



EGUsphere, author comment AC2
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Reply on RC1

Costanza Rodda et al.

Author comment on "Jet stream variability in a polar warming scenario – a laboratory perspective" by Costanza Rodda et al., EGU sphere,
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We thank the referee for the careful and insightful review of the manuscript and the efforts towards improving our manuscript. In the following, we comment point by point on the referee's suggestions and highlight our efforts to address the concerns.

Comment:

Line 50. How does an atmospheric Rossby wave (ARW) differ from a thermal Rossby wave? Further, how are mid-gap baroclinic instabilities analogous to ARW's when this experimental system contains no beta-effect?

Reply:

The thermal Rossby wave is discussed for experiments with very fast rotation. In that case, centrifugal force becomes strong compared to gravity and a strong radial buoyancy develops. Such experiments are outside the regime of atmospheric baroclinic instability but are relevant for the formation of convective columns in stars or fluid planet cores (see e.g. Busse and Or 1986, Convection in a rotating cylindrical annulus: thermal Rossby waves, JFM). In contrast, the differentially heated rotating annulus is a simplistic model that captures the essential dynamical processes driving the circulation of the terrestrial atmosphere at mid-latitudes, particularly the large-scale waves and the meandering of the jet stream. Such a laboratory model has been extensively used to understand the mechanism generating large-scale atmospheric waves (baroclinic waves): the so-called

sloping convection (see Lorentz 1967, Hide and Mason 1974). The jet stream is driven by baroclinic instability which does not depend on β -plane effects. The most simple model for linear baroclinic instability is the Eady model where β -effects are excluded. Similarly to theoretical models aiming to capture the most essential aspects of the dynamics, the differentially heated rotating annulus experiment does not include processes that bring secondary effects into the dynamics, among which is the β -effect. Another type of laboratory experiment, consisting of a rotating cylinder with sloping boundaries, can be used to investigate β -effects and generate topographic barotropic Rossby waves. The addition of such sloping bottom in the baroclinic set-up has been studied, even if the analogy with the planetary β -effect is no longer exact. In general, the sloping bottom modified experiment is reported to excite more complex dynamics with the formation of multiple jets in some cases and direct and nonlocal exchange of energy between the eddies and the zonal modes (see Wordsworth et al. 2008). We plan to do further experiments including beta-plane effects in the future.

We have rewritten the paragraph to better explain the analogy between the experiment and the atmospheric waves.

Comment:

Line 80. What is the rotational Froude number for these experiments? Is it far less than unity?

Reply:

The centrifugal Froude number, defined as $Fr = \Omega^2 (b-a)/g$, where $(b-a)$ is the gap width, is in the order of 5×10^{-3} and hence much smaller than 1.

Comment:

Line 85. It says that the temperature in the outer gap is held fixed, but elsewhere it says the heating power is held fixed. So what is actually fixed? A time series of the bath temperatures showing thermal equilibration and then the final experimental phase would make all this clearer to the reader.

Reply:

The heating power is held fixed. The system is then let to reach thermal equilibrium with the outer and inner baths setting to a constant temperature that is maintained throughout the entire duration of the experiment. We have added an appendix with details about this and figures showing the time series of the temperature bath, as suggested (see also reply to a similar point of RC2 and figure attached).

Comment:

Line 119. Why does increasing Ta lead to a more turbulent flow? How do you define turbulent? Is it the value of the Reynolds number or is it the variability / irregularity of the flow field? Typically, higher Ta gives more organized rotating flows. Here, I think it makes the Rossby radius smaller, eventually allowing for multiple structures to fit within the fluid gap. Once this can occur more complex solutions can develop. Is that correct?

