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Interactive comment on "A simple 3-D numerical model of thermal convection in Earth's growing inner core: on the possibility of the formation of the degree-one structure with lateral viscosity variations" *by* M. Yoshida

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Response to Interactive comment on "A simple 3-D numerical model of thermal convection in Earth's growing inner core: on the possibility of the formation of the degree-one structure with lateral viscosity variations" by M. Yoshida Anonymous Referee #2

This manuscript is aimed to reveal the style of thermal convection in Earth's inner core with its thermal history that might have degree-one structure. There have been several studies on the style of inner core convection mainly done by R. Deguen and his

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colleagues that was called as 'Translational Regime'. The author discussed formation of degree-one convection caused by the rheological heterogeneity because a series of studies by Deguen and his colleagues could not address the lateral viscosity variation due to their numerical limitation. The lateral viscosity variation could be caused by the generation of hemispherical feature observed by various seismological analyses but the time-scale is too much long compared to the current understanding on the age of the inner core, which would be over 3.0 Gyrs. This means that the 'translational regime' found by Deguen and colleague would be still a great candidate for under-standing the large-scale seismic heterogeneity and anisotropy observed in the inner core but this study would be somewhat interesting and important step for the commu-nity. However, unfortunately, I CANNOT recommend publishing this manuscript without huge amount of revisions experienced because I found a bunch of technical errors in the model assumptions and treatment of boundary conditions as well as missed cita-tions on various important literatures and important discussion on inner core dynamics and its implication for seismic observations. If the author fix all of his mistakes in his model assumption and numerical procudure, cite various important literatures and add some important discussion on the age of the inner core, I would re-consider my recom-mendation. Again, currently, this manuscript SHOULD NOT be published in anywhere. Before discussing contexts of science in the original manuscript, author should address significant issues listed-up (also required with re-run for all cases as well as several ad-ditional runs will be required. In addition to that, both thermal and mechanical boundary conditions used in this study MUST be changed.) below because entire discussions in this manuscript would NOT be quite understandable and convinced for readers.

[Reply] I sincerely thank the reviewer for critical comments. I have carefully incorpo-rated all the comments and suggestions into the revised manuscript attached below. The revised parts are highlighted by red. Although I regret that there are some mislead-ing descriptions for readers in the original manuscript, this paper did not violate any of previous studies published so far. I have total respect for all of the previous studies on the core modeling. However, I would acknowledge sincerely the lack of explanation of

our model and discussion with the previous studies. Thanks to the reviewer's thoughtful comments, I could find new literatures, expand discussion largely, and largely improve the manuscript, separating the previous Section 4 to new Sections 4 (Discussion) and 5 (Conclusions). However, the main conclusions was not changed from that in the original manuscript. Of course, I knew that there are many theoretical and numerical studies on core dynamics, but am only trying to present a simple numerical model and open the discussion. This is because I submitted the manuscript to Solid Earth, not to other journals. However, again, I would acknowledge the lack of required discussion in the original manuscript and I deeply regret this point and deeply acknowledge all of reviewer's comments.

I provide below my response to reviewer's comments.

1. I demand author to check effects of radioactive heating because the possibility of radioactive elements in the outer and inner core would be still under the debate in mineral physics, geodynamics, geomagnetism and seismological communities.

[Reply] Thank you very much for your comment. Because the radioactive potassium may be major heat source in the inner and outer core, and there is a large possibility that the role of potassium is important in determining the history of growth of inner core and geodynamo power (e.g., Nimmo et al. 2004). However, I ignore explicitly radioactive heating in the present model because the amount of radioactive potassium in the inner and outer core would be still under the debate in mineral physics, geodynamics, geomagnetism and seismological communities, and instead consider primordial heating in the heat source term of the conservation equation of energy. I have explained explicitly this point in Section 2.

2. Regarding the treatment of convective vigor and secular cooling (eq. (5) and eq. (6)), I did NOT quite understand why the author used such formulations. For example, the heat capacity was appeared in the Rayleigh number, which was quite odd to me. Why was the heat capacity appeared in the Ra? Please justify how to formulate the

Rayleigh number.

[Reply] I opened eq. 5 to enable readers to understand a relationship of (thermal diffusivity) = (thermal conductivity) / (density x heat capacity)

3. I did not get it on the non-dimesionalization done by author's formulation. I did not understand the temperature difference without bottom boundary as the convection was occurred in the inner core. Since the temperature at the center of inner core could not be determined uniquely due to its singularity, using a temperature difference across the convective region would not be correct. The only correct way for determining the temperature difference across the inner core should be scaled by the amount of heat source. The author MUST correct it.

