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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the climatological probability of the vertical propagation of stationary planetary waves" *by* K. Karami et al.

Anonymous Referee #1

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The authors propose a new method for evaluating the climatological conditions for the propagation of stationary Rossby waves as a function of latitude and height. This is based on calculating a "probability of favorable propagation condition for Rossby waves" (PrRo). I found this paper interesting and it is quite well written. This work has the potential to be a useful contribution to the literature, but I think some shortcomings of the work first need to be addressed.

Major comments:

- It is asserted many times in the text that the proposed diagnostic for assessing the propagation conditions for Rossby waves is superior to previously proposed diagnostics or that other diagnostics are unsatisfactory, but the justification given for this seems weak. It relies principally on qualitative characteristics of wave propagation deduced

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from studies using idealised models of linear waves, summarised in table 1. However, the assertions made in the text that other diagnostics give unsatisfactory results are often not readily apparent to me in the figures. Also, the usefulness of such a diagnostic would be to better understand wave momentum fluxes in the real atmosphere, and it is not clear to me at least how well the results of the idealised studies predict the behaviour of the real momentum fluxes. The best way to demonstrate the usefulness of the new diagnostic would be to include more information about the EP fluxes for different wavenumbers, and show how well these fluxes correspond to what is expected given the diagnostic. For example, EP flux vectors could be plotted on top of the data shown with filled contours in figures 4, 8, 9 and 14. (In addition, I think it would be helpful to show EP fluxes on the panels in fig. 11 rather than separately in fig.12, to more clearly show how well the EP flux differences between the weak and strong vortex regimes correspond to the difference in PrRo.) I also think the horizontal component of the EP flux needs to be considered. These diagnostics were presented by Li et al. (2007), work which this paper is attempting to extend, and making these changes would bring the paper up to a similar standard to that work.

- I cannot find the definition of refractive index given on p.32294 in either of the given references Andrews et al. (1987) or Matsuno (1970) (and Kalnay et al. (1996) does not appear to discuss the refractive index, contrary to what is stated), and it is not obvious how the given definition could be derived from the material in any of those sources. Please give a reference for this quantity, or indicate how it is derived from the quantity given in another reference. The definition used here also has a dependence on meridional wave number which is not present in the definitions used in the other references (and which seems crucial for the conclusions relating to the propagation of waves with different meridional wavenumbers). Some discussion of the different physical assumptions made to arrive at this quantity compared to, say, those used by Andrews et al. (1987) to arrive at their equation 5.3.7 is therefore important to include.

Other significant comments:

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- The importance of setting mu_Ro to zero for nˆ2>600 is not demonstrated anywhere, and it would be useful to know how important this added complexity to the diagnostic is. Could analysis similar to what is shown in fig.14 be done for the case with mu_Ro set to 1 for nˆ2>600 and set in the same way as shown in fig.7 for nˆ2<600?

- The paper compares the new diagnostic with using the time-mean refractive index squared (MRIS in the manuscript), which is shown to be a much noisier quantity. A better diagnostic than the MRIS may be to take a "trimmed mean" of the refractive index squared, where the top and bottom X% of the data at each (y,z) position are excluded before taking the time-mean, where X could be 10, say. This would help to reduce noise by excluding very large positive or negative values. It would be useful to know whether this method performs much better than using the MRIS, and how much of an improvement the new diagnostic makes on this method.

- I think it should be made clearer that the PrRo diagnostic is likely to be most useful as a qualitative indicator of wave propagation, rather than as a quantitatively accurate tool, given the limitations of linear wave theory on which it is based.

Minor comments:

p.32293 L20-22 - the comment about focussing on vertical propagation seems unclear. If "there are also many studies using refractive index studying the horizontal propagation of the planetary waves", then it would seem important to look at the horizontal propagation. As I said above, I think the horizontal fluxes should be considered in the analysis.

p.32294 L6-7 - missing brackets around citation.

p.32295 L10 - "probabilities", not "PDFs"

p.32295 L22 - The flux has a minimum rather than a "discontinuity"

p.32295 L25 - the divisor in the mathematical expressions should be "10m/s" rather than "10"

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p.32295 L26 - either "troposphere" should be "tropopause", or "at the" should be "in the".

p.32295 L26 - p.32296 L2 - this sentence could be made clearer e.g. "at these" -> "in the same"; "taking away the u-bar" -> "dividing by 10m/s rather than by u-bar"; "maxima is" -> "maxima are".

p.32296 L12 - what does "upper and lower limit" refer to?

p.32296 L15-16 - using the same nˆ2 notation to refer to both nˆ2 at different times and the time-averaged nˆ2 is confusing. Perhaps say "nˆ2(y,z) at different sampling times" rather than "nˆ2(y,z,t)"?

p.32296 L20 and p.32300 L15 - acronyms need to be defined (normally this is done separately in the text and in the abstract. In any case, it would be helpful for the reader for them to be defined again here).

p.32297 L11 - I think the u-bar<0.5m/s definition doesn't need to be given here, as it is given below, where it is more relevant.

p.32298 L17 - "greater" should be "great"

p.32299 L16-19 - it would be useful to clarify here to refer to figs. 8 and 9 again.

p.32299 L26 - is this really "the most important information" or just a significant piece of information?

p.32300 L15-16 - some people may expect "weak vortex events" to correspond to sudden stratospheric warmings (SSWs), which is not what you mean. Perhaps your events should be named something like "weak westerly vortex events"? (It would also be interesting to know how your diagnostic performs in SSWs.)

p.32300 L18 - it would be helpful here to give an equation or reference for the critical Rossby wave velocity.

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p.32302 L4-6 - I suggest "study the difference in stationary Rossby wave propagation between different meridional wavenumbers" in place of "study the climatological effect of meridional wavenumbers on stationary Rossby waves propagation".

p.32303 L1 - English is unclear.

p.32303 L2 - some explanation of the choice of constants used in equation A2 would be helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32289, 2015.