Reply:

Rotation makes the flow more organised since it becomes more 2D. However, 2D turbulence can develop in such flows and this kind of turbulence is relevant to our experiment. Assuming an aspect ratio of about 1, $Ta = (1/Ek)^2$, where Ek is the Ekman number. For small Ek , viscous damping is small compared to advection and waves and vortices can propagate larger distances. For geostrophic turbulence, where the Rossby number has to be small, the Ekman number needs to be small too and hence $Ta \ll 1$. Increasing Ta leads the flow to regimes where the waves undergo amplitude and/or structural vacillation, i.e. they show more irregular features with respect to time and space until there is a regime transition to geostrophic turbulence (as shown by other studies such as Hide and Mason 1975). We used "turbulent", similarly to what is often done in the literature, to indicate the irregular regime, which is analogous to geostrophic turbulence. It is correct that the Rossby deformation radius is reduced and indeed by increasing rotation only, experimentally one observes an increase in the azimuthal wave number in the regular baroclinic wave regime. However, our study focuses on high Ta and low Ro_T , which corresponds to a regime where the waves are in an irregular state (or very close to the transition). In this case, the behaviour of the waves is much more complex as nonlinear interactions between the dominant wavenumber and the sidebands occur. We have expanded the section explaining the regimes in more detail.

Comment:

Table 1 appears at odds with my arguments above. The irregular m cases correspond to lower m values. Why would lower m be more complex? Also, why is lower Ro_T corresponding to fewer structures? I would expect from a Rossby radius argument that this would be just the opposite of what is reported here. Again, please explain this in more depth.

Reply:

The main consequence of decreasing Ro_T is that the flow becomes more complex due to several wave interactions, as discussed above. The spectra become wider, and it is more difficult to associate a dominant wavenumber to characterise the flow. We agree that Table 1 is potentially misleading the reader and have changed it to show a range of the most energetic wavenumbers for the lower Ro_T .

Comment:

Line 158. It is not clear how the c_M equation is used to generate the data in Figure 5. It is a bit opaque as to how the thermography data is processed to measure $d\Phi/dt$. How is this done, operationally? ---Further, it is not clear why $c_M \approx U_T$.

Reply:

We have rewritten the text in the hope that it is now clear to the reader how c_M is obtained from the data. To calculate c_M from the temperature data, we have extracted the temperature along a fixed radius R_d and plotted them as a function of time. In this way, we obtain a Hovmöller diagram with the azimuthal coordinate φ on the y -axis and time on the x -axis from which we can calculate the zonal phase speed graphically. The wavefronts appear in this plot as tilted lines of maximum temperature, and their slope is the zonal phase speed at which the baroclinic wave is travelling. We obtained the drift speeds by measuring the slope of ten wavefronts and then calculating the mean value and the associated standard deviation. This procedure is repeated for each experimental run associated with a different ΔT .

If we use the linear theory proposed by Eady, the waves can be assumed to be travelling with the same speed of the background flow and therefore $c_M \approx U_T$.

Comment:

Line 170. It is stated that the phase speed predicted is close to the measured c_M values. But this is not the case since $\zeta \sim 1/2$. That suggests $c_M \sim U_T^{1/2}$, which is far from $\sim U_T^1$. Again, please clarify.

Reply:

The Eady model can be used to make some quantitative predictions for the experimentally observed baroclinic waves, keeping in mind that the model is highly idealised and therefore some differences can be expected.

The differences in the phase speed and also Figure 5 show that simple flow estimations based on the linear theory by Eady are quite limited and cannot easily be carried over to large-amplitude nonlinear waves.

We have added this remark to the revised text and corrected a typo:

"Differences between the theory and the real flow notwithstanding, the zonal phase speed c predicted by (5) is close ..." there should have been "...by (4) is close..."

Comment:

Section 4: I had trouble connecting the first 3 sections to the analysis in sections 4 and 5. A schematic or two showing how the ARW dynamics lead to these different thermal field properties would be greatly appreciated I believe.

Reply:

In the first three sections, we describe the temperature influence by considering flow features. In section 4 and 5 we focus on statistical properties that depend on the temperature. It is not always possible to directly connect the statistical results with the Rossby wave dynamics and hence schematics cannot be drawn at this stage. We try to connect the statistical results by referring to other works (e.g. Linz et al. (2018) or Dai and Deng (2021)). Moreover, by comparing the findings with the one from reanalysis data in section 5 we hope that our statistical results obtain more reliability. In the introduction of the revised manuscript, when describing the structure of the paper, we point out more clearly that section 4 and 5 focus more on statistics. We think that the reader can then better see the focus of the individual chapters.

