??????????????????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 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.

»»»»»»»»»»>Absolutely: this has been done in the new version of the paper. Speaking of advection how is advection resolved? I think this should be discussed. Advection terms of the energy equation are solved thank to half-implicit, second order accurate, alternate finite-difference schemes. Such methods are classically used for non-linear terms of partial derivative equations. This introduces constraints on time stepping that, for temperature, follows the Courant criterion but is over-relaxed for the stream-function and vorticity elliptic operators. These points have been emphasized in the new version of the paper.

?????????????????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.

»»»»»»»»»>The heat that is brought through melt injection is implicitly taken into account by the thermal boundary conditions at the ridge axis which is equal to the injection temperature from the MTZ level to the upper lens level and to the half –space cooling model conditions in the sheeted dyke layer from the upper lens to the surface. However, in this layer, the full energy equation is solved in thermal connection with the lower part of the crust (below the sheeted dyke layer). Then, the lateral propagation of heat is taken into account through the complete temperature equation, from the ridge axis to the lateral boundaries through; the conductive process, the latent heat release and the horizontal advection that occurs in the sheeted dyke layer. The vertical advection of heat is automatically cancelled by the zero vertical velocity condition in the sheeted dyke layer. However, the word "freezing" was misleading. It has been replaced by the word "solidification" at the ridge axis to describe modeling of the sheeted dyke layer.

???????????????????I generally like the discussion of the modeling results and the implications of melt emplacement geometry for the cooling f 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 is done in the manuscript.

»»»»»»»»»»As now shown in the Summary and Discussion section, the differences of thermal structures obtained for the G, M and S hypotheses induce minor temperature differences in temperature with depth and distance off-axis, which makes it difficult to use temperature (or geophysical proxies of temperature) directly trying to discriminate among the different crustal accretion scenarios. All cases investigated in this paper are consistent with the temperature structure at the ridge axis derived from geophysical studies at the East Pacific Rise (Dunn et al. 2000; Singh et al. 2006) that show a 8-12 km wide magma chamber (T<1150°C) with steep isotherms near the ridge axis. As a modeling work, however we are particularly conscious that we must be very cautious benchmarking our results with geophysical data. We are confronted here to a very difficult problem since the available geophysical data present by them self their own difficulties of interpretation and local characters. In our case, the depth of the melt lens, or the geometry of the melt intrusion at the ridge axis are not a result of the model, which could have be compared (with prudence) with the seismological results, but are a starting hypothesis. This is also done in previous numerical models where the depth of the melt lenses is taken at a starting parameter (e.g., Chevenez et al., 1998; Maclennan, 2004). This is why we clearly know that we must bound our ambitions (at least for this paper) to describe the trends that occurs according to the assumed thermal structure of the ridge. In the new version we have explained more the situation and tried to develop the discussion section in order to address this point which will, in any cases, remain a weakness of the theoretical modeling approach.

???????????????????Do the different emplacement geometries require different amounts of hydrothermal cooling? What happened to the findings of Chen 2001 that only limited amounts of melts can crystallize close to Moho level?

»»»»»»»»»»>It is clear, from the new Fig. 3 and Fig. 4 that, at constant hydrothermal cooling (or at least using the same parameter to assess the hydrothermal cooling); the G crustal accretion mode induces lower temperatures, at least locally and particularly near MTZ. This result appears clearly on the positive thermal anomalies that exist on the M and S structures just above MTZ. This comment has been added in the new version of the paper. Concerning Chen (2001), he did not consider the possibility of deep hydrothermal cooling; later thermal models of crustal accretion (e.g. Cherkaoui et al., 2003; Maclennan et al, 2004; Maclennan et al., 2004; Theissen-Krah et al., 2011) showed that substantial amounts of melt can crystallized in the lower crust if deep hydrothermal cooling is taken into account.

????????????????????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 introduction of the text.

»»»»»»»»»»»According to the previous remarks the discussion of cooling rates has been significantly reduced to focus on the main results that come out from the model: the strong dependence of the cooling rate curves versus depth on the temperature working range of the diffusion and the possibility for the Igneous cooling rate to be a deciphering tool for the melt intrusion structure. The numerical models are now clearly separated from the discussion that has been gathered in section 6.

