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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Dynamics of the Hadley circulation in an axisymmetric model undergoing periodic change in forcing stratification" *by* Nazario Tartaglione

Anonymous Referee #2

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This short paper describes a numerical model study of an axisymmetric atmospheric circulation (somewhat akin to the Earth's tropical Hadley circulation) subject to a periodic forcing that cyclically modifies the static stability. The configuration is not particularly realistic as a representation of the true circulation in the Earth's tropics, but its motivation is justified as an idealized test of an hypothesis that at least some aspects of the observed response of the annular modes of the atmosphere to seasonal and intraseasonal variations of static stability may be determined by the axisymmetric circulation itself, independently of non-axisymmetric waves and eddies.

As such, this is not unreasonable to explore, at least as an academic exercise, and the author sets out a plausible case with reference to a body of recent publications.

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Given this motivation, the author's methodological approach, using a reasonably well established axisymmetric numerical model in spherical geometry, seems sound in principle, though does beg a number of questions (some of which are discussed below). A number of numerical experiments are conducted using this model, with a forcing that pushes the static stability periodically towards (precisely!) neutral stability with a period that is varied over a fairly wide range. The results indicate several different regimes, a (so-called) quasi-periodic regime (see my later comments) with periods from around 20-60 days, an intermittently chaotic regime for periods > 60 days and a further regime at forcing periods of <20 days in which the response is strongest at half the forcing period. A simple bifurcation diagram is constructed which illustrates the onset of the intermittent/chaotic regime.

These would seem to be novel and interesting results, but I do have a number of concerns and questions which I enumerate below.

(i) Perhaps a little pedantic, but I would disagree with the classification of the response at moderate forcing periods as "quasi-periodic". The latter term is normally understood to represent a system exhibiting 2 or more incommensurate frequencies - otherwise the attractor is a "simply periodic" limit cycle (or perhaps it really is a torus?). In the cases shown in Fig. 1, the responses show spectral peaks predominantly at the forcing period and integer fractions thereof. These additional peaks in the spectrum look very much as though they are simple harmonics of the forcing frequency omega - i.e. at 2*omega, 3*omega etc. Such components are not incommensurate with the forcing and are therefore not independent. This can be easily tested by constructing phase portraits e.g. using standard delay embedding of the time series shown in Fig. 1 (i.e. plot $\psi(t)$ vs $\psi(t+q)$, where q is a time interval around 1/4 of the forcing period). If the additional frequencies are indeed harmonics, the phase portrait will resemble a simple closed loop, whereas for a genuinely quasi-periodic evolution a topologically more complex object (such as a torus) will result. Examples could perhaps be presented to the reader in an additional figure?

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(ii) For periods longer than 60 days, an apparent dynamic bifurcation to an intermittently chaotic behavior appears. The chaotic bursting behavior seems to appear around the phase in the forcing where static stability is becoming very weak? Are you sure the chaotic behavior that is seen is due to a physical instability and not a numerical artifact? I would have like to have seen some kind of evaluation of this (e.g. by varying the spatial resolution and/or an exploration of what kind of motion is occurring when the chaotic bursts appear). Is this due to some kind of symmetric baroclinic instability or some other process? This should be explored more carefully to make sure we are not simply seeing the result of a defect in the model numerics.

(iii) On a related point, the model configuration chosen looks to have a number of singular symmetries, both geometrical (symmetric about the equator) and dynamical (static stability being forced precisely towards neutrality). Is the response and bifurcation sequence dependent on satisfying these symmetries or is the observed behavior generic? This would be important to check, since non-generic responses are unlikely to be observed in a real atmosphere.

(iv) On a similar theme, are these bifurcations to intermittent chaos likely to be observed in a fully 3D atmosphere? This ought at least to be considered and the means to test this discussed in the closing sections of the paper. Are there plans to extend the study to a fully 3D model with similar forcing? This would be the logical next step, but I would be frankly somewhat skeptical that some of these phenomena are robust to interactions with non-axisymmetric flow components.

Overall, therefore, this manuscript raises some interesting issues that might suggest new insights into certain kinds of annular mode variability in the Earth's tropics on intraseasonal timescales. But there are a number of points that need to be clarified before this study can be considered complete. Apart from the reservations outlined above, the work is reasonably clearly and concisely presented, though would benefit from some help from a native English speaker to tidy things up in places. Some examples are given below:

P.2 line 29 “has” instead of “have”

P.3 line 10 Strictly speaking M is the angular momentum per unit mass per unit radius of the planet and has dimensions of velocity.

P.4 line 24 “smaller than”

P.4 line 26 “associated with”

P.4 line 30 “by radiative processes only[?]”

P.5 line 4 “contribution. . ..such a way that. . .”

P.5 line 5 “alternately” not “alternatively”

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