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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Role of equatorial planetary and gravity waves in the 2015–16 quasi-biennial oscillation disruption" *by* Min-Jee Kang et al.

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General Comment:

This paper provides quite many missing pieces of the 2016 QBO disruption puzzle. Previous literature has concentrated on the role of equatorward-propagating extratropical waves, with only a couple papers giving equatorial wave modes any focus. Overall, the effort made by the authors is quite impressive. All equatorial wave modes are separated and their effect on the wind structure is studied in detail, and additionally parameterized convective GWs are included in the study. As the authors mention, of course the CGW parameterization has some limitations (which parameterization doesn't?),

but I think the approach is physically consistent and the CGW results are, at least, qualitatively correct. The paper is a bit lengthy, but short summaries are provided for the trickier figures, which I appreciated. Overall well written, figures are mostly good, a nice sketch in Fig. 16 to put all together, interesting discussions and definitely worthy of publication in ACP. I only have a fair amount of minor comments, mainly to make some figures or methods easier to interpret for the reader, and in some parts to request a more detailed comparison with recent QBO disruption literature.

Response: First of all, we would like to thank the reviewer for the time spent on reviewing our long manuscript. All reviews have been beneficial and made us aware of important points which had to be addressed. We, the authors, are therefore thankful for the reviewer's contribution to improve the manuscript. We carefully addressed all comments and tried our best to improve the manuscript based on the suggestions and comments.

Specific Comments:

1. Title: strictly speaking, it's not only planetary wavenumbers that you study, I suggest something more accurate "Role of equatorial waves and convective gravity waves in the 2015/16 QBO disruption" or similar (it's ok to use the QBO abbreviation in the title, since all your potential audience will know what it means)

Response: Thank you for your good suggestion. Following the suggestion, the title is changed to "Role of equatorial waves and convective gravity waves in the 2015–16 quasi-biennial oscillation disruption". [p.1, L1–2]

2. p.2, l.31-33: specify that this is for extratropical latitudes when you refer to polar vortex and its downward impact.

Response: It is specified as suggested. [p.2, L32–33]

3. p.2, l.55: 'is the prerequisite for' I think a better wording would be '... eq. waves preconditioned the extratropical Rossby wave breaking, ...'. prerequisite implies that

without the MRG, the ex. Rossby wave breaking and QBO disruption wouldn't have happened, which to my knowledge cannot be assured 100%.

Response: We agree with you. The sentence is modified as suggested. [p.2, L56]

4. p.2, l.56: '... each equatorial wave mode to the QBO...'

Response: It is changed as suggested. [p.2, L57]

5. p.3, l.87-88: sentence is repetitive, can be removed

Response: The sentence is removed as suggested.

6. p.3, l.89-93: specify subsections where each item is done, e.g. specific wave types within section 3

Response: Thank you for your good suggestion. We specified subsections where each item is done. [p. 3, L90–93]

7. p.6, l.178: include the GPM dataset into section 2.1

Response: Thank you for pointing this out. We included the GPM dataset in Sect. 2.1. [p. 4, L106–108]

8. p.6, l.179: Here or perhaps within the supplement, justify why the magnitude and scaling are not crucial for both datasets in Fig. S1 to match: not the exact magnitude, but rather the shape of the vertical profiles is what's important, correct?

Response: Thank you for your comment. The spatiotemporal variations in the magnitude of convective heating rate are important, but the overall magnitude largely depends on the resolution of the data provided. Therefore, the 'exact' magnitude would be less important compared to the shape of the heating profiles. The difference in the overall magnitude can be adjusted by the conversion factor (Kang et al., 2017), a scale factor that constitutes convective source spectrum, when calculating convective gravity wave momentum flux. The related sentence is included in the revised supplement.

[Fig. S2]

9. p.6, l.180: SPB –> Since this abbreviation is not used later in the manuscript, I recommend to keep the full name, and remove the abbreviation from the first time it's used

Response: It is modified as suggested. [p.3, L85; p.7, L186]

10. Figure 1: I suggest making 1(a) the lat.-height sections and 1(b) the vertical profiles and refer to them accordingly in the text. Also, make it clear in the caption that the climatology is for WQBO years. Y-axis labels: for better visibility I'd keep the pressure levels only, since the numbers with height in km get mixed up in between panels.

Response: Thank you for your good suggestion. Following the suggestion, we marked (a) and (b) in Fig. 1 and deleted labels of the height axis. We also clarified the meaning of the climatology in the figure caption of Fig. 1 in the revised manuscript. [Fig. 1]

11. p.7, l.188: In section 2.1 you say it will be referred to as WQBO climatology, you may want to rephrase that for consistency. Here, remind the reader that when you're talking about climatology, it refers to WQBO phase.

Response: Thank you for pointing this out! We revised the statement in Sect. 2.1 that the 10-selected years will be referred to as the 'climatology' for conciseness. We also remind the reader of this in the results section. [p.4, L114; p.7, L205–206]

12. p.7, l.199-200: '... positive (negative) drag on the zonal wind in regions of positive (negative) shear, ...'