?????????????????????Minor comments ?????????????????????The abstract should be rewritten.

»»»»»»»»»»»>This has been done in the new version of the paper.

?????????????????????sampled near/far from the ridge"?

»»»»»»»»»»»>This has been better explained in the new version of the paper

?????????????????????Page 2431: line 26, why cracking temperature of peridotites?

»»»»»»»»»»»>The sentence has been corrected in the new version of the paper.

?????????????????????page 2435: line 5, Advantages with respect to what? May be 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.

»»»»»»»»»»»>This has been done in the new version of the paper

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

»»»»»»»»»»»>Please, see our answer above.

?????????????????????Page 2440: line11, all the simulated flows are laminar. Better to rephrase this.

»»»»»»»»»»»>This has been done in the new version of the paper

?????????????????????Page 2444: line 7-10 . . . all the case 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

»»»»»»»»»»»>We modified these sentence and increased the discussion about this in the discussion part of the new version of the paper.

Please could you find in the following of our answers the proposed new version of our paper

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C1086/2013/gmdd-6-C1086-2013-supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., 6, 2429, 2013.

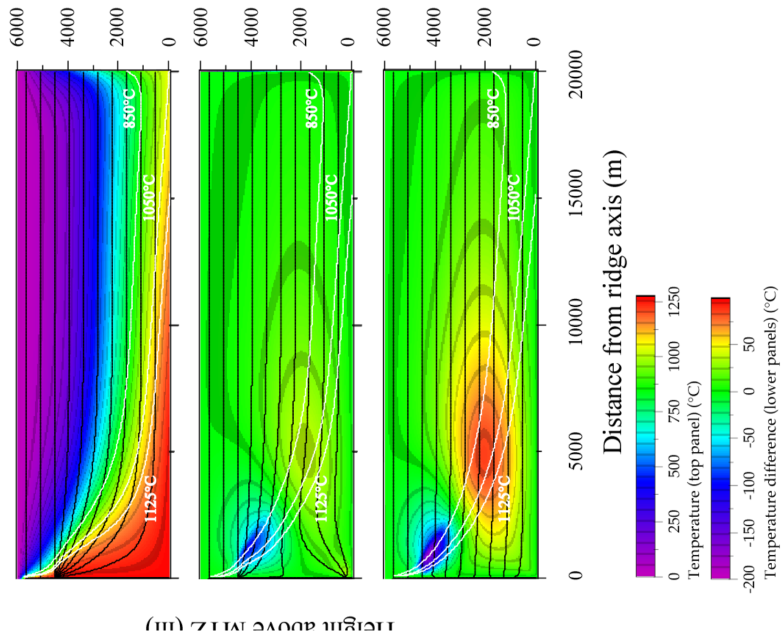

Fig. 1. new figure 3

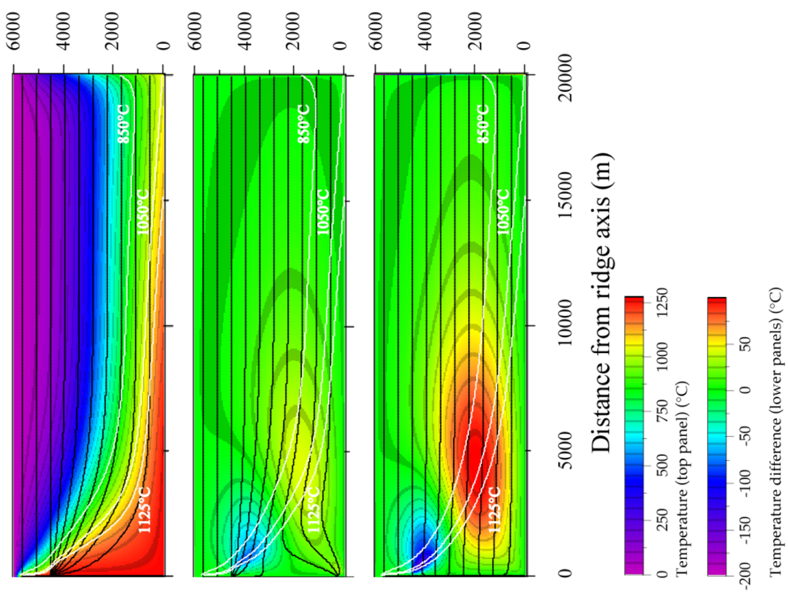

Fig. 2. new figure 4

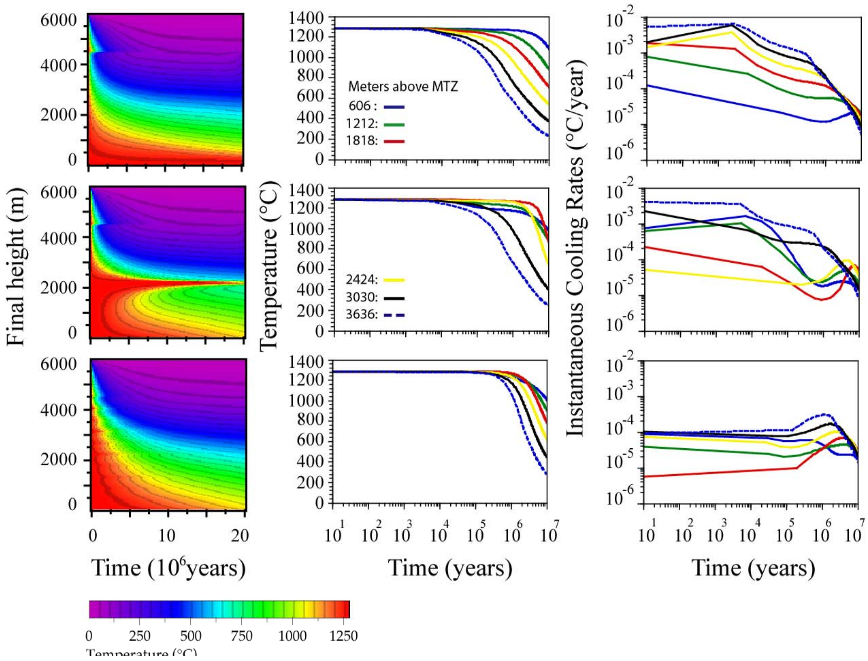

Fig. 3. new figure 5

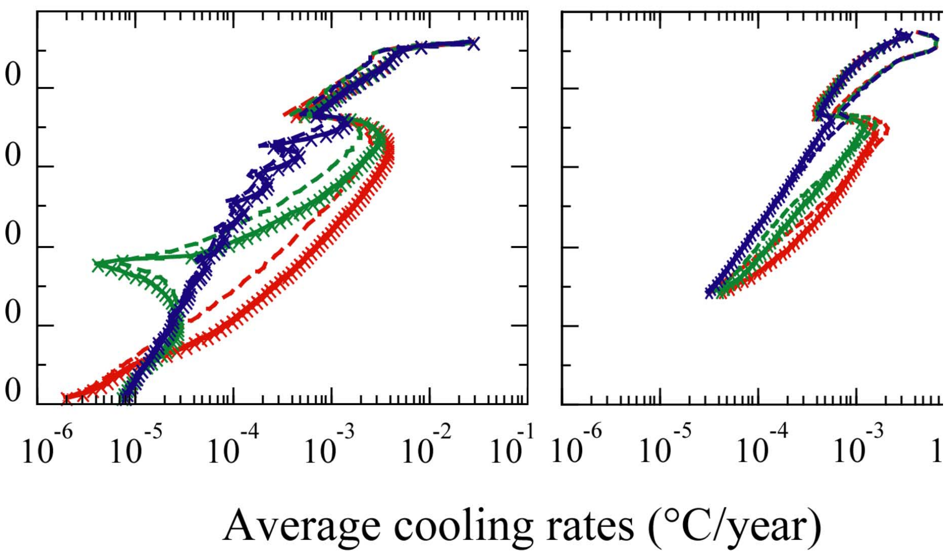

Fig. 4. new figure 6