Author response to referee comments on ‘Early warnings of the transition to a superrotating atmospheric state’.

Dear Editor,

We would like to thank the four reviewers for giving up their time to read our manuscript and for suggesting improvements.

There are some general themes for revision that emerge that we will take up in a revised manuscript. These are:

- The section on the theory of the EWS is overdetailed: The manuscript is more pedagogical than a typical journal article because it is a submission to a special issue on tipping points in ESD and from the author’s experience, this group (and likely readership of this manuscript) are largely not familiar with the techniques used and we wish to make them aware. However, we will look to shorten and improve these parts of the manuscript.
- The presentation of figures 11 and 12 could be improved: These are the central results and are therefore key. We will try and improve them.
- The discussion of similarities to phase transitions in section 7 is over detailed: We will look to significantly shorten and improve this discussion.
- Section on evidence for bistability: We will rename this section to ‘lack of evidence for bistability’ or similar and get to the point no bistability is found before presenting the evidence.
- Discussion of physical mechanisms for superrotation and the transition: This was lacking in the original manuscript by design, however we will include some discussion including relevant references. We plan on investigating the physical mechanisms in detail in a future manuscript.

Detailed responses to referee comments are given point-by-point below in bold face print.

Yours sincerely,

Mark Williamson and Tim Lenton.

RC1

1. This paper analyses the transition to superrotation in an idealized GCM as a function of the thermal Rossby Number that is controlled by varying the radius of a planet. The focus, in particular, is on the early warning signs (EWS), including amplitude and autocorrelation of the noise before the transition. The authors present an expansion of the concept of EWS to multi-variate state space. I find the subject interesting, the analysis well done, and the paper well written. I make some minor suggestions below, and I recommend accepting the paper subject to minor revisions.

   Thank you for the positive evaluation, it is much appreciated.

   Specifics:

   1.1. It would be helpful to mention in the introduction that superrotation does not occur at the surface in this study, which is consistent with related recent studies. I believe there is a recent paper by Caballero and collaborators where they try to see what might lead to surface superrotation. Such superrotation, which does not reach the surface, may not have dramatic socioeconomic consequences, and it might be good to mention that as well in the introduction and conclusions.

   This is a good point and one we will incorporate into a revised manuscript.

   1.2. It seems that the analysis of the multi-variable EWS is based on building a reduced-space, EOF-based linear inverse model and then analyzing it following the principle oscillation pattern (POP) approach. The linear inverse model is not explicitly mentioned; I am deducing this from what is written. In any case, the specific methodology should be mentioned using wording that connects the methodology to the existing literature in the abstract, introduction, and conclusions. It would be helpful to the readers if the authors cited previous papers that use POP and linear inverse models in different contexts.

   The method for reducing the dimensionality for calculation of the Jacobian J in Section 6.2 is described between lines 426-434 i.e. the dynamics around the mean state are approximated by projecting the full dynamics onto the n largest EOFs where $n = 60$. These reduced fields of u and v are then used to calculate J. We were not aware of the linear inverse model (LIM) approach but having googled it, it is indeed identical to the approach followed here. It seems POPs, LIMs and empirical normal modes (ENMs, another technique that popped out of the LIM google) refer to the same thing. We will note this in the revised manuscript and add references. We thank the referee for pointing this out.

   1.3. The discussion of the spatial modes at the end of section 6 is a bit of a let-down after the buildup of this multivariate analysis as a main new result here. Is there anything else that can be said?

We think that tracing the importance of the precursor mode (the wavenumber zero mode with the 25 day period) through the dynamics is a nice result in our humble opinion. We can trace its appearance to just before the transition, its dominance in the dynamics at the transition and its quick decay in importance after the transition. We particularly like this result as it seems it is this mode that triggers all the usual EWS. We can therefore attribute a spatial signature as an early warning (as well as the usual, widely used temporal EWS) to the transition. This is something that we have not seen reported before for a climate tipping point (which is not to say it has not been done). We will look to strengthen these points in a revised manuscript particularly in section 6.

1.4. The paper explains all concepts used very carefully, perhaps even at a somewhat too elementary level at times. Mostly, this is fine, although I would suggest removing lines 373-380 and 390-401, which are just too basic.

