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Title: Mean-field interpretation of star-in-a-box simulations of red giants

Author(s): C. A. Ortiz-Rodríguez, A. Brandenburg, P. J. Käpylä

Dear Ms. Ortiz-Rodríguez,

Your paper was submitted to a referee who recommends publication after revisions (see the enclosed report).

Kindly make the requested revisions and send us the revised version via

<http://nestor.aanda.org/aa/user/article/aa61979-26>.

In your cover letter, please list all the changes made (including new or removed coauthors), and mark the changes in your manuscript using boldface or colored text. However, please note that your source files should only include the "clean" revised version. A separate pdf with the marked changes (bold or diff file) should be uploaded as an "optional file."

Your revised version should be submitted within two months. Should the revisions require more time, please contact the editorial office to request an extension.

Yours sincerely,

Lisa Bugnet
A&A Associate Editor

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Referee report(s) on this version:

Report by referee 1:

This manuscript presents an interesting comparison between mean-field dynamo models and star-in-a-box simulations of magnetic field generation in red giants, which is of keen interest to the community. The paper is generally well written and the methodology is described mostly clearly. However I have a number of comments before I can recommend this manuscript for publication in Astronomy & Astrophysics.

§1 provides a good overview of the context with precise references and descriptions of previous works. However, the distinction between surface and deep magnetic fields for red giants could be made clearer. I also recommend adding a concise outline at the end of the introduction.

§2 describes the method, which relies on a comparison between star-in-a-box simulations and mean-field dynamo simulations. The set-up should be made more precise; most importantly, the stellar properties used for the Red Giant should be made explicit.

§3 presents the results of the study, with several figures and descriptions. It is clear overall and Table 4 provides an interesting and concise summary of the comparison. I have a few

comments on the presentation and notations, and I suggest including some physical discussion of the results.

§4 is a clear conclusion and discussion of the results. It would be beneficial to include a comparison with observations of surface magnetic fields in red giants.

General scientific comments

Paragraph 1

The introduction provides a fairly good overview of the context with some precise references. However,

1. I.1, and in all the manuscript. A clearer distinction between the surface magnetism in the envelope, and the deep internal magnetism of fossil origin, together with the related asteroseismic observations in would be beneficial to readers. (e.g. Fuller et al. 2015, Loi et al. 2021, Bugnet et al. 2022, Li et al. 2022, Deheuvels et al. 2023)

Thank you very much for this point. We now make a clear distinction between our work, which is focused on studying the magnetic field generation through the dynamo mechanism in the envelope, and the magnetic fields inferred from asteroseismic analyses; see lines 46-53 on page 1.

2. P.1, bottom right: Earlier works such as Noyes 1984 (Chromospheric emission Ca II), and other proxies such as Sph (Mathur et al. 2014, Santos et al 2026) might be cited here. Thank you for suggesting this literature. We added the Noyes 1984 citation before Lehtinen 2020. We added Mathur et al., 2014 and Mathur et al., 2025, and mentioned the relation they found between the Sph and Rossby number for F stars. However, we could not find the work by Santos et al. 2026, we found Santos, A. R. G., 2025, A&A, 698, L23. That work focuses on the subgiant beta Hyi, but it uses spectropolarimetry and X ray data rather than Sph. Therefore, we do not include it in the introduction. We found Mathur et al. 2025, which uses the photometric activity index Sph.

PJK: Maybe the referee meant this:

<https://ui.adsabs.harvard.edu/abs/2025A%26A...698L..23S/abstract>

PJK: I think we don't need to discuss the F stars in the paper as much as we do now

COR: Yes, I have shortened that sentence and only mention the magnetic index as another method for characterising magnetic fields. AB: at the moment, it says we didn't find it. COR: Now the answer is complete.

3. I. 80 The Lamba effect is not the only proposed mechanism at play in the angular momentum distribution. Mentioning the other possible mechanisms extracting angular momentum from the radiative zone (internal waves, mixed modes, magnetic instabilities,...) would be appropriate here following your paragraph on the asteroseismic observations.

