Review of: " Revisiting barotropic instability from the perspective of wave evolution theory" by Yaokun Li

The author presents an interesting way of viewing barotropic instability and barotropic normal modes in terms of the evolution of wave energy, wave amplitude and wave activity along wave rays, by deriving an equation for how the group speed converges/diverges along wave rays and relating it to the above quantities. The analysis is broken into 3 wave-geometry cases, where there is a turning latitude, a critical latitude and an unstable flow which has both (I think..).
I find the idea really interesting, but as written the paper does not fit Weather and Climate Dynamics. Specifically the physical setups of the different cases discussed and their relevance to the real atmosphere are not clear and should be supported with schematics.

The paper contains a lot of derivations of, of different basic setups, and different sub-cases for each, and it is challenging to visualize in order to understand the main point and relevance to the atmosphere. Also, large parts of the derivation are still not quite clear to me (given that I don't have unlimited time), and though I read through the whole paper, I am only able so far to provide detailed feedback up to section 3.1 including. Specifically, I have not verified for myself the derivations of section 3.3 so can't say whether I agree with the statements on over-reflection. I think once the author adds some schematic and clarifies some points about the setup and underlying approach, I will be able to say more about the latter part.

Following are more detailed comments listing what confused me most.

A main point which bothers me is the relevance of the analysis in point 3.1 for the observed midlatitude flow (which I take it is what the paper is aimed at helping explain?)
Specifically, the setup of a turning point south of the westerly jet, with the wave propagation to the south of the turning surface is opposite what is found in observations, for the case of waves propagating along a jet and being reflected equatorward from its poleward flank. A more realistic setup would be a turning point in a region of negative meridional shear, for example. It is also not explicitly stated if dU/dY and beta* are constant or not for this case.
A schematic of the mean flow, where in the atmosphere this is relevant, and what phenomena does it represent are needed to convince the reader of the relevance of this analysis to the real atmosphere.

When the wave field consists of and incident and reflected wave, the group speed is not defined. Rather the wave consists of two waves with oppositely directed group velocities. Specifically, the sentence on lines 130-133: strictly speaking, the back and forth reflection allows the meridional wavenumber to keep constant, but only if you take into account the superposition of the two meridionally reflected waves. This means the solution is not a pure plane wave, and the relevance of the ray tracing formulation for understanding the actual amplitude is not clear. This point is discussed in Harnik (2002).

Paragraph on lines 144-152: another fundamental difference between ray tracing and the index-of refraction type of analysis that is at the heart of over-reflection theory is the inability of ray tracing to represent tunnelling - see e.g. Harnik (2002).

Also, I am not sure I fully follow the sentence starting with "this contrasts" - over-reflection requires the waves to approach the critical surface via tunnelling through an evanescent region. This can happen also when the critical surface does not coincide with the turning surface (inflection point). See e.g. Harnik and Heifetz (2007)

Sentence starting on line 168 through equation 12 - this is the central point of the underlying theory on which this paper is based. I confess that I have not fully internalized the argument of why div(Cg) is not well defined and why it can be replaced with equation 12, and it would require more time than I can spare for this. Given that this is published already, and that I can vaguely see that this could hold, I have continued the review assuming this is correct.

The derivation of equation 15 took some time, and the way it is phrased suggests only equation 11 is used, while actually also equation 12 and A=E/w' were used. I suggest at the very least stating this explicitly, but adding an appendix derivation will help.

The explanation on lines 197-202 is a bit cumbersome. I think it is much simpler to state that the anomaly extract energy from the mean flow when the anomaly is tilted against the shear and it return energy to the flow when it is tilted with the shear.
Similarly the sentence in parenthesis on line 216-217 can be changed to "mediated by structures tilting against the shear". Also the next sentence- "conversely, lambda<0 indicates a tilting with the shear... "

Section 3.1: I don't understand the sentence on lines 231-232.

Please add the derivation of conditions 23 and 25 to an appendix (and remove "It is easy to derive" - on lines 235 and 241).

Please add a schematic illustrating the different cases discussed in page 10 - a schematic of the flow profile, the turning surface and what the different phase peed ranges imply about the value of U-c, and the implications for the wave geometry would be helpful.

Line 248 and the discussion of an upper limit of group velocity is very confusing.
is c_max in equation 26 a phase speed or group speed?

The only way to understand this is as follows:
You state that it is there "to make sure the ray can arrive at the northern turning point". This as far as I understand requires a real meridional wavenumber l, or a positive l^2 (I can't think of any other way by which the phase speed affect the ability of the ray to reach the turning latitude except for the condition that l^2>0 south of this latitude.)

assuming l^2>0 south of the turning latitude, you then mean to say that the maximum zonal group speed c_gx value is obtained when l^2=0 but then the group speed would be U+beta*/k^2 and not U-beta*/k^2.
The phase speed for this case will actually be as written in equation 26 but what insures that this is the maximal phase speed? without knowing how beta* varies in space (assuming dU/DY>0 as stated at the beginning of this subsection).

Moreover, when I try to think about any conditions on the phase speed that will insure that the meridional wavenumber is indeed real south of the turning latitude I only come up with an opposite argument: If we compare the meridional wavenumber squared for two phase speeds, c1 > c2, then l^2 will be larger for c1, as long as c<u, which implies a lower limit for c in order to insure a positive l^2, not an upper limit.
Please explain this argument more clearly, and again, supporting it by a schematic of a concrete flow configuration will help.

Sentence on lines 256-7: I am not sure I follow. Why does dE/Dt have to be zero at the turning latitude? you said it vanishes at y_m..

Discussion from line 260 to end of section: It will help to have schematic of the wave geometry of the different phase speed cases, drawn on the mean flow (U and beta*). I was not able to follow the point about the fast phase speed range- I have a feeling I am missing something basic in the way you view the problem. Specifically the discussion of the critical latitude starting on line 269: unless beta*=0 this line separates wave propagation from evanescence. Also, while the EP fluxes suggests wave activity emanates from teh critical line for unstable waves, why do the rays that emanate from it are most important for the instability to be able to exist? Instability should occur no matter where you place a wave source, no?

Minor:
Line 35 - the absolute vorticity (not its gradient) has an extremum, the gradient changes sign, no?

line 59 fix typo (w..)

line 104- insert the definition of the material derivative

Line 248 it should be "phase speed" not "group velocity" at the end of the line, no?

line 318: it is easy to *show*

References:

Harnik, N. 2002: The evolution of a stratospheric wave packet. JAS 59, 202-217

Harnik N. and E. Heifetz, 2007. Relating Over--Reflection and Wave Geometry to the Counter Propagating Rossby Wave Perspective: Toward a Deeper Mechanistic Understanding of Shear Instability. J. Atmos. Sci.64 2238-61.