The manuscript is more pedagogical than a typical journal article because it is a submission to a special issue on tipping points and from the author's experience, this group (and likely readership of this manuscript) are largely not familiar with the techniques used and we wish to make them aware for future research. We agree they are basic concepts to readers from dynamics or similar backgrounds. We are happy to remove 390-401 but would prefer to keep lines 373-380 for readers that may not be familiar with these techniques.

1.5. Remove the paragraph on lines 560-567, which seems irrelevant to this paper.

The purpose of this paragraph was to continue comparing and contrasting the transition with traditional phase transition studies. However we are happy to remove this paragraph as it is speculative and more detailed than need be.

1.6. The definition of tipping points in the second sentence of the paper is vague. I realize the authors have been using this definition in the past. On the positive side, I note that the paper carefully discusses the different types of bifurcations (noise-driven, rate-driven, equilibrium) at a later point. Despite that, this vague definition seems difficult to digest for this particular reviewer. Perhaps the authors can simply say, 'We define tipping points as...'.

Our impression is that the tipping point community as a whole has a vague definition of tipping points, particularly when viewed through the lens of disciplines that are careful with such things. Would the referee be happy

with the following text in place of sentence 2 of the introduction?

The IPCC has defined a tipping point as a 'critical threshold beyond which a system reorganizes, often abruptly and/or irreversibly' (IPCC, 2021) and 'for the climate system, the term refers to a critical threshold at which global or regional climate changes from one stable state to another stable state' (IPCC, 2019).

1.7. I forget if the papers mentioned on line 72 (Huang et al., 2001; Caballero and Huber, 2010; Mitchell and Vallis, 2010) examined a gradually increased CO2 to look for a transition or just simulated at high CO2. If the latter, they didn't look at the actual transition, so a slight rewording of the sentence may be needed.

Huang et al., (2001) report a transient coupled simulation of the period 1900 - 2100 with CO2 increasing according to the IS92a scenario (an IPCC 1992, business as usual scenario). Superrotation in the tropical upper troposphere appears smoothly under strong warming in the later part of the simulation. The work of Mitchell and Vallis (2010), is essentially repeated in this manuscript i.e. the control parameter (the thermal Rossby number) was fixed in each run but changed incrementally between runs. Caballero and Huber (2010), report a similar protocol to Mitchell and Vallis i.e. fixed control parameter (in this case atmospheric CO2 concentration) but changed incrementally between runs (CO2 concentration is doubled). In all simulations the change in mean u appears smooth. We therefore prefer to keep this sentence as written.

1.8. Line 75: is → does

Good catch, thank you.

1.9. I suggest eliminating Figure 6 as it is repeated in Figure 7.

We will remove figure 6 in a revised manuscript.

1.10. Around line 300: I agree that the differences between the results for the increasing and decreasing parameter value are not significant and may be due to the short time series and are not a sign of bi-stability. The authors do say so eventually. Perhaps they can make it clear as soon as this result is presented.

This is a similar comment to other referees. We will change the title of this section to make it clearer that we do not find bistability and mention this fact in the first paragraph of this section.

1.11. Figure 11 needs some work: why are dotted circles drawn around the spheres? Remove them (colored titles are sufficient) to allow increasing the sphere size instead; eliminate white space, make middle panels larger; find a different way to present the black dots in the middle panels that are currently just too messy; reformat titles to be over two lines to allow increasing the size of graphics elements and eliminate white spaces; add (a), (b), etc. to the different panels and refer to them in the caption.

Please see the reply to the next item.

1.12. Figure 12: make graphics/spheres much larger by reducing white spaces.

Figures 11 and 12 have a lot of information to convey and we tried to get this across as best we could given the limitations of a non-animated presentation format. Figure 12 in particular would ideally be presented as an animation, which we have included as a video supplement. That being said, we agree this could be presented better and we will give a lot of thought of how to do this best in a revised manuscript, particularly as these figures are the central results. Another referee suggests cyclindrical projections of the globe, rather than the spherical plotting.

Thank you for the suggestions, we will incorporate them as best we can in the revised figures.