That is important to point out; thank you very much. For completeness, we now mention additional mechanisms that could contribute to angular momentum transport. We discuss the role of mixed modes and mention magnetic fields, meridional circulation, and hydrodynamic instabilities, which are the mechanisms discussed by Aerts, C., et al. 2019, ARA&A, 57, 35.

Paragraph 2

4. While this manuscript is centered on the magnetic field of red giants, nothing is said about the stellar model used in the simulations. Do you use a stellar model from an evolution code? What are the stellar properties? Are the models the same in the mean-field and star-in-a-box simulation? How does the surface magnetic fields in your simulations compare with existing observations?

Thank you for noticing the lack of information on the stellar model. As a reference, we used a MESA model for a red giant at the bottom of the red giant branch. We added a new subsubsection for stellar parameters and a short sentence about the choice of geometry in the mean-field model. Added details about the stellar model.

There are a few works that report the magnetic field surface topology of red giants. For instance, Aurière, M. et al. 2021, A&A, 646, A130, focused on the red giant Pollux. They extracted the Stokes parameters and performed Zeeman-Doppler imaging to obtain the surface magnetic field topology, finding a dominant poloidal component with dipolar topology which is inclined 10.5 degrees with respect to the rotation axis, and a strength of 0.44 Gauss. As Pollux is a slow rotator, with ~660 days of rotation period, the authors point at the possible role of turbulence for dynamo generation (Lehtinen 2020). Our mean field runs A_0 and C_0, without rotation rate and positive alpha effect, show a dominant dipolar topology that does not change with time, indicating the presence of an α^2 dynamo. This supports the suggestion of Aurière, M. et al. 2021, A&A, 646, A130 about the importance of turbulence for magnetic field generation.

Additionally, as mentioned in the introduction and in our conclusions, Tsvetkova, S., et al. 2013, A&A, 556, A43, reported a poloidal component with dipolar topology for Beta Ceti, a giant with 215 days of rotation period.

V390 Aur is a red giant in the rapid rotating regime, with around 10 days of rotation period. Konstantinova-Antova, R. et al. 2012, A&A, 541, A44 reported a poloidal component with dipolar topology, in addition to a high-latitude toroidal belt. They interpret this magnetic field as a result of an alpha-Omega dynamo. Our mean field runs with alpha Omega approximation require a minimum shear for the magnetic field to become supercritical, highlighting the importance of the differential rotation for the alpha Omega dynamo. In the case of our 3D runs, our rapidly rotating cases B10 and B20, show less clear structures in their axisymmetric magnetic field, suggesting a transition towards non-axisymmetric modes. This may reflect the complex magnetic field observed in the rapid rotator V390 Aurigae. We have added these references and discussion as part of the conclusions of our revised manuscript.

5. What are the benefits of using star-in-a-box simulation compared to other set-ups?
As mentioned in the manuscript, in the star-in-a-box setup, the star is fully embedded in the box; the advantage is that there are no singularities on the axis or at the centre of the star. Additionally, the boundary of the star is contained within the domain, allowing the magnetic field to evolve more freely.
6. In addition to the distinct magnetic field configurations, runs B1, B2, B5 have a smaller B_{rms} value ($B_{\text{rms}} \approx 0.45 B_{\text{eq}}$) than the last two simulations ($B_{\text{rms}} > 2B_{\text{eq}}$), this could be commented on.

Yes, this is commented now at the end of the first paragraph of 3.1.

Paragraph 3

7. In Fig. 3, could you give details about the sources of the fit uncertainty? What defines the upper and lower values of the grey zone?

About Figure 3: The fit corresponds to a power-law $C_{\lambda} \propto Co^{\alpha}$, obtained using least-squares linear regression with `numpy.polyfit`, degree 1, applied to the data in log-log space. The band is the $\pm \sigma$ uncertainty of the log-log fit, obtained by combining in quadrature two contributions: the propagated uncertainty of the fitted slope and intercept, and the rms scatter of the data points about the fitted line.

8. I. 292 “The axisymmetric magnetic field shows small-scale fluctuations and no clear cyclic behaviour” Are there some comparable studies in the literature which highlight the disappearance of cycles in this regime?