[Reply] Thank you for your comment. Of course, there is another way for normalizing the temperature the inner core by the amount of heat production. However, as stated in reply to reviewers comment #1, I ignore explicitly radioactive heating in the present model. This is because the temperature the inner core is normalized by the characteristic temperature variation, although there is another way for normalizing the temperature the inner core by the amount of heat production. I have explicitly mentioned this point in Section 2. I agree that the word "reference temperature difference" misleads the readers, so, it is replaced by "characteristic temperature variation".

4. On addressing the secular cooling, the non-dimensional heating shown in Eq. (5) was somewhat odd to me as well. Yes, it should be zero without the inner core BUT the inner core got cooling down rapidly (11.3 TW!) once the inner core gets started growing. I am not quite sure if the inner core might have the primordial heating or not. I guess that it would be probably NOT. The correct way is for secular cooling to be addressed as the boundary condition NOT the internal heat source in the convection system. The initial temperature of inner core should be determined by the solidus temperature of iron-alloy and adiabatic heat flux across the ICB plus the latent heat release and gravitational energy caused by light element release. I would understand that the

molten core might have initial accretion energy before inner core started growing but, again, NOT in the inner core. Therefore, this assumption is completely WRONG. The author MUST fix it then re-run all cases.

[Reply] Thank you very much for your thoughtful comment. I absolutely agree your comment. In section 4, I have explicitly mentioned the lack and limitation of the present simple numerical model. I absolutely agree your comment and expanded the discussion. As reviewer pointed out, the dimensionless internal heating production shown in Eq. (5) is incomplete because the inner core got cooling down rapidly once the inner core gets started growing in the present model. The exact way is for the secular cooling to be addressed as the boundary condition problem, not the internal heating production, because the initial temperature of inner core should be determined by the solidus temperature of iron-alloy, adiabatic heat flux across the ICB, the latent heat release and gravitational energy caused by light element release. Furthermore, the molten core might have initial accretion energy before the inner core started growing, but not in the inner core. Although the integration of these two serious problem into the present numerical model is numerically difficult and is beyond the present study on the possibility of the formation of the degree-one structure with lateral viscosity variations, they should be considered in the future model. In any case, I have explicitly discussed these point in Section 4.

5. Moreover, related with comment #3 listed here, I demand author to check if singularity of center of the inner core (Earth) could avoid correctly because the spectral method approach done by Deguen and his colleague and Takehiro [2011; 2015] could avoid the singularity at the center of inner core with special technique. In other words, I demand author to check the validity of model comparing with results of Deguen and his colleague if the author's way to avoid the singularity at the center of inner core would be robust or not. I do not really think that the initial state of Earth's core should not have such a small particle as the author stated before inner core got started growing. As far as I understood, a numerical method used by the author (finite volume/difference

scheme) that could avoid the singularity at the center of sphere should be found in various literatures [e.g. a series of papers on core formation by Taras Gerya's group]. Please check them out and ask author to avoid the singularity completely. Otherwise, numerical results shown in this manuscript would NOT be quite RELIABLE for the community.

[Reply] About the extremely small virtual-sphere, I regret that the model description in the original manuscript misleads the readers. This is for the purpose of the computational convenience characteristic of the Yin-Yang grid, and this setup does not mean the existence of the real singularity that violates the mass and heat transport near the center of the model. I have explicitly mentioned this point in Section 2. Yes, I know that there are other theoretical models that avoid the mathematical singularity in the Earth's center. Just in case, I read the papers below by Gerya's and Takehiro's groups that reviewer kindly listed. A largest difference in the methodologies used in our 3D spherical code and Geryas' 2-D spherical code is that they used finite difference method based on a fully staggered rectangular Eulerian grid with a Lagrangian marker-in-cell technique for solving the momentum, continuity and temperature equations, whereas we used finite volume method based on a fully staggered spherical Eulerian gird for solving these basic equations by a primary variable method (Yoshida and Kageyama, 2004 in GRL; 2006 in JGR, Yoshida, 2008 in G-cubed).

- Gerya, T.V. & Yuen, D.A., 2007. Robust characteristics method for modelling multi-phase visco-elasto-plastic thermo-mechanical problems, Phys. Earth Planet. Int., 163, 1-4, 83-105. - Golabek, G.J., Gerya, T.V., Kaus, B.J.P., Ziethe, R. & Tackley, P.J., 2011. Rheological controls on the terrestrial core formation mechanism, Geochem. Geophys. Geosyst., 10, 11, Q1107. - Deguen, R., 2013. Thermal convection in a spherical shell with melting/freezing at either or both of its boundaries, J. Earth Sci., 24, 5, 669-682. - Deguen, R., Alboussière, T. & Cardin, P., 2013. Thermal convection in Earth's inner core with phase change at its boundary, Geophys. J. Int., 194, 3, 1310-1334. - Deguen, R. & Cardin, P., 2011. Thermochemical convection in Earth's inner core, Geophys. J.