Response: It is changed as suggested. [p.8, L217–218]

13. p.7, l.212: February –> March

Response: Thank you! It is corrected. [p.8, L234]

14. p.7, l.195-213: I miss some linking of these results with recent literature in this

paragraph: –> How do these results compare to Lin et al. (2019)? Especially for MRG, IG and CGW, it should be highlighted your results build upon those by Lin et al. (2019) (their Fig. 3 is quite similar to yours), that focus mostly on MRG in their discussions. Your eq. wave differentiation is more detailed, which is a big plus. –> Regarding your Kelvin wave forcing results, I find it really neat that it helps maintain the two westerly jets. This was suggested/hinted by Li et al. (2020), who studied the long-lasting westerly jet around 20hPa, but your results really confirm this is the case. Li et al. (2020) also showed above-average upper-tropospheric Kelvin wave activity linked to El Nino. This should be mentioned/discussed within this paragraph. –> Also, this is a very long paragraph, could be easily split into 2-3.

Response: Thank you for your detailed comments. We modified the paragraph as suggested: - We included a comparison between Fig. 2 and Lin et al. (2019)'s Fig. 3. We also emphasized that the contribution to the QBO disruption from the IG waves is first shown in this study. - We included a discussion on the long-lasting jet near 20 hPa and its contribution from Kelvin waves, referring to Li et al. (2020). - We split a paragraph into three. [p. 8, L221–235]

15. Fig. 3: I suggest to move the climatology plot to the supplement (as it doesn't have crucial information for the main text), and make the rest bigger. Right now this figure is too busy and it's easy to get lost within, specially with the duplicate letters for the sub-panels.

Response: Thank you for your comment. Although we respect the reviewer's suggestion, we decided to keep the figure in the revised manuscript, as two figures (disruption and climatology) together can clearly show how the structures of the wind and wave forcings are unusual during the disruption. We instead changed letters for sub-panels and adjusted each figure to increase visibility as much as possible. [Fig. 3]

16. p.9, l.253-254: Mention to Lin et al. (2019) needed here. Your results are in line with theirs, plus the addition of IG in the preconditioning.

Response: We mentioned Lin et al. (2019) in the revised manuscript as suggested. [p. 10, L279–280]

17. p.10, l.294: origination –> just 'origin'

Response: It is corrected. [p.11, L325]

18. p.11, l.324: I suggest making this (dividing by density) for all figures: the vertical/ horizontal arrows without scaling for pressure in the previous plots, may lead the reader to underestimate upper level wave propagation at first glance, specially if one is quickly comparing upper tropospheric and stratospheric levels without paying much attention to the legend. However, this is not a must: the figures have the divergence in pressure-independent units, and the units of the arrows are clearly shown - more than enough to correctly interpret everything with basic knowledge about EP flux.

Response: Thank you for your comment. Following the reviewer's suggestion, we made all figures divided by air density except for Fig. 2, because the EPF for the CGWs in Fig. 2 divided by air density was too strong in the upper stratosphere. [Figs. 5, 7, and 11]

19. p.12, l.369-374: This belongs in the methods section

Response: We moved the statements to the methods section (Sect. 2.5). [p. 7, L190–198]

20. Fig. 8(c-d): I suggest to, apart from making the c-d plot smaller, put the timeseries next to each other, or even merge them into one continuous timeseries.

Response: Thank you for your suggestion. We merged Figs. 8c and 8d into one continuous timeseries. [Fig. 8]

21. p.13, l.379-380: You may easily add a climatology line into Fig. 8(c-d)

Response: Following the reviewer's suggestion, we added a climatology line with ± 1 standard deviation range in Fig. 8c. [Fig. 8]

22. p.13, l.381: Could the authors perhaps provide a daily timeseries of MRG EPD (in the boxed region) to compare with the q timeseries to assess this as a source? Would be a nice addition as Fig. 8(e-f)

Response: Thank you for the good suggestion. Following the reviewer's suggestion, we added the daily EPD time series for the MRG waves as a new figure Fig. 8d, and included a discussion that the magnitude of the positive EPD increases as the number of grids with q_ÏŢ<0 increases. [Fig. 8; p. 13, L406–407]

23. Fig. 9: I suggest to translate the y-axis unit (mean rain rates) into something more relatable, e.g. mm/day

Response: Thank you for the good point. The unit is changed to "mm/day" in the revised manuscript. [Fig. 9]

24. p.14, l.417-419: This was linked by Li et al. (2020) to the El Nino event that winter. Perhaps it would be useful to add a discussion somewhere in this section, about the overall increase in eq. wave activity and precipitation together with El Nino. Also a mention to Barton and McCormack (2017) could be added.