RC2

2. This paper examines the transition to superrotation in an atmospheric dynamical core with Held-Suarez forcing. The transition to superrotation is induced by changing the planetary radius, following previous work. The novelty here is in the application of tipping-point 'early warning signal' diagnostics to the transition. While the general idea of the paper is of some interest, it seems to me that the specific way it is set up and developed in this paper misses the mark. To be more specific, I can think of two reasons to be interested in EWS of superrotation: (i) in the real-world climate change context, it is certainly of broad interest to examine methods for early detection of such a transition in response to increasing CO2; (ii) in the context of atmospheric dynamics, the EWS approach may give new physical insight into the mechanisms of the transition, which would be of interest to a broad audience in GFD and planetary atmospheres.

   In its present form, this paper unfortunately does neither of those things: by the authors' own admission, the modelling setup is not relevant to real-world climate change, and the analysis is purely empirical and does not give much (or any) insight into underlying physics. The paper also feels haphazardly written, with long expository passages covering basic text-book material, and some paragraphs at the end that veer into theoretical physics language whose relevance is not very clear. There are also some logical non-sequiturs, many typos and some figures which are hardly legible and not very informative. As a result, I can't recommend this paper for publication in its present form. My suggestion is to extend the analysis in the direction of (ii) above, to include some more material that at least yield some hints as to the physics. I offer some comments and suggestions below that may hopefully help the authors develop and improve the paper.

   We thank RC2 for taking the time to read our manuscript and offer suggestions, it is appreciated. To give RC2 some background, this paper is written for a special issue on tipping points in ESD and is therefore framed in the language of tipping points and their precursors. From the author's experience, the likely readership of this manuscript are largely not familiar with atmospheric dynamics, superrotation and the vector techniques used. The purpose of the manuscript was therefore to introduce superrotation as a potential tipping point, introduce the vector techniques for diagnosing spatial EWS and see if there were EWS of superrotation in a simple, idealised scenario studied before by Mitchell & Vallis (2010). These, we believe, are all novel and worthwhile contributions to the tipping points literature. With this target readership in mind, it was written in a more pedagogical style than a typical journal article and we also made the decision to leave out a detailed diagnosis of the atmospheric dynamics, some of which has been discussed before by Mitchell & Vallis (2010), as this was judged

to be overdetailed for the target audience. We will add text to our introduction in order to make this framing clearer.

Our main finding, that we can attribute a spatial signature as an early warning (as well as the usual, widely used temporal EWS) to the transition, is something that we have not seen reported before for a climate tipping point (as far as we are aware). As RC2 suggests in points (i) and (ii) there is still a great of deal of work to do here. In the future we plan on diagnosing the atmospheric dynamical mechanisms at the transition in a submission to a more suitable GFD or planetary facing journal. Thank you to RC2 for the many useful suggestions in this direction, we will look at them in detail. Having said that, in this manuscript we will include a discussion on physical mechanisms including the references suggested and any insights from initial investigations during the revision. We also plan on performing simulations of the transition that are more relevant to climate change to see if the EWS found here are still present.

2.1. The exclusive focus of the paper on a single level, $\sigma = 0.74$, seems unmotivated and gives a sense of cherry-picking. What is special about this level? When this level transitions to superrotation, at $Ro_T \sim 1$, the entire troposphere above it is already superrotating, as is clear from Fig 2. Wouldn't it make more sense to focus on the *first* transition to superrotation, which happens in the upper troposphere presumably at lower Ro_T?

Our criteria was to choose the lowest vertical level that superrotated, the idea being that the lowest atmospheric level would have the largest impacts on people through changing weather patterns. This level is about 2-3 km above sea-level. But the referee makes a good point, higher up in the troposphere superrotation does occur earlier and this would also impact weather patterns. We will make this clearer in a revised manuscript.

2.2. Does the oscillatory behaviour found here in the lead-up to superrotation also happen at those levels? I would suggest repeating the analysis of Sec. 4 at various levels, at the Ro_T values relevant for transition to superrotation at those levels.

From recollection we believe it does but we will investigate further in a revised manuscript.

