差出人: **Geophysical Journal International** onbehalfof@manuscriptcentral.com
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Dear Mr Nishida

We have received reviews of your manuscript GJI-S-20-1179 entitled "Investigation of dipolar dominancy in geodynamo simulations with different inner core sizes". The handling Editor, Prof. Richard Holme, has carefully considered these reviews and the manuscript, and has consulted other members of the Editorial Board. On balance, the Board feels that, whilst elements of the material are appropriate for publication in Geophysical Journal International, significant additional work is needed. The extent of the required changes are likely to be greater than can be reasonably described as "major revision", so the Editors have taken the view that the manuscript should be revised substantially and resubmitted as a new manuscript. Should you decide to resubmit, please add a detailed response letter addressing the reviewers' concerns.

The editor offers the following additional comments: Editor Prof. Richard Holme
Comments to the Author:
Two broadly positive reviews, and yet I have recommended revise and resubmit. This seems harsh, but there are some details within the reviews that raise concerns related to publication in Geophysical Journal International. First of all, GJI focusses on work with Geophysical application. As noted by one of the reviewers, the Ekman number is large to make performing simulations easier - however, this also results in the study being even further from Earth conditions than other current work. This was state of the art 10-15 years ago, but now, in order to at least be able to claim Earth-like results, a lower Ekman number is necessary. Secondly, what are the implications for the geomagnetic field? Most studies now place the origin of the inner core of order 1 billion years ago. What change does the arrival of the inner core make to the observable field? What change might it make to the energetics, and so the evolution of the Earth system?

Your work has intrinsic value in terms of an extension of the parameter regime of dynamo studies, but if this is its significance it must be phrased in this way. Also, if it doesn't have direct Geophysical application, another journal might be more appropriate - perhaps GAFD. If you wish to publish a non-cutting edge study in GJI, it must really be related to what implications it might have for the geophysical system. Only if you feel you can convince me that your paper achieves this should you resubmit to GJI with the appropriate changes - otherwise, another journal is more likely to provide an appropriate home for your work.

When a paper is rated revise and resubmit for GJI, it is considered by at least two members of the editorial board. In this case, the second editor is Prof Andy Biggin, who writes:

Upon reading the title of this manuscript, I was excited to see what it said. Unfortunately, I must agree with the judgement of the first editor that this study is borderline reject for GJI on the grounds of its questionable geophysical significance. While the topic itself is certainly suitable, the approach does not seem to quite hit the required mark and the authors must think carefully over whether to try to address both the editor's and the reviewers' comments in a resubmission or rather resubmit to a more specialised dynamo-facing journal.
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I apologise for the disappointment this decision may cause, and wish you luck driving the work forwards towards publication, either with GJI or with another more appropriate journal..

I recognise that this news will be a disappointment to you. The editors hope that you will take this feedback in the positive spirit intended, that you will undertake the additional work and revision suggested, and that we will see an improved and publishable account of this research at Geophysical Journal International in a few months' time.

Regards,

Louise

Louise Alexander
Assistant Editor
Geophysical Journal International

cc: all listed co-authors.

Reviewer: 1 Anonymous

Comments to the Author(s)
The submitted paper reports a set of numerical dynamo simulations which attempt to investigate the magnetic dipolarity with varying aspect ratio (different inner core sizes). The suite of simulations show that dipolar dominance is achieved for aspect ratios ri/ro=0.25, 0.35. The methods of evaluating the dipole dominance make for an impressive tool to address the title question.
The authors show that dynamo action with a small aspect ratio requires a large Rayleigh number, consequently, the different aspect ratio simulations do not sample the same range of supercriticality values. This makes it difficult to separate the aspect ratio dependence from the supercriticality dependence – I suggest 2 more simulations at ri/ro=0.25,0.35 with larger Ra/Rac to connect with the ri/ro=0.15 case. Finally, I think that the discussion needs to clarify where the authors can/cannot distinguish between the Ra/Ra_c and ri/ro dependencies on the output diagnostics.
The paper is logically structured with the results obtained appearing to contain enough novel material to make it suitable for publication in Geophysical Journal International once the major concerns discussed here are addressed. Furthermore, I think this work will be of interest to a sizeable subgroup of the journal’s readership. I recommend some revisions before publication and these are listed below.

Major Comments
A1: Throughout the manuscript the authors interpret their results in terms of the aspect ratio as the title suggests. However, due to the range of Ra/Rac sampled for each ri/ro it seems difficult to separate the parameter dependence of these two quantities. The authors could address this by running two additional simulations with ri/ro=0.25,0.35 at Ra/Rac values between 10 and 15 to allow a direct comparison with the ri/ro=0.15 case. The interpretation of the results should then be updated throughout the manuscript. See minor points listed below where I identify sentences in the manuscript for which I find the statement of an underlying ri/ro dependence unconvincing given the current results.
A2: Conclusions. The geophysical implications and interpretation need to be better described. Yes, the geometrical effect is important to determine but when considering geological time the mode of convective driving also changes (e.g. internal versus bottom heated convection, CMB heat-flow patterns, etc.). These should be highlighted explicitly to better aid the geophysical connection of how the simulations can be relevant to Earth's core.
Out of necessity the numerical simulations are ran at a rather moderate value of the Ekman number (E=1e-3). This does indeed allow a systematic survey, however, I suggest that this be put into the context of the parameter regimes outlined in Gastine et al. (2016) and Long et al. (2020). At E=1e-3 the simulations do not sample the rapidly rotating regime which suggests that they are not governed by the force balance relevant to the Earth (see also e.g. Schwaiger et al. 2019).
(Gastine et al. (2016) use a large aspect ratio but fixed temperature boundary

