To the Reviewer #2

Many thanks for the referee's valuable comments and his/her time spent in reviewing our paper (SE-2016-12). I would like to mention that in the revised paper, all the points have been taken into consideration. Following, I refer the comments made by the referee with their corresponding answers as italic font. The changes are highlighted in the revised paper (in yellow color).

1. Lack of literature review. The authors give a very rather incomplete review of previous work on mantle convection, for which an enormous amount of literature exists (interestingly they miss seminal work by McKenzie, Christensen, Tackley, Moresi, Solomatov and many others). I strongly suggest that they read up on the topic, for example by reading textbooks by Schubert, Davies, or some of the many review papers (by Tackley, Bercovici, Ricard, many of which are available online). As the current paper deals with viscoelastic convection, I had expected at least a complete review of existing work on this topic which is however also not the case, which shows a rather large ignorance towards previous work.

Response: *Due to the too number of literature on mantle convection, we decided to present only the best-known and related papers in the literature section of our paper. We have classified the collection of articles and books that we have access to them based on the main purposes of our work, i.e. Newtonian/non-Newtonian medium, the constitutive model for the viscosity, temperature- and depth dependency of viscosity model, geometry …. In addition, all the mentioned works by the reviewer have used the Maxwell constitutive model which is a linear viscoelastic model. It is important to remember that the Maxwell that is not able to predict the nonlinear viscosity. According to the reviewer's comment, we modified the paper by reporting these work in literature (refer to pages 4 & 5 of the revised paper).*

2. Gravity. Some of the other reviewers were a bit annoyed of your use of a depth-varying g, yet your polynomial does indeed reproduce the depth dependent effect of g on Earth, if I plot it on MATLAB, using the following lines:

» y=[1:3000]/1000; » g=-0.118*y.ˆ6+ 0.602*y.ˆ5 - 1.006*y.ˆ4 + 0.6884*y.ˆ3 - 0.3708*y.ˆ2 + 0.167.*y - 9.846; » plot(g,y*1000), axis ij

The point is, however, that the variation of g within the Earth's mantle is very minor which your expression also shows, which is why it is usually assumed to be constant.

Response: *We checked data of Bullen (1939) and Dziewonski & Anderson (1981) and found that both of data are mostly similar with reasonable confidence. Both of data predict that the maximum variation of gravitational acceleration in mantle is about 6.8%. Although its variation is small, utilization of a depth-dependent gravity would lead to more realistic results. It is important to mention that Eq. (13) is dimensionless and we should not use it based on the meters of depth. In this Equation,* $y = \tilde{y}/H$ *is the dimensionless depth of the bottom plate and using the dimensionless depth (y), the formulation is completely correct.*

3. Employed constitutive relationships. All above mentioned papers employ linear viscoelasticity, for the simple reason that there is not much data to support the use of more complicated elasticity models for applications on the scale of a convecting mantle (apart maybe from using a Kelvin body for bulk deformation). One can of course come up with arbitrary complex constitutive relationships but if there is no data to back it up you are not modelling a problem that is geoscientifically relevant. It is unclear to me why the Giesekus model should be relevant for geoscientific applications and you don't give a justification for that which implies that your paper is simply irrelevant for geoscientific purposes.

Response: *This question is similar to comment#1 of the first reviewer that is answered in detail.*

4. Viscous dissipation Different than what you claim, viscous dissipation has been studied in the context of mantle convection models since at least the mid-80ies (please look at classical papers by Christensen and the above mentioned textbooks). Importantly, you should include adiabatic heating if you include viscous dissipation as they are of similar order of magnitude. You don't do that here.

Response: *Thank you for your valuable comment. In some studies, the effect of viscous dissipation on boundary condition has been considered as a small constant heat flux (instead of adiabatic boundary condition). Due to the small effect of viscous dissipation in scale of problem, the heat flux resulted from viscous dissipation is negligible so it can be removed* *from the boundary condition. Therefore, in the viscous dissipation is just considered in the numerical domain (and not the boundary condition)*

5. Inertial terms Mantle convection is low Reynolds number fluid flow; yet you include inertial terms in your formulation. You can use that to model mantle convection but you would have to employ a very small timestep; moreover, you should first demonstrate that your code actually reproduces normal viscous convection for example by reproducing the Blankenbach benchmarks.

Response: *Thank you for your indication. The advection term is negligible in our CFD code and due to using the dimensionless form of governing equation; the time step is dimensionless so, the small dimensionless time cause no problem on convergence (The real time step is large enough). The code is also verified by comparing the results with previous solutions (refer to section 4.1 and Fig. 2) and the grid study is done to find the proper mesh and time step.*

6. Missing figures the uploaded pdf has no figure which makes it impossible to review it. Yet, from the text alone it is clear that this work is currently very far removed from being publishable in an Earth Science journal.

Response: *We are sorry for this problem. We sent the Figures separately during the first submission and unfortunately, there were not uploaded in the website by the office of Journal. After one week from submission, the Figures have been uploaded and in the revised version, we inserted the figures immediately after the manuscript to avoid any problem.*

Best,

M. Norouzi