2.3. The general idea of EWS is that there is an underlying loss of stability of the system. The paper never looks at the equations of motion and their stability, so this link is never explicitly made. There is in fact much theoretical work not mentioned here that in fact supports this superrotation-as-an-instability idea in precisely the context studied here, for example Wang, P., and J. L. Mitchell, 2014: Planetary ageostrophic instability leads to superrotation. Geophys. Res. Lett., 41, 41184126, Zurita-Gotor, P., and I. M. Held, 2018: The finite amplitude evolution of mixed Kelvin-Rossby wave instability and equatorial superrotation in a shallow water model and an idealized GCM. J. Atmos. Sci., doi:10.1175/JASD170386.1, Zurita-Gotor, P., . Anaya-Benlliure, and I. M. Held, 2022: The sensitivity of superrotation to the latitude of baroclinic forcing in a terrestrial dry dynamical core. J. Atmos. Sci., 79, 13111323. The basic idea is that superrotation arises from wave-mean flow interaction, specifically the interaction of equatorial Kelvin and Rossby waves that mutually amplify each other (the Kelvin-Rossby instability) generating a planetary-scale mode that converges zonal momentum onto the equator and drives superrotation. This happens first in the upper troposphere near the tropopause, where zonal jets are strongest and yield high phase-speed Rossby waves which can lock in phase with Kelvin waves. This previous work should at least be cited and discussed here. More interestingly, the POP analysis applied here should be able to pick up those Kelvin-Rossby modes, providing an interesting path to confirming their relevance for the transition beyond what is already done in the papers of Mitchell and Zurita-Gotor.

Good points and thank you for the references. We will read them and look at incorporating these references and their insights in a revised manuscript and future work.

2.4. Different levels in the troposphere are not independent, they are coupled through momentum transports by the mean overturning circulation (the Hadley cell), among other mechanisms. It would be interesting to have some idea of how the oscillatory behaviour found at a single level relates to vertical exchange with other levels, and more generally with oscillations in Hadley cell intensity. For example, the authors could examine regression maps of zonal-mean u at one level with that at other levels, and with the intensity of the Hadley cell.

Good suggestion and one we will investigate further.

2.5. Line 21: It's not clear to me how the quasi-resonance phenomenon counts as a tipping point, there is no suggestion of a bifurcation in the underlying physical picture described by the proponents of that theory.

We mentioned this phenomenon because some of the proponents of the theory have placed it on their putative maps of possible tipping points, but we agree with the referee that there is no valid suggestion of an underlying bifurcation. Hence we can remove discussion of the phenomenon in the revised paper or add these caveats (a tipping point need not be due to a bifurcation as described in the reply to comment 2.7.).

2.6. Line 23: What specifically are the 'huge impacts' that superrotation would have if it occurred?

We talk about the impacts in lines 66 - 72.

2.7. Lines 28-31: The term 'tipping point' usually refers to a sudden transition caused by a smoothly-changing parameter crossing a bifurcation threshold. By contrast, (a) and (b) here refer to noise-induced transitions between metastable states at a *fixed* parameter setting. So please either give a clear definition of what you mean by 'tipping point', or remove these two examples, which in any case feel superfluous to the rest of the paper.

The standard discussion of tipping points in this community lists these three ways for a tipping point to occur, namley noise induced (N tipping), bifurcation induced (B tipping) or rate induced (R tipping) in the language of Ashwin et al. (2012), the original reference. In this manuscript, tipping from crossing a local bifurcation is the main interest as there is potential for EWS in this scenario. Other types of tipping are also thought of as tipping points in this community however. RC1 (see comment 1.6. and reply) also suggests tightening the definition of a tipping point. We will revise this.

2.8. Line 67: The Caballero and Carlson (2018) paper does not argue that transition to superrotation is unlikely; it argues that transition to superrotation *at the Earth's surface* is unlikely. Superrotation in the upper troposphere happens readily in climate states possible under high-end future climate warming scenarios.

Good catch, we will revise the wording in the revised manuscript.