PJK: I think that we can shorten this and simply state that a transition from predominantly axisymmetric and often cycling dynamos to non-axisymmetric happens in many simulations at rapid enough rotation and mention Viviani et al. (2018), Käpylä (2021) and Navarrete et al. (2022) as examples. Since the referee refers to the rapid rotation regime, I don't think we need to talk about the slowly rotating anti-solar DR regime.

COR: yes, now it shortened and only most relevant references are mentioned.

As discussed in the review by Käpylä, P. J., et al (2023). Space Sci. Rev., 219(7), 58; the dynamo regimes vary from model to model, and differential rotation plays an important role. For main-sequence stars, solar-like differential rotation is accompanied by large-scale axisymmetric and cyclic fields, and when rotation is increased ($Rossby \ll 1$), the cycles are either maintained or disappear by the emergence of non-axisymmetric modes.

A transition from axisymmetric and cyclic solutions at slow rotation to non-axisymmetric dynamos at rapid rotation was also found in different simulations (Viviani, M., 2018, A&A 616, A160; Käpylä, P. J., 2021, A&A, 651, A66; Navarrete, F. H., 2022, A&A, 663, A90).

In particular, Viviani, M., et al, (2018). A&A, 616, A160 performed simulations using a spherical grid and varying rotation rates from 1 to 31 times the solar value. They found a transition between axisymmetric and non-axisymmetric at 1.8 times the solar rotation rate. Using a similar setup, Navarrete, F. H., et al. (2022) simulated a solar-mass star rotating at 20 and 30 times the Sun's rotation rate, finding mainly non-axisymmetric dominant modes.

9. In Fig.4, the magnetic field is stronger around $t=2$, and then between 3.4 and 4, is there a physical explanation to this?

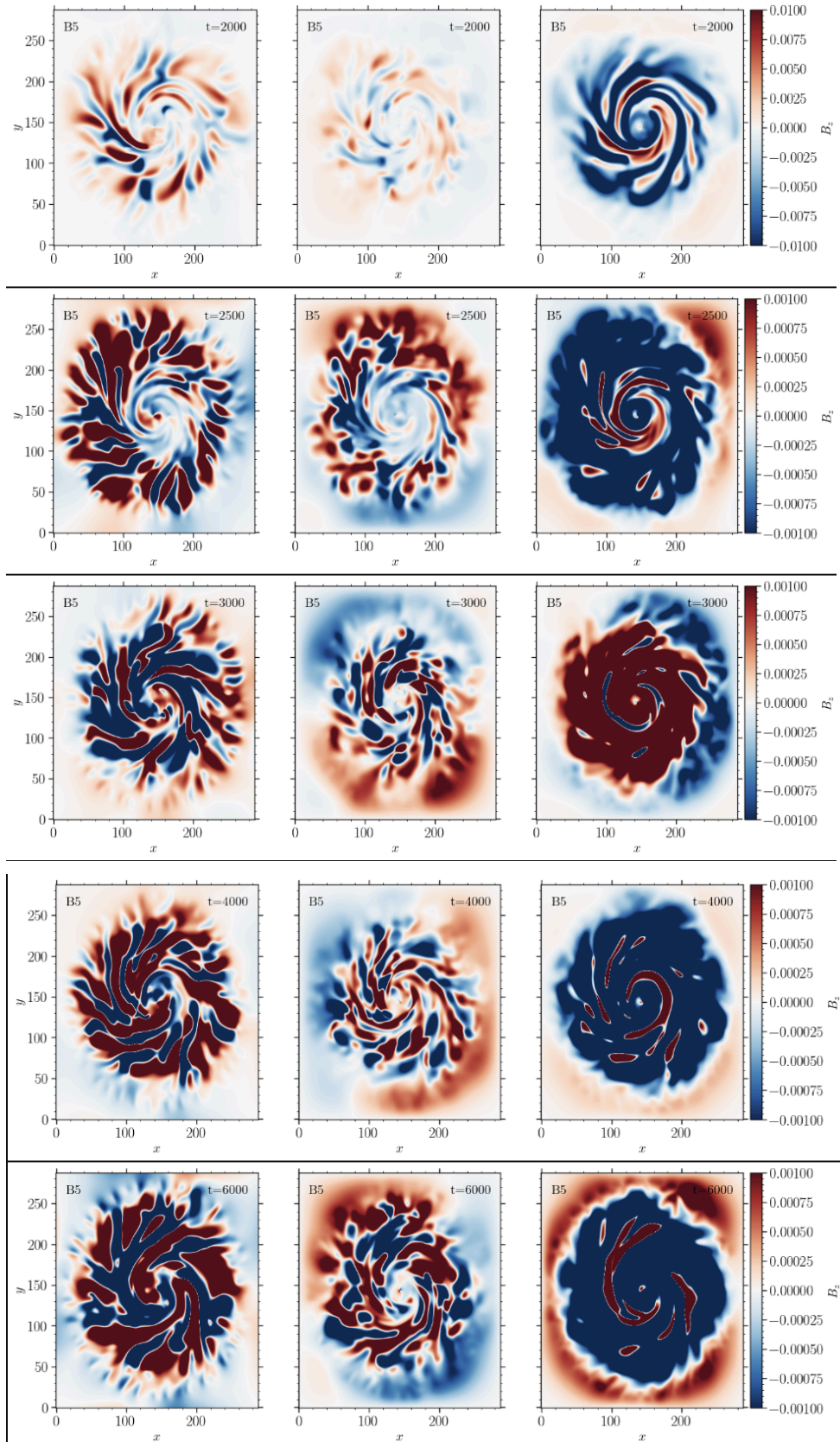
If referring to Run B5, when checking the time evolution of the velocity field and kinetic helicity, they coincide with the weak and strong field episodes, with no clear suppression due to back reaction of the field on the flow. This suggests that the magnetic field variations are related to the variations in the flow, rather than to a quenching dominated by the Lorentz force.