(Gastine et al. (2016) use a large aspect ratio but fixed temperature boundary conditions, whereas, Long et al. (2020) use a present day Earth aspect ratio, gravity profile but use fixed heat-flux conditions)

Minor Comments
B1. Lines 93-97: Is the observation of Heimpel et al. (2005) robust or was it only observed at fixed Ekman and Rayleigh number? It is worth explicitly stating.
B2. Lines 134-135: Do the authors motivate the choice of these three inner core sizes? Are these geophysically motivated or arbitrarily chosen?
B3. Equation 6: Are g_o and \kappa_T defined anywhere?
B4: Line 168: “For the time steps …” should read “For the time stepping, …”
B5. Line 173: Should “sectorial” be “sectoral”?
B6. Line 182: A statement of numerical convergence should be included; how did you ensure spatial and temporal resolutions/durations were sufficient? e.g. did you check energy conservation?
B7. Lines 204-205: Always use standard form. The 6% difference in Rac value with A-Shamali et al. (2004) for ri/ro=0.15 seems quite large. This might be fine but worth checking.
B8. Line 221-224: How was the averaging period of 0.5 diffusion times determined to be sufficient?
B9. Lines 225-227: No need to describe markers in main text, this belongs in the figure caption and/or legend itself.
B10. Line 237: Can the authors comment on why these two simulations are different to the other cases? Any description or observations would add to the interpretation.
B11. Lines 254-259: The authors claim that the difference in the convective flow and the generated magnetic fields is due to the different aspect ratios. However, the contours for ri/ro=0.15 are shown for a much higher Ra/Rac than the other cases – at higher Ra/Rac one typically expects more chaotic and unstructured flow. I think the discussion needs to explicitly separate these two dependencies. (This difference may fit with the regimes mentioned in major comment A2.)
B12. Lines 284-286: The value of fdip looks to depend on Ra/Rac so stating the tendency is different only for ri/ro seems inappropriate. If larger Ra/Rac runs with ri/ro=0.25,0.35 are performed they will explicitly show if this dependence depends solely on Ra/Rac.
B13. Lines 305-310: Similar to before, does the f_mag_fit data collapse for Ra/Rac >= 10, independent of aspect ratio? The two additional simulations mentioned in major comment A1 would also help to address this.
B14: Lines 369-371: Table 4 states that none of the ri/ro=0.35 cases have an fdip<=0.35. I don’t think the authors can say their results are consistent with previous work or that they verified the dipolar-nondipolar transition. This claim needs to be reworded or removed.
B15: Figure 2: I suggest adding a second panel showing a failed dynamo simulation.
B16. Figure 3: I suggest making the x-axis the same for all three plots.

Additional references not in the manuscript
Gastine, T., Wicht, J. and Aubert, J., 2016. Scaling regimes in spherical shell rotating convection. Journal of Fluid Mechanics, 808, pp.690-732.
Long, R., Mound, J., Davies, C. and Tobias, S., 2020. Scaling behaviour in spherical shell rotating convection with fixed-flux thermal boundary conditions. Journal of Fluid Mechanics.
Schwaiger, T., Gastine, T. and Aubert, J., 2019. Force balance in numerical geodynamo simulations: a systematic study. Geophysical Journal International, 219(Supplement_1), pp.S101-S114.

Reviewer: 2 Anonymous

Comments to the Author(s)

General Comments

This paper explores the effect of spherical geometry (ratio of inner/outer boundary radius) on magnetic dipolarity in numerical dynamo simulations. The application is the nature of the geomagnetic field over the time from after inner core nucleation to the present. The radius ratios used here (namely 0.15, 0.25 along with the the present day Earth core radius ratio 0.35) were studied in previous dynamo models (Heimpel et al, 2005) and are cited in this work. The novelty of this work is a focus on bipolarity for radius ratios that are smaller than 0.35. The analysis of dipolarity has been extensively studied in previous work, some of which is appropriately cited here.

If anything can really be called new in this work it is the dipolarity analysis of the 0.15 and 0.25 radius ratio cases. This may be of some interest to the geodynamo community.

However, the parameter space of the numerical simulations has been studied previously and extensively, and unfortunately the simulations are of relatively low resolution, given modern computing capabilities. Indeed, the fixed Ekman number of 0.001 is arguably too viscous to give Earth-like behaviour (e.g. Christensen , PEPI 187, 157, 2011). Also, many studies dating from the 1990's to 2000's have used higher resolutions, and explored much larger ranges of parameter space (e.g. Kutzner and Christensen, 2002).

Specific comments

The discussion of failed dynamos at high Ra is not explained very well. Comparing with previous work, (e.g. Kutzner and Christensen, 2002), it is clear that failed dynamos occur for Ek = 0.001 at the boundary between dipolar and multipolar dynamos, which occur at even higher Ra. However, these failed dynamos do not occur at higher resolution (i.e. lower Ek), and it is not obvious that they should occur at lower radius ratio, even at Ek = 0.001.

A failed dynamo is found at radius ratio 0.25 here, but not at 0.15. Does that mean that the transition to multipolar dynamos exist only for radius ratios greater than 0.15? Why did you not do runs for higher Ra at radius ratio 0.15? This radius ratio is arguably the most interesting in terms of the relationships between Ra, dipolarity, and the transition to multipolar dynamos. It seems that more computation is necessary to make these relationships clear.

It is argued at the end of the paper that more simulations should be carried out for different boundary conditions, and for dynamos with no inner boundary. However, you fail to cite key papers on this topic. There are numerous papers on the effect of different boundary conditions (e.g. Heimpel and Evans, PEPI, 224, 124, 2013). The effect of no inner core was studied by others, including Landeau et al. (EPSL 465, 193, 2017).