2.9. Line 146: If zonal-mean u is negative, then Rayleigh damping as specified in Eq 6 will *accelerate* the zonal-mean wind and act as a *positive* angular momentum flux convergence, so this argument does not make sense to me. The reasons why it is difficult to obtain superrotation at or near the surface are examined in detail in Caballero and Carlson (2018). Please rephrase this section to strengthen your argument. Also, as noted above, focusing exclusively on this level feels like cherry-picking because it is influenced by Rayleigh damping, which could affect the dynamics; hence the value of repeating the analysis at higher levels which are not influenced by Rayleigh damping.

Good spot, we meant to say for any positive u, Rayleigh drag acts as negative momentum flux. Although for any u (positive or negative) equation (6) implies $|u|$ should decrease exponentially with time provided $k_v(\sigma)$ is positive. However, the equation is still not quite correct. We have revised the text starting at line 115 to read:

Friction from the planet's surface on the atmosphere acts as to relax horizontal velocities back towards zero in the atmospheric boundary layer and this is specified as an extra, linear and vertical height dependent (Rayleigh) drag term in the equation for the horizontal velocity. This extra drag term is applied as

$$\frac{\partial \mathbf{v}}{\partial t} = \cdots - k_v(\sigma)\mathbf{v}, \tag{1}$$

$$k_v(\sigma) = k_f \max\left(0, \frac{\sigma - \sigma_b}{1 - \sigma_b}\right) \tag{2}$$

where $\mathbf{v}$ *is a vector of the horizontal velocities* $:= (u, v)$. *(For the full horizontal momentum equations see Mitchell and Vallis (2010)).*

And we have also revised the text around line 148 to read:

None of the simulations show superrotation $(u > 0)$ *at the equator in the lowest two vertical levels* $(\sigma > 0.8)$. *This is due to Rayleigh drag within the boundary layer* $(\sigma \geq 0.7)$ *decelerating the horizontal flow and acting as a negative angular momentum flux for any* $u > 0$.

Regarding the choice of level see the reply to comment 2.1. however we will investigate other vertical levels when revising our manuscript.

2.10. Line 192: This describes an Ornstein-Uhlenbeck process. The authors should note that the noise in this process is internally generated by the chaotic high-frequency dynamics, and approximating it as white noise is a strong assumption.

We will note this in the revised manuscript. This is a commonly made assumption in climate science and it is frequently not a bad assumption, even when external forcing is time varying (not the case in this study). Methods such as empirical orthogonal functions, linear inverse models, empirical normal modes, principal oscillation patterns, linear regressions (and many others etc, etc) approximate fast or short timescale chaotic dynamics as white noise.

2.11. Sec. 6.1: This long exposition of the POP decomposition is text-book material and I don't see its value here, it only dilutes the text. I recommend shortening to a single paragraph giving a concise, intuitive explanation of POPs, and a reference to a relevant text book such as von Storch, H., and F. W. Zwiers, 1999: Statistical Analysis in Climate Research. Cambridge University Press.

As mentioned in the reply to comment 2, the manuscript is more pedagogical than typical for a scientific article. We have written it this way due to the likely readership who (from our experience) are largely not familiar with the technique. We therefore prefer to keep much of the material. However, we will look to shorten and revise where we can in a revised manuscript.

2.12. Figs 11, 12: I don't see the value of using the orthographic projection in these figures, which doesn't show the full global structure of the modes (so it's difficult to see at at glance what the zonal wavenumber is, for example). Also, the panels are too small to see the arrows properly. I recommend re-plotting using a standard cylindrical projection for ease of comparison with previous work (see main point 2).

As we replied to RC1, figures 11 and 12 have a lot of information to convey and we tried to get this across as best we could given the limitations of a non-animated presentation format. Figure 12 in particular would ideally be presented as an animation, which we have included as a video supplement.

That being said, we agree this could be presented better and we will give a lot of thought of how to do this best in a revised manuscript, particularly as these figures are the central results. We will look at plotting cylindrical projections in a revised manuscript.

2.13. Lines 542-566: These three paragraphs are full of impressive-sounding language, but I don't see how they are directly relevant to the work discussed here. I recommend rewriting to make the relevance clearer, or (preferably) eliminating them.

The purpose of these paragraphs was to continue comparing and contrasting the transition with traditional phase transition studies. However we are happy to remove the paragraph starting line 560 and possibly one or more of the others as they are speculative and more detailed than need be. We will have a good look at improving this part of the manuscript in the revision.