PJK: My hunch here is that this run is near the transition to non-axisymmetric large-scale fields and during the times of weak axisymmetric fields (as in Fig. 4) the non-axisymmetric large-scale field is stronger. You can check this from snapshot if you have enough saved.

COR: The figure below shows B_{sphx} , B_{sphy} , B_{sphz} from left to right at different times (the plot is saturated so structures can be seen).

I would say the structures are coherent, but there are signs of non-axisymmetry, mostly in the upper layers. The minimum of the axisymmetric fields are from 1000 and 2000 time units and

again from around 5000 time units. The last row shows the time when the axisymmetric fields are in its maximum, and indeed the sphx component looks more axisymmetric (bottom left) than in the previous times. What do you think?



10. Are there some relevant comparisons between your work and that of Noraz et al. 2022 (or other similar set-ups)?

The work by Noraz, Q. et al. (2022) A&A, 658, A144 focuses on solar-type stars, considering a domain with $0.6 < r < 1.0$ and $0 < \theta < \pi$. They consider a portion of the radiative zone ($0.6 < r < 0.7$), while the rest corresponds to the convective zone; the domain of our setup considers a deep convective zone ($0.2 < r < 1.0$), which corresponds to an evolved star. Another relevant comparison is that they focus on two different rotation profiles, using an analytical fit to obtain both solar-like and anti-solar differential rotation profiles, while we focus on the rotation profile extracted from the 3D-modeled red giant. This profile shows a core that rotates more rapidly than the envelope. In terms of dynamo mechanism, Noraz et al. consider the Babcock-Leighton flux transport model, which we do not consider, while we explore α^2 , $\alpha^2 \Omega$ dynamos, absent from their study. Both works consider $\alpha \Omega$ dynamo and find cyclic reversals of the magnetic field near the surface.

PJK: Noraz et al. conclude that traditional turbulent dynamos can sustain cycles for anti-solar DR whereas Babcock-Leighton models in general do not. We consider only turbulent dynamos and also find cyclic solutions in our mean-field models, in agreement with our 3D simulations.

PJK: Do we mention this comparison in the text?

COR: Now it is included in the conclusions.