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Int., 187, 3, 1101-1118. - Lin, J.-R., Gerya, T.V., Tackley, P.J., Yuen, D.A. & Golabek, G.J., 2009. Numerical modeling of protocore destabilization during planetary accretion: Methodology and results, Icarus, 204, 2, 732-748. - Takehiro, S., 2011. Fluid motions induced by horizontally heterogeneous Joule heating in the Earth's inner core, Phys. Earth Planet. Int., 184, 3-4, 131-142. - Takehiro, S., 2015. Influence of surface displacement on solid state flow induced by horizontally heterogeneous Joule heating in the inner core of the Earth, Phys. Earth Planet. Int., 241, 15-20.

6. Besides of comments on technical issues mentioned above, the first issue was how to address rheological properties of inner core material, i.e, iron-nickel alloy. Author used the simple temperature-dependent viscosity based on numerical mantle convection simulation but not quite sure if this type of rheological properties could be applicable for the inner core material or not. Please give appropriate references in the manuscript with some justification why the inner core rheology could be similar to that of the mantle one.

[Reply] I could not find appropriate references for rheology of the inner core material, i.e, iron-nickel alloy, except Karato's textbooks [2003; 2008], but The essence of the viscosity equation in Eq. (15) is just a simple relationship between the temperature and viscosity under the assumption that non-Newtonian rheology does not work in the iron-nickel alloy unlike in the rocky mantle I have explicitly mentioned this point in Section 2.

7. The second issue in this manuscript was a treatment of inner core growth model – Author assumed that the energy releases caused by latent heat and gravitational energy were IGNORED because they were secondary effects (see eq. (8)). To understand inner core growth itself, the energy balance should be SATISFIED even if they were secondary effects. Why did author ignore those two important energy resources in the core evolution system? I could not find it in the manuscript. Thus, I demand author to give sufficient justifications (explanations) on reason that those could be ignored because I could not get explanations or justifications in the manuscript.

[Reply] Yes, this issue is raised by another reviewer, who gave me appropriate references (below) required for discussion. First, "the heat source associated with solidification of the inner core are ignored because these effects play a secondary role in the growth of the inner core" in the original manuscript have been removed because there is a possibility to mislead the readers. Next, I have modified the relevant sentences in Section 2 to clarify that most of other studies kept these effect. For example, the modeling studies of the core evolution by Gubbins et al. (2003) and Nimmo et al. (2004) revealed that the latent heat plus gravitational energy is larger than the specific heat for the present Earth, and once the inner core starts to freeze, the core temperature decreases significantly with time, which has probably influence on the growth speed of the inner core and the generation and maintenance of geodynamo. The added references for discussion are as follows:

-Gubbins, D., Alfè, D., Masters, G., Price, G.D. & Gillan, M.J., 2003. Can the Earth's dynamo run on heat alone?, Geophys. J. Int., 155, 2, 609-622, doi:10.1046/j.1365-246X.2003.02064.x. - Nimmo, F., Price, G.D., Brodholt, J. & Gubbins, D., 2004. The influence of potassium on core and geodynamo evolution, Geophys. J. Int., 156, 2, 363-376, doi:10.1111/j.1365-246X.2003.02157.x.

8. Third issue is about the comparison between previous studies finding the 'translational regime' and author's model result. L.5 to L.13 of page 3228 was absolutely unclear to me. As far as I understood, the degree one convection generated from lateral viscosity variation such like 'Sluggish Lid Regime' would be alternative idea compared to the 'translational regime' (=this second form). As discussed the following paragraph (pointed out the next comments), the formation time-scale of degree one convection would be _3.0 Gyr from author's model simulations, which would be much longer timescale than the age of the inner core, thus translational regime would be STILL one great candidate for understanding to find out the large-scale seismic heterogeneity (anisotropy) observed in the inner core. Such a logical flow seems to be an ethic of self-denial, which means that this type of simulation of convection in the inner

core by the author would NOT be worth investigating. To get more worth doing, author should change his boundary conditions on both mechanical and thermal (similar boundary condition to other groups' studies instead of impermeable boundary condition) as well as laterally heterogeneous heat flux condition at ICB should be applied if mantle heat flux pattern could be transparent to the ICB suggested from 'top-down hemispherical dynamics'. If author could do these stuffs, author's results might be more comparable with other investigations to check if the 'translational regime' would be a strong candidate or not. Again, the author should avoid the singularity in his model at the center of the inner core.