2.14. Line 87: rotating $\rightarrow$ superrotating

Corrected, thank you.

2.15. Eq (8): Should be $\overline{T}$, not T_0?

Strictly Ro_T is given by equation (8) i.e. it does vary with latitude. However, we attribute a single value of Ro_T to each simulation by using the average temperature i.e. $T_0 \rightarrow \overline{T}$ in equation (8). The average over the surface of a sphere of equation (8) amounts to the transformation $T_0 \rightarrow \overline{T}$, so the same thing although Ro_T can vary over the globe. We will add a sentence in a revised manuscript around equation (8) describing this.

2.16. Line 210: minimally what?

The minimal dimension of a real valued J that can show a Hopf bifurcation is 2 by 2. We will rephrase this to:

For a real valued $J(x)$ to describe a Hopf bifurcation, it must be at least a 2 by 2 (non-symmetric) matrix with complex eigenvalues, λ and λ^*, that appear in conjugate pairs (the $*$ denotes complex conjugation).

2.17. Line 350: This sentence is garbled, please re-write.

We think it reads ok. If we replace with *'The new basis given by the set of $\delta\tilde{x}_i$ go by several names i.e. they are often known as linear, critical, normal or the eigenmodes modes.'* does this make it clearer?

RC3

3. The authors explore early warning signals of super-rotation in a GCM, finding that spatial mode decomposition can yield useful information for predicting a transition to the super-rotated state. The also confirm that standard signals, like lag-N autocorrelation and variance, herald the transition, and explore whether there is evidence for bistability. The paper is well-written and makes a good contribution to the literature on spatial early warning signals by pointing out the potential value of looking at dominant spatial modes. I only have a few minor comments that the authors may wish to take up.

   Thank you for the positive evaluation, it is much appreciated.

   3.1. Regarding bistability, an increase in the skew of a time series may indicate the presence of a nearby stable state (https://link.springer.com/article/10.1007/s12080-013-0186-4), and the fluctuations in Figure 8 seem to show a strong upward skew for Ro_T near 0.5. The authors could compute skew and see how it compares to lag-1 AC and variance.

   This is very useful suggestion and one we will look at. Thank you.

   3.2. Further to point #1, since a complex system may transition well before or well after the point where theory suggest to expect the tip, I dont see why ramping Ro_T up and then down across the tipping point is a good way to check for bistability. It seems like it would be easier to check through experiments that initialize the system in either a rotated or super-rotated state, for the same Ro_T, and then study whether it remains in those states. Ro_T would be varied across a range. But perhaps this is not computationally feasible with a GCM.

   This is again a useful suggestion and one we will look at and attempt to implement.

   3.3. Figure 6: what accounts for the initially high value of lag-1 AC, when Ro_T is close to zero?

   Good question. The full (multiple lag) autocorrelation function of this simulation is shown in figure 5 in blue (far RHS). It is not oscillatory or with high variance but it does not look like a typical exponentially decaying function either like the other simulations in the manuscript. It appears to be approximately linearly decaying with time so the lag 1 measure may not be the best way to infer the decay timescale (and critical slowing down) for this particular value of Ro_T. This value of $Ro_T = 0.02$ corresponds to a simulation that is 'Earth-like' so the mean equatorial zonal flow may be in a different regime to the other simulations as Ro_T is increased. This would be our guess.

   3.4. Figure 11, 12: A legend for colours on the sphere (windspeed) would be helpful.

Concerns about the readability and presentation of figures 11 and 12 have been raised by other referees as well. We will give a lot of thought of how to do this best in a revised manuscript, particularly as these figures are the central results.

RC4

4. I have read with interest you paper. The topic is important and I find it useful to look into the connection between regimes of superrotation and indicators of proximity to potential tipping behaviour. I have to say though that I am not convinced by various key aspects of the paper that I mention below. Most importantly, I am particularly concerned by the fact that the paper sometimes seems not to follow a clear logical thread.

   Thank you for acknowledging the importance and interest of the topic of the manuscript. We will address your concerns of logical inconsistencies in the following which we show are based on your misunderstanding.

