

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

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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 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

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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 understanding the large-scale seismic heterogeneity and anisotropy observed in the inner core but this study would be somewhat interesting and important step for the community. 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 citations 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 procedure, cite various important literatures and add some important discussion on the age of the inner core, I would re-consider my recommendation. Again, currently, this manuscript SHOULD NOT be published in anywhere.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 additional 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.

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.
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.
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

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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.

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.

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].

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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.

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.

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.

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 time-scale 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

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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.

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.

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

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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.

Interactive comment on Solid Earth Discuss., 7, 3817, 2015.

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