

Weather Clim. Dynam. Discuss., referee comment RC3  
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## Comment on wcd-2021-21

Anonymous Referee #3

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Referee comment on "Observed wavenumber-frequency spectrum of global, normal mode function decomposed, fields: a possible evidence for nonlinear effects on the wave dynamics" by André Seiji Wakate Teruya et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-21-RC3>, 2021

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General comments:

The present study investigates the zonal wavenumber-frequency spectrum of zonal wind field (at 200 hPa) as reconstructed for each type of wave (Rossby, westward/eastward inertia gravity, Kelvin, and mixed Rossby waves) based on the normal mode decomposition approach. The authors attribute the deviation of the observed spectrum from the theoretical expected behavior to nonlinear processes.

I would agree that this approach may be useful for classifying and understanding of the equatorial wave spectrum from a different perspective. However, I have a major concern about the interpretation of the results. Especially, (as mentioned in Introduction of this manuscript) the authors need to be aware that the normal modes used for this study are obtained under a quite ideal condition: motionless and frictionless atmosphere that has a rigid lid at the upper boundary. So, at least for me, it is no surprising that the results are different from the expected mode behavior. I would like to suggest that the authors make it clear what is the advantage of using this approach under such ideal assumption and that the interpretation of the results be carefully reexamined. Please see below for specific comments.

- One should note that the normal modes used here are obtained under a quite "ideal" condition: i.e., motionless and frictionless atmosphere without meridional gradient in background temperature and with a rigid lid at the upper boundary. In the real world, these assumptions are of course not valid: for example, even a presence of background zonal wind modifies the mode shape; there is no upper boundary (in this case the vertical structure equation is no more a Sturm-Liouville problem). Thus, it is no surprising that for example large Kelvin wave signals in real data contaminate other wave spectra such as WIG and EIG (Sections 4.3 and 4.4). I think not only nonlinear processes (see also my comment #3 for this point) but also other quite basic processes

such as again the presence of background wind and dissipation processes, which could be considered within a "linear" theory, are all responsible for the deviation from the ideal mode behavior.

- The 2D spectrum results are all shown as the ratio to the background spectrum. This could be misleading if the background spectrum is very small: i.e., Even if the signals appear to be significant for the ratio, the actual wave energy could be very small. I suggest that the authors should also show and examine the raw (or background) spectrum as well. For instance, I would suspect that the background spectrum for the eastward components in Fig. 7 (ideal WIG modes) may be small.
- (Following my comment #1) The authors' arguments about the nonlinear interaction (e.g., L264-267, L326-330) sound just a speculation and not so convincing and I think some more quantitative analyses and/or discussion are necessary. For L265-267, for example, most readers including me may not be familiar with plasma physics and I cannot understand why such specific process (of other many processes) is considered the most important. I would guess, again, that there are many possibilities for the deviation from the ideal normal modes (i.e., background wind etc.).