   4.1. Introduction: I think it would be useful to mention explicitly the existence of multiple competing states for the tipping elements mentioned around line 15. It would also important to emphasize that metastability occurs also in very dynamical circumstances very different from the usual scenario, see Feudel 2023[1].

   We discuss the different ways a tipping point may occur at the start of the next paragraph (line 28 onwards). The reference you suggest seems to fall under rate induced tipping (which we describe on line 31). We will read it carefully and likely cite this reference here. RC1 wanted a more precise definition of tipping point around line 15 (see reply to comment 1.6. and 2.7.). We will address this in a revised manuscript.

   4.2. Additionally, when discussing early warning signals, I think that citing the recent paper by Boettner and Boers 2022 Phys Rev Res would be beneficial

   We have cited many of the key references (paragraph 2 of the introduction) with regard to early warnings. The reference suggested by RC4 does not seem appropriate here (false warnings from coloured noise).

   4.3. I do not find Figure 1 very informative, it is similar to standard images one can find in many books of mechanics or geophysical fluid dynamics.

   We prefer to keep this figure as it serves as a description of the notation in the introductory equations. Having said that we are happy to remove it if the referee insists.

   4.4. Around line 75, the authors introduce the concept of smooth transition to the super rotating regime, differentiating from the abrupt case. Abrupt transitions seem those associated with the kind of tipping behaviour investigated below. Since the authors find a smooth transition, this create a sort of logical non-sequitur in the later part of the manuscript.

[1]https://npg.copernicus.org/articles/30/481/2023/npg-30-481-2023.html

This we believe is the root of the RC4s claim of logical inconsistencies. We will attempt to demonstrate to the referee that is not the case here: The characteristic of any local bifurcation is the loss of stability of the state. Approaching a local bifurcation results in the state becoming less stable and this decrease in stability shows up as increased recovery time of any small perturbation away from this state, often known as critical slowing down. Early warning indicators are essentially functions of the stability of that state and therefore change as the stability changes. At the bifurcation, the state looses all stability and the system has to find a new stable state. If this new stable state joins smoothly (or non-smoothly but the jump is small) then there will be a smooth (or approximately smooth) transition. If the new stable state is far away (or far enough away that the jump is observed as large) then the transition is abrupt. Passing through this local bifurcation could therefore result in either a smooth or abrupt transition to a new stable state, both result in a loss of stability and therefore both correspond to a change in early warning indicators and are equally well detected by an increase in critical slowing down. Examples of smooth transitions include transcritical and pitchfork bifurcations, see chapter 3 of Strogatz (2001) (or similar introductory non-linear dynamics text). We will include some of this discussion and examples of smooth, bifurcation induced transitions around line 81 to avoid potential confusion for other readers.

4.5. I do not understand Equation 6 - where are the advective terms?

Good spot, we have now revised this passage (see comment 2.9.).

4.6. Line 130: the author explain that the transition to superstation is achieved for $Ro_T \approx 1$. This means, keeping the rest unaltered, considering a value of meridional temperature gradient about 40 times larger than the terrestrial one. This seems to make extremely little physical sense (one would have to consider temperature that cannot be realised by the geophysical fluids). So I am lost at to relevance for the terrestrial circulation. I understand that the authors consider different radii to perform simulations (even if I wonder whether the model behaves consistently well for such altered conditions).

As discussed multiple times in the manuscript (see para starting line 501, para starting line 567), these simulations are idealised and not (we think) directly applicable to how the Earth may transition to superrotation under future climate change scenarios. However, this is not explicitly mentioned in the abstract and we will update this in a revised manuscript. The simulations reported in the manuscript were meant to serve as a test bed for whether we can detect this transition and what sort of precursors that might be expected.

We plan to perform simulations that are more realistic in the future building on the work here.

4.7. Eq (8): is it T_0 or $\bar{T}$? T_0 seems to depend on latitude (Eq 7)

Strictly Ro_T is given by equation (8) i.e. it does vary with latitude. However, we attribute a single value of Ro_T to each simulation by using the average temperature i.e. $T_0 \to \overline{T}$ in equation (8). The average over the surface of a sphere of equation (8) amounts to the transformation $T_0 \to \overline{T}$, so the same thing although Ro_T can vary over the globe. We will add a sentence in a revised manuscript around equation (8) describing this.