11. I.341. How is the weight factor computed here?

Thank you for this question. The weight factor was probably an incorrect way to phrase it, as C_α was calculated as the sum over the N grid points within the convective zone. We updated the equation and text accordingly.

PJK: Maybe the referee is referring to the $\cos\theta$ -factor in the equation

COR: They were asking about $w(r, \theta)$ defined as the spatial weighting factor in the old version; now that the equation is corrected, we no longer have $w(r, \theta)$.

12. Fig. 12 The slopes seem to be rather similar, whereas most of the change in the dynamics appear close to start time. What is the physical explanation?

The change in the dynamics at the beginning of the runs can be interpreted as the effect of shear reorganising the field. Strong shear makes the field settle at a higher level and decay more slowly than in the runs with weaker shear. However, the non-rotating case decays at the lowest rate and settles at a lower level.

13. Fig. 13. Is the cycle period similar in the last row ? How does it compare to the period of the 3D simulations? Are there some physical intuition for this?

Yes, by eye inspection, the cycles have similar periods of around ~ 10 -11 reversals per diffusion time, which means ~ 5 cycles per diffusion time; therefore, the cycle period would be around $1/5$ diffusion times. In the case of the 3D runs that show cycles (B1, B2, and B5). The cycle period decreases with increasing rotation rate, ranging from ~ 1.4 diffusion times for run B1, to ~ 0.25 in B2, and ~ 0.06 in B5. This reflects the influence of rotation on the cycle period for the 3D runs, while the period in the mean field runs with $\alpha \Omega$ is roughly constant across the different imposed shear, comparable to the period of run B2.

PJK: Shouldn't the cycle period in $\alpha \Omega$ dynamos should be proportional to $C_\alpha \Omega^{0.5}$ in α remains constant?

14. What are the values of the surface B fields in your simulations in both set-ups, and how do they compare with the observations you mention in the introduction? This point should also be emphasised in the conclusion.

COR: how can we compare the magnetic field strengths of our simulations if we consider B_{eq} ? PJK: We can use the conversion factors to physical units for the 3D simulations. In the mean-field runs the saturation strength depends on the form of the alpha-quenching. How did we choose that (I believe I don't have access to the runs themselves)?

COR: Yes, I can add an extra column for the magnetic field strength of the 3D runs in Table 1, which allows for comparison with the observational works mentioned in the introduction. In the mean field runs, we use the default case for alpha_quenching:

```
case default;
  Beq21=1./meanfield_Beq**2
endselect
alpha_quenching_tmp=alpha_quenching
alpha_total=alpha_total/(1.+alpha_quenching_tmp*p%b2*Beq21)
```

We use $\alpha_{quenching} = 1$ in `run.in`, and $B_{eq} = 1$, so we directly get B/Beq .

I added one column to the table in Appendix 1 with $Brms/Beq$, so we can compare the mean field runs values with the 3D ones.

You can access to all the run local input and source files from the zenodo link:

<https://doi.org/10.5281/zenodo.21361381>

Comments on the writing

15. I. 41 "The authors calculated the Rossby numbers to be 0.08 and 0.19 based on the convective turnover time half a pressure scale height above the base of the convective zone and in the middle of it, respectively." This sentence would gain in clarity if rephrased or shortened.

Yes, the phrase has been shortened to "The authors calculated the Rossby numbers to be 0.08 and 0.19 at two different depths of the convective zone."

16. I.96 The standard reference for the CO5BOLD code should be mentioned.

Yes, now it is mentioned.

17. It would also be valuable to add the values of the rotation rate and the differential rotation in Table 1, as well as the other dimensionless numbers introduced Pr_{SGS} , Pr_M . The numerical resolution of the simulations and simulation time should also be stated.

$Pr_{SGS} = Pr_M = 1.0$; they are given in the caption of Table 1. We added the resolution of the 3D runs in the caption of Table 1. The rotation period in days is given in a new column in Table 1.

COR: it is not clear what they refer to with differential rotation, but I added in the caption that the core rotates faster than the envelope in all 3D runs.

