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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

Anonymous Referee #1

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Review of the manuscript "Numerical model of crustal accretion and cooling rates of fast-spreading mid-ocean" by Machetel and Garrido (gmd-2013-48)

Overall I consider that the manuscript requires minor revisions, because it could present interesting results on the thermal evolution of crustal accretion in mid-ocean ridges, provided that some changes are made before publication. The numerical model with its strengths and limitations has been developed in previous studies. In my opinion, a more robust argumentation is required to justify some rather arbitrary choices in the model setup and interpretation of the results (for example, variation of Phi with depth, viscosity of the crust, melting vs temperature, discussion on cooling rates).

Section 5 (thermal history and cooling of the lower crust) should be extensively rewritten because 1) The discussion on cooling rates is not clear, the concept of cooling rate has limited validity and the definition of opening and closing temperature are not correct when used in relation to petrological studies. 2) Most importantly this section does not -clearly- show new results or conclusions, at least not in the format that is currently presented. I would strongly suggest to omit the discussion of petrological cooling models and just presents the results of the thermo-mechanical model. In particular it would be simply fantastic to see a x-y plot of T versus time of selected portions of the model, i.e. by following the position of selected tracers at different depths and horizontal displacements. In this way the authors will show the -true- cooling evolution in time and space. This by itself would be a great achievement. Petrologists will decide how their approach to thermal cooling fit into the more general thermal evolution presented here (in a x-y plotting format). In addition, it would be interesting to see how the model from this study would compare with geophysical observations such as surface heat flux or topography which are directly affected by the thermal evolution of the upper and lower crust. More detailed comments are listed below.

pag 2430: line 12-14, I am not aware of any analogy between the cooling rates defined as ICR and SRC and the cooling rate defined by experimental(?) petrology. What does it means "cooling rates sampled near/far-from the ridge"?

pag 2431: line 29, Theissen-Krah et al (2011) adopted a upper cracking temperature limit of 600C. The 400-1000C temperature range comprieses the values used by several of authors, some of them are cited correctly in the next few lines.

pag 2435: line 15-16, I don't see any physical reason that justify the assumptions that phi_c = 2 V_p H and phi_lb = 0.5 phi_c = V_p H.

pag 2436: line 22-23, It is not clear to me why the thermal behavior of the sheeted dyke layer is simulated by instantaneous freezing.

pag 2437: line 23-25, Can I see some references from literature that support the temperature and the temperature interval for crustal melt that have been used here (1230C, dT=60C). The experimental petrology studies that I am aware of (Green and Ringwood, 1967, Yasuda and Fujii, 1994), show something different, at ∼1kbar, T solidus ∼1100C, T liquidus ∼1350C.

pag 2438: line 23-25, Viscosity of the crust (hot and cold) is extremely low, between 2-4 order of magnitude lower that commonly assumed (list of references is very long). Could the authors explain why they have chosen those values and what are the consequences of such assumption on the thermal and dynamic results?

pag. 2438: line 24, I believe low cracking temperature is ∼400C (Nicolas et al, 2003) therefore the temperature of 700C adopted by the authors cannot be really considered a low T.

pag. 2439: line 20, It is very difficult to see from fig. 3 any temperature variations among the 3 models (G, M, S). Wouldn't be better instead to plot the temperature difference with respect to one model, say panel a) T(G), panel b) T(G)-T(M), panel c) T(G)-T(S).

pag. 2440: line 16, Fig. 4 has the same problem of fig. 3. Plotting the temperature difference may help to visualize better the T variation of the 3 models. Is there any reason why despite a different dynamic evolution, the 3 models show very similar temperature fields?

pag. 2441: line 1 and following. Fig.5 is not clear at all. Does the plot refer to tracers along the ridge axis (x=0)?, what is the lateral position of the tracers that are plotted in the figure?

It would be terrific to see a x-y plot of temperature versus time for selected tracers located at different depths and horizontal positions at time zero.

pag 2441: line 20-21, The concept of opening and closing temperature in this contest does not make any sense. The model presented in this study provides much more. It is

possible to evaluate the instantaneous cooling rate by simply taking dT/dt at each location over time. If the authors really want to make a comparison with petrological cooling models, I would suggest two possibilities: 1) compute the diffusion profiles given the T-t path retrieved by the thermo-mechanical simulation. The computed profile should match the measured chemical profiles in real mid-ocean ridges rocks. 2) Cooling rates based on chemical kinetics are related to the closure temperature of the geochemical system. Comparison should be done by choosing a particular system (say Ca diffusion in olivine), calculate the closure temperature and the cooling rate and then compare it with the cooling rate at the corresponding temperature from the geodynamic model.

pag. 2441: eq. 15, The definition of cooling rate given in eq. 15 is only true for linear cooling otherwise it is useless. If the intent is a comparison with petrological models, I would suggest the authors to review the definition of closure temperature (Dodson, 1973), it is not an arbitrary concept (temperature at the characteristic time when D decreases by a factor equal to e ∼2/3). Furthermore, cooling rates from petrological models are dependent on the closure temperature and the geochemical system, therefore a comparison of cooling rates from this study and petrological studies would require the same critical approach.

pag. 2445 line 5 and following, I will omit completely this part. It is not clear to me why it is so important to compare the cooling rates from this study with those retrieved from petrological studies. Beside adding few x-y plots of T versus time, the impact of this work would dramatically increase if the results would be compared with mid-ocean ridges observable data rather than numerical results using other methods. For instance, assuming that fig.6 does not plot cooling rates computed from eq. 15 but the true instantaneous cooling rates versus height, it would be interesting to see a discussion on how these extremely low cooling rates affects heat flux on the surface. Mid-ocean ridge topography could be easily computed and compared with available data.

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