Review for EGUSphere of Wirth et al "How to diagnose barotropic Rossby wave resonance along a circumglobal jetstream?

Isaac Held (July 25, 2026)

This paper is the latest addition to a series of papers by Wirth and collaborators on "Rossby waveguideability" and the possibility of quasi-stationary Rossby wave resonance, in the context of the linearized barotropic vorticity equation. It has been argued that resonances of this kind, trapped in a waveguide created by a strong jet, create sensitivity to the mean flow and can be relevant for aspects of extreme weather and climate change. This claim is important and controversial.

Some papers that make these claims use an approximate method to determined which zonal flows support resonances in this kind of barotropic model, focusing on the existence of trapping between two turning latitudes for a given zonal wavenumber. The present paper compares this approximate approach with explicit numerical solutions to the barotropic vorticity equation linearized about a zonal flow and forced by a localized topographic source. They conclude that the "turning latitude-only" approximation can be misleading. The implication is that our field needs to refrain from claims of relevance of this resonance picture to climate variability and change until we understand it better. I agree with this conclusion and that it is important to approach this problem, as here, from theoretical directions as well as from observations and GCMs. I recommend that this paper be published after an adequate response to the following comments.

This paper is an addition to a fairly extensive series by these same authors on this topic, all of them focusing on linear barotropic dynamics. These previous papers are frequently cited here and readers could go back and study the whole series (I suppose), but I strongly recommend a paragraph in the introduction patiently describing what is covered in the previous papers and how this new paper fits in.

The numerical solutions considered here all have significant "leakage" due to propagation into the tropics, so all of the resonant peaks have substantial width. It is not surprising that one can find cases where these results differ from those obtained with an approximation that is based on a picture of very narrow resonances. One could try to replace the narrow resonance picture with a leaky waveguide picture, but it is not clear how best to do this in a spherical model. If you are going to use a barotropic theory to quantify waveguideability, the authors argue, why not just use the numerical solutions that they describe rather than worry about approximating them further. This makes sense to me.

But there is a bit of art required in designing the latitudinal structure of the damping in their numerical model, which has some arbitrary aspects. On the equatorward side, working with flows with critical latitudes and then letting these determine where the damping occurs would be more physical than starting with solid body rotation for which you have to arbitrarily add damping to avoid propagation to the equator. I am nto suggesting that the authors need do this, but because it is a prominent part of the solutions, it would be nice to demonstrate that this leakage is not sensitive to the damping strength, which is plausible since there is no damping in the jet region itself. Perhaps this is discussed in a previous paper; or perhaps a figure could be added to show the dependence on damping strength. In any case, this issue should be discussed.

There is a brief section describing tests of WKB as a possible compromise between the approximation depending only on the location of turning latitudes and the full linear solution. This amounts to using local dispersion relation and integrating in latitude to estimate the total phase change between the turning latitudes, taking into account the phase change between the incident and reflected waves at each turning latitude using standard formulae, and then checking if the resulting phase is consistent. This quantization requirement will reduce the number of resonant states. But this section does not address the leakage issue as I understand it – the wave propagating into the tropics after tunneling through a turning latitude should affect the phase of the reflected wave, thereby affecting the quantization condition for the modes, or am I mistaken?

Also, it is standard when working with WKB in a barotropic model on the sphere to transform first to Mercator coordinates, which results in an equation that is identical to the Cartesian version, albeit on an meridionally infinite plane, eliminating the first derivative in the equation and making WKB more accurate. Couldn't this change a quantitative comparison of the full and the WKB solutions? I think this section needs more substance, perhaps including a figure showing the amplitude and phase in the WKB and full solutions for a few representative cases.

A minor point: The authors discuss the absence of an "l= 0 mode" on solid body rotation on a sphere, which contrasts with its presence in the simplest \beta-plane model in a channel (discussed in an earlier paper).   I don't see how this "mode" can ever be physical. In the channel model with no topography, if \psi =0 at the wall and \partial \psi/\partial y = 0 for all y, then the only solution is the trivial one with \psi = 0 everywhere. Unless I am totally confused – I admit to getting on in years -- I suggest removing the discussion of this "mode" in this paper, where it does not appear in any case. I am not sure what the issue is in the previous paper on the channel model.

There are a few places where the authors get carried away, in my view, in their language describing the papers that they are critiquing. I suggest they go through the paper and soften this language where needed (I trust them to locate these). We are all trying to do our best.