Response: Thank you for your suggestion! We included a discussion on the enhanced Kelvin wave activity related to the El Niño event and the overall enhancement in the equatorial wave activity by referring to Li et al. (2020). We did not include a paper by Barton and McCormack (2017) here because as far as we know, their focus was not on the equatorial waves but on the midlatitude Rossby waves and their equatorward propagation. [p.15, L447–449]

25. p.14, l.440-442: There is a lot of new information and little justification about the source level here, I suggest you detail more about this in the methods section, and refer to the corresponding methods section when you start with this figure.

Response: Following the reviewer's suggestion, we included the details about the source level in the method section (Sect. 2.3) by including a new figure (Figure S1)

and refer to them when we started to describe Fig. 12 in the revised manuscript. The changes in the direction of the vertical EPF and the sign of the EPD appear at 150 hPa and below (140 hPa and above) for H_e (H_w) waves. Our focus is on the westward waves, so we simply assumed the source level of H_e+H_w as 140 hPa. [p.6, L160–162; p.15, L471]

26. p.15, l.446: A non-expert will need more explanation about the source level to understand the attribution made in this sentence. Again, this could be already detailed in the methods section and referred to here.

Response: As discussed in the Comment #25, we included more explanation in the methods section. [p.6, L160–162; p.16, L475]

27. Figure 13 and p.15, l. 447-460: I don't doubt the validity of your results and conclusions regarding Fig. 13, but it would be much easier to interpret if you showed the same plots for He and Hw separately. Mixing both makes this figure a bit confusing. Separating He and Hw will allow the reader to identify which fraction deviates more from climatology (and gets filtered by the wind shear) in a more straightforward way.

Response: We plotted Fig. 13 mimicking the phase-speed spectrum of the GW parameterization (e.g., Fig. 1 of Beres et al. 2004; Fig. 2 of Kang et al., 2017) which, together with wind profiles, can well represent the sign and magnitude of the potential GWD. Therefore, we would like to keep Fig. 13 as the original. We understand that some readers might be confused and there could be an offset between eastward and westward waves, so we provided the same figure as Fig. 13 but for the eastward and westward propagating parts separately, in the revised supplement (Figure S9). Please note that the figure is not separated into H_e and H_w but into the eastward and westward waves. This is because we defined H_e where k>0 and H_w where k<0 based on the assumption that the zonal wind is close to zero near the equator, but precisely, the direction of the waves is determined by the wind speed at the altitude where the waves are generated (i.e., source level). Since we assumed the source level as 140 hPa (please refer to Response #25), the spectrum where the phase speed is greater (smaller) than zonal wind at 140 hPa is defined as eastward (westward) waves. [p.16, L492–493; Fig. S9]

28. p.15, l.462-467: You discuss the Kawatani paper in the next paragraph. Barton and McCormack (2017) showed important ENSO influence on the background winds and momentum fluxes below 30hPa (see e.g. their plots 3 and 4). It would be worth to add it into your discussion.

Response: Following the reviewer's suggestion, we mentioned ENSO influence on the background winds in the revised manuscript referring to Barton and McCormack (2017). However, ENSO influence on the momentum fluxes below 30 hPa in Fig. 4 of Barton and McCormack (2017) is shown for the horizontal component only, while IG waves have dominant vertical component. Therefore, no further discussions regarding to the work by Barton and McCormack (2017) are made. [p.16, L502–504]

29. p.16, l.488-490: Please detail a bit more what convective source and WFRF mean for the non-expert. Convective source spectrum is related to the movement of convection itself, WFRF to the GWs emitted from it, correct?

Response: Yes, it is correct. The convective source spectrum is about the size, magnitude, and moving speed of the convection and the WFRF is about the shape of the GW spectra emitted from the convection, which is related to the vertical configuration of the convective heating, the critical-level filtering, and resonance between the forced mode and natural mode. The combined effect of the two determines the magnitude and spectral shape of the CGW momentum flux at the source level. We included more explanation in the revised manuscript, as suggested. [p.17, L522–526]

30. p.16, l.496: higher static stability at which height range? Intuitively, deeper convection is related to tropospheric instability.

Response: Thank you for your comment! We found that the original statement was

somewhat unclear. We included the height range, 200–300 hPa, and modified the sentence more clearly referring to He et al. (2019) in the revised manuscript. According to He et al. (2019), high tropical static stability appears under global warming because of the enhanced surface temperature resulting in a smaller moist adiabatic temperature lapse rate. Please note that 2016 is the warmest year on record for the global-mean surface temperature (GISTEMP Team, 2020). [p.17, L531–533]

31. p.16, l.497: please use relative terms: warmer / colder

Response: Thank you for your suggestion, but the related sentence was removed during the revision process.

32. p.16, l.497-499: Rephrase this sentence to make it clear that El Nino increases overall amount of convection in the tropics. There must be earlier studies (probably mentioned in the Domeisen review paper) providing this relation to say this with more certainty than 'possibly triggered by El Nino'

Response: Thank you for your suggestion. We rephrased the sentence that El Niño increases the overall amount of convection in the tropics referring to Geller et al. (2016) and Kawatani et al. (2019). [p.17, L529–531]

References

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Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-791/acp-2020-791-AC1-supplement.pdf

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