4.8. I wonder whether Fig. 2 would be more informative if including information on the angular momentum instead dof the zonal velocity - at the end the authors discuss only the properties of the flow at the equator.

We have chosen u as this is the way zonal profiles have been presented in other studies. We choose this representation for ease of comparison with other studies and for ease of interpretation - we have more intuition for velocity than angular momentum.

4.9. The authors describe (lines 165 to 222) the derivation of early warning signals (EWSs) for system characterised by a fixed point. Yet their dynamics is turbulent. In order to construct the EWS, they use as reference the long term average of u (Eq (19)). Assuming that the fixed point and the long-time average are the same thing is a major and critical simplification.

We derive EWS for a stationary point, rather than a fixed point i.e. statistical properties of a stationary point are independent of time. This is a commonly made assumption in climate science and it is frequently not a bad assumption, even when external forcing is time varying (not the case in this study). Methods such as empirical orthogonal functions, linear inverse models, empirical normal modes, principal oscillation patterns, linear regressions (and many others etc, etc) approximate fast or short timescale chaotic dynamics as white noise.

4.10. Additionally, the authors make reference to lag-1 autocorrelation . It is not clear what "1" refers to, in which time units. Additionally, other indicators are much more robust at the lag-1 autocorrelation, like the integrated autocorrelation time.

Lag 1 refers to a lag of 1 day. This is mentioned on lines 254 and 258.

4.11. This part - as well as the related Section 6.1 below - reports - with fairly good degree of clarity - material that has been dealt with in greater generality in Tantet et al, (2018) Nonlinearity, Chekroun et al. (2020) J Stat Phys and Santos Gutierrez and Lucarini

(2022) J Phys A, with no restriction on the fact that the reference state is a fixed point. These references should be cited for completeness.

We will read, evaluate and cite these references (or similar) in a revised manuscript.

4.12. Similarly, these papers also discuss the modes (Sect 6, spatial precursors) associated with criticality as being critical modes of the Koopman operator of the system. In the case of fixed point reference solutions, one finds the special case discussed in this paper.

We are sure the techniques described here can be generalised in many directions. Indeed it appears the technique presented here has been discovered multiple times under different names already (see reply to comment 1.2.). Here we prefer to present as simply as possible as in our experience the majority of the tipping points community which the manuscript is written for, is unfamiliar with the generalisation to multiple variables. Reconstructing Koopman operators is generally not straightforward.

4.13. In any case, In terms of presented results and interpretation, I am not sure why one should expect that the EWSs discussed in this section - which refer to the case of abrupt transitions - should work in the case of smooth transitions. I might be wrong , but I do not find a clear logical thread here.

You are wrong. You will see critical slowing down when approaching any local bifurcation, whether the transition is smooth or abrupt. See our reply to comment 4.4.

4.14. I am a bit of at loss here. The content of the section indicates that there is no evidence of bistability. The title is in my opinion misleading.

This is a similar comment to other referees. We will change the title of this section to make it clearer that we do not find bistability and mention this fact in the first paragraph of this section.

4.15. In section 6 - mentioned above - the 'critical' modes are discussed, and in my opinion the claim that their represent criticalities is not well founded. The relaxation times are of the order of 20 days both for the reference case $Ro_T = 0.02$ and the case $Ro_T = 0.87$. So in my opinion the authors are finding 'simply' the slowest decaying modes, which, indeed, contain important information on the dominating feedbacks of the system. The reason why the slowest decaying mode is responsible for the variability of the system is presented in Chekroun et al. 2020 mentioned above.

We are indeed 'simply' finding the slowest decay modes. This is exactly what the technique is designed to do. It is well known that the slowest decay modes start to dominate and simplify the dynamics close to a bifurcation (or

phase transition). This is exactly the property we want to detect and that is what we see.

We used the term 'critical' mode in the manuscript probably too loosely and we might have borrowed a term from the phase transition literature to describe the mode that leads to criticality i.e. the bifurcation/transition to superrotation. The more standard term would probably be 'normal' mode. We will replace 'critical' with 'normal' in a revised manuscript.

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