PJK: We can state the amplitude of the relative differential rotation, e.g., $(\max(\Omega) - \min(\Omega))/\Omega_0$

COR: yes, I added a new column in Table 1 with the amplitude of the relative differential rotation.

COR: what do they mean by simulation time? Real running time?

PJK: We can state the length in convective turnover times or in days/years if we convert to physical units

18. In Eq (1)-(4), ν should be introduced.
 ν is the kinematic viscosity, and it is introduced after Eq. 5.
19. In (4) typo with the parenthesis
Thank you for noticing it, now it is removed.
20. I. 139 I suggest replacing “box” with “Cartesian box” here for clarity.
Yes, now it is changed.
21. I.167 “The magnetic field is normal to the boundaries through the choice of boundary conditions of the vector potential” → Consider rephrasing this sentence for clarity.
Yes, the sentence was shortened to: “The magnetic field is normal to the boundaries”.
22. I. 178 the bracket notation and the bar notation are not defined here, is it the same as at line 190 i.e. bar is temporal average and brackets spatial average? In part. 2.2 at line 200 is it the opposite i.e. the overbar denotes the azimuthal average. I suggest standardizing these notations.
Yes, thank you for noticing this. Now it is corrected, and we specify brackets for the temporal average and for the volume average.
23. I.192 “Direct solutions of the MHD equations capture the rotational influence on the flow, represented here by the Coriolis numbers, realistically, whereas other parameters such as Prandtl and Reynolds numbers are either several orders of magnitude too large or too small.” Consider rephrasing this sentence or making it more concise for clarity.
Yes, for clarity, the sentence was rephrased.
24. I. 205 It is written that α depends on r , what is its dependence ?
This line about α was moved below and combined with the description of ellipsoid- z profile. PJK: I further shortened the description in the text COR: Ok
25. Eq. (8) What is the reference for this relation?
This is the uncurled mean field induction equation. The reference for Brandenburg & Subramanian 2005 was added.
26. I. 245 Some more details about the ellipsoid- z profile in Pencil code would be appropriate here.
Yes, now the ellipsoid- z profile is described in more detail. PJK: I further shortened the description in the text. COR: Ok
27. Fig. 1 The diverging blue-to-red colourmap is not appropriate here are only positive values of Ω are featured, I suggest switching to another one.
This is a good observation. I have changed the colourmap to ‘viridis’, which is a sequential colourmap.

28. Legend Fig. 2 “zoom on the growthrate” → “zoom on B_{rms}”
Yes, for clarity, now it is changed as suggested.
29. Fig. 4 As you also mention the runs B2 and B10 in the main text, consider including their butterfly diagrams in the figure.
Thank you for pointing that out. We have now included the butterfly diagrams for B2 and B10.
30. I. 320 In this part, the notations are not easy to follow. If I understand correctly, C_{alpha}^{epsilon_f} is computed according to Eq. (13), while C_{alpha} is computed from Eq. (12) in Tab.2? Or is C_{alpha}^{epsilon} computed from Eq. (11) as explained in the main text I. 325? I recommend changing the notations to make them more explicit, for example C_{alpha}⁽¹²⁾, C_{alpha}⁽¹³⁾ referring to the equations (or any other possibility). Physically, what is the difference between the two ways of computing C_{alpha}?
Thank you for your question. The references for the equations in Table 2 were (PJK: I assume this is now fixed, COR: yes, they are) indeed mixed. C_{alpha}^{epsilon_f} is computed according equation 11 (13 in the revised version), while C_{alpha} is computed according equation 13 (15 in the revised version). This is corrected in the caption of Table 2. Therefore, as it is clear now, the notation remains the same.
31. I. 355 “[...] where Delta Omega is the estimated shear from the radial contrast of the equatorial angular velocity profile” should be rephrased for clarity, or Delta Omega be defined with an appropriate equation.
Yes, the phrase was rephrased, and Delta Omega is now defined in the text.
32. Fig. 8 would benefit from having the labels for the D number, as in the next Fig.
Yes, now the labels for the D number have been added.
33. I. 379 The Alpha Omega approximation should be defined.
There is now a more detailed definition of the alpha Omega approximation.
34. While understandable, I suggest replacing the notation Omega_{B1 x2} by 2 Omega_{B1}, as the overall rotation profile is multiplied by 2 (same remark for all the other models and their notations)
This is a good suggestion to improve readability. The change has been made.
35. I. 411 “Sect. C” should read “Appendix C”.
Yes, now it is changed.
36. Fig. 9 As you comment C_{alpha} in the text, could you include their values also in the legend (I. 390)?
Yes, the legend is now indicating C_{alpha}. Before it was indicating \alpha, which is in code units and we do not mention this value in the text. Therefore, C_{alpha} is the appropriate one.
Thank you for noticing this.
37. Fig. 8 The fits for the top figure should be commented on in the main text.
The fits are now commented in the main text.