[Reply] Again, thank you for your comment. On the lack and limitation of the present simple numerical model, in particular, boundary condition problem, I have further expanded the discussion in Section 4. Again, on the singularity problem, no worries as stated above. Next, about the sluggish lid regime and translational regime, the degree-one thermal structure of mantle convection that lies between the mobile-lid regime and the stagnant-lid regime. In 3-D spherical convection, the sluggish lid regime are replaced by degree-two or degree-one convection with increasing degree of the temperature-dependence of viscosity (e.g., Yoshida and Kageyama, 2004 in GRL, 2006 in JGR; McNamara and Zhong, 2005 in GRL). In this study, I show that the degree-one thermal/mechanical structure is only appeared for a limited range of parameter values for lateral viscosity variations. If the value of the total CMB heat flow is significantly larger than that used in this study, the possibility of the formation of degree-one structure by an endogenic factor due to rheological heterogeneity becomes less likely, because it takes c. 3.0 Gyr to form the degree-one structure. I have emphasized this point in new Section 5.

9. Forth issue is about the age of the inner core. Looking at Figure 3, the inner core has already grown 714.3 km at t = 3.0 Ga. It seems for author to integrate 0 to 4.6 Gyr in his model. This is very odd because, as quoted by the author, the age of inner core would be less than 1.3 Ga to 2.0 Ga with referring some literatures. Why did the author

decide to integrate over 4.6 Gyrs? To be more comparable for other studies, again, author should improve his own boundary condition. In addition to that comment, author ignored a bunch of literatures by S. Labrosse and F. Nimmo as well as by C. Davis. I do think that their accomplishments would be quite important for both evolution and dynamics of the inner core. To behave a fairplay in the scientific research, the author should not ignore those important literatures to cite in his manuscript. They are really IMPORTANT for discussing the thermal history of Earth's core and its influence to the inner core dynamics. Please cite them and add appropriate discussion.

[Reply] I totally agree your comment and regret the missing references on the young inner core age estimated from paleomagnetic and geodynamics studies. In Section4, I have expanded the discussion about this point and added new references below:

- Biggin, A.J., Piispa, E.J., Pesonen, L.J., Holme, R., Paterson, G.A., Veikkolainen, T. & Tauxe, L., 2015. Palaeomagnetic field intensity variations suggest Mesoproterozoic inner-core nucleation, Nature, 526, 245-248. - Davies, C.J., 2015. Cooling history of Earth's core with high thermal conductivity, Phys. Earth Planet. Int., 247, 65-79. - Labrosse, S., Poirier, J.-P. & Mouël, J.-L.L., 2001. The age of the inner core, Earth Planet. Sci. Lett., 190, 3-4, 111–123. - Nimmo, F., Price, G.D., Brodholt, J. & Gubbins, D., 2004. The influence of potassium on core and geodynamo evolution, Geophys. J. Int., 156, 2, 363-376.

10. Additional idea for understanding the seismic heterogeneity in the inner core would be caused by the magnetic field – Please check literatures by Takehiro [2011; 2015 both in PEPI] and Lasbreis et al. [2015 in GJI] and author should discuss effects of magnetic field with his simulation results without the magnetic field effects. Here I listed 10 significant and critical points that author should address getting the revised manuscript. Since I think that a bunch of additional works and model validations should be done by author to get the revised version of manuscript and it would take a long time, I would not give any detailed comments here (line-by-line comments). Nevertheless, those comments would be useful for getting revised manuscript done by the author.

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[Reply] Thank you very much for your comment. As pointed out by the reviewer, some authors suggested that the hemispheric seismic heterogeneity in the inner core would be caused by the magnetic field (Takehiro 2011; Lasbleis et al. 2015; Takehiro 2015). The effects of magnetic field on the 3D numerical model of thermal convection in the growing inner core should be investigated in future. As a start, we are developing a new 3-D numerical simulation code for multiple-layered thermal convection with a large viscosity contrast among each layer, considering the methodology used in the present study and several issues discussed in this Section. The relationship between the degree-one thermal convection and characteristic of geomagnetic field may be an interesting topic not only in earth science but also in computational fluid dynamics. I have expanded the discussion in Section 4 and, following your suggestion, I have cited the following papers:

- Lasbleis, M., Deguen, R., Cardin, P. & Labrosse, S., 2015. Earth's inner core dynamics induced by the Lorentz force, Geophys. J. Int., 202, 1, 548-563. - Takehiro, S., 2011. Fluid motions induced by horizontally heterogeneous Joule heating in the Earth's inner core, Phys. Earth Planet. Int., 184, 3-4, 131-142. - Takehiro, S., 2015. Influence of surface displacement on solid state flow induced by horizontally heterogeneous Joule heating in the inner core of the Earth, Phys. Earth Planet. Int., 241, 15-20.

[Reply] I again deeply appreciate you for the careful reading and significant improvement of this paper. I will accept all of the valuable reviewer's comments with sincerely and would like to use these comments to improve the numerical model in future.

Masaki Yoshida

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/7/C1943/2016/sed-7-C1943-2016-supplement.pdf

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