38. Fig. 12. The choice of colors makes it difficult to read.
We have changed the colours and the linestyle for clarity.
39. It would be valuable to display the values of Omega and alpha on each plot. The labels should also be bigger.
The rotation profile and C_α are shown for the different figures. In Fig. 9, the rotation profile is indicated in the caption, and C_α is indicated in the legend. In Fig. 10, they are also indicated in the caption and legend. In Fig. 12, the rotation profile is indicated in the legend and stated more clearly in the caption. In that plot, $C_\alpha=0$, and it is also indicated in the caption.
40. l. 539 “simply” is perhaps an overstatement here.
Yes, simply was removed, and the paragraph was extended for further discussion.
41. Appendix A: In the Omega columns, it should be Ω_{B1} , $2 \Omega_{B1}$ etc ...
Yes, the Omega column notation was changed accordingly.
42. Appendix A: I suggest including in the table B_{rms} for each simulation.
A new column with B_{rms}/B_{eq} was added in Appendix A.

List of changes:

- Paragraph 1 (Introduction) :
 - Added the Noyes 1984 citation before Lehtinen 2020.
 - Added Mathur et al., 2014 and Mathur et al., 2025. Mention the relation they found between the Sph and Rossby number for F stars.
 - As suggested, and for completeness, we discuss additional mechanisms that could distribute angular momentum, in addition to the Lambda effect.
 - Added CO5BOLD reference.
- Paragraph 2 (Section 2):
 - Added a new subsubsection for Stellar parameters and a short sentence about the choice of geometry in the mean-field model.
 - at the end of 2.1, new lines mentioning the advantages of using the star-in-a-box setup.
 - Expanded the last sentence of 2.1.3, giving more details of what Table 1 is listing.
 - In 2.1.3, “Direct solutions of the MHD equations capture the rotational influence on the flow, represented here by the Coriolis numbers, realistically, whereas other parameters such as Prandtl and Reynolds numbers are either several orders of magnitude too large or too small.” was rephrased to “Direct solutions of the MHD equations capture the rotational influence on the flow through the Coriolis numbers, which is the only parameter that can be captured in 3D simulations. Other parameters such as Prandtl and Reynolds numbers are either several orders of magnitude too large or too small.

- Paragraph 3 (section 3)
 - In 3.1, we added a line mentioning the effect of rotation on the saturation level.
 - As suggested by the referee for Fig. 4, we added the butterfly diagrams for runs B2 and B10.
 - As suggested by the referee for the caption of Fig. 2, we changed from: zoom of the growth rate -> zoom of B_{rms} .
 - I clarified in 3.5 that the field of runs A_0 and C_0 shows a dipolar polarity, with opposite polarities between the northern and southern hemispheres, while run E_0 shows a quadrupolar topology.
 - In Figure 8, the labels for the D number have been added.
 - In Figure 9, we changed the legends from alpha to C_{alpha} , as suggested by the referee.
 - At the end of subsection 3.4, the implications have been reformulated to refer specifically to the magnetic field of the envelope.
- Conclusions (Section 4):
 - A discussion regarding observed magnetic field topologies is added to the 1st and 2nd paragraphs.
 - At the end of the 2nd paragraph, we mention that the conclusions are in particular for the envelope magnetic field.