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Atmospheric Chemistry and Physics
Discussions
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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 gives a comprehensive overview of the different wave forcings that act during the 2016 QBO disruption. The paper discusses the effect of wave forcing by extratropical Rossby waves, equatorial waves, and small scale convective gravity waves, and is therefore the most complete description of the QBO disruption so far. The paper is very well written and fits well into the scope of ACP. The paper is recommended for publication after addressing my minor comments as detailed below.

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Discussion paper

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.

Main Comments:

1. For the spectral analysis time segments of 90 days are used after applying sine and cosine windows at the first and last 30 days. According to Parseval's theorem, this will lead to an underestimation of spectral amplitudes, and, on average, EPF and EPD will be ∼30% underestimated. This relatively small effect will not affect the basic results of the paper, but it should be mentioned.

Response: Thank you for pointing out this part. We already recognized that the variance of the 90-day time series becomes two-thirds of the original variance after applying sine and cosine windows at the first and last 30 days, respectively. Therefore, in our calculation, EPF and EPD were multiplied by a scale factor of 3/2, which was not mentioned clearly in the original manuscript. Although no specific sentences on the scaling were given in the paper of Kim and Chun (2015), it was mentioned in a recent paper by Kim et al. (2019). The calculation of the EPF and EPD is clarified in the revised manuscript referring to Kim et al. (2019). [p.5, L148]

2. I am not sure whether the preconditioning of the QBO disruption by MRG EPD and IG EPD in October 2015 is a reliable result. Firstly, the magnitude of EPD is quite small, secondly, the EPD in October 2015 looks different in ERA-Interim, as can be seen in the supplement. Thank you for asking an important question.

Response: With regard to your first question, the sum of the MRG and IG wave forcings are found to be ∼-0.9 (-1.2) m/s/mon, which is ∼61% (55%) of the total negative forcing in October (November) 2015. We think that these magnitudes are considerably larger than what we expect and cannot be negligible. As for your second question, we included the time series of zonal wind, zonal wind tendency, and each wave forcing using ERA-I data as a new figure in the supplement (Figure S6). The contribution of each wave forcing to the total negative wave forcing is given in the below table (Table A1). It is found that the sum of MRG wave forcing and IG wave forcing in October (November) 2015 using ERA-I data is ∼-1.0 (-1.3) m/s/mon, 61% (53%) of the total negative forcing, which is very similar to that using MERRA-2 data, although MRG (IG) wave forcing is somewhat greater (smaller). Based on the large contribution by MRG and IG waves and its consistency between datasets, we concluded that MRG and IG waves precondition the zonal wind in the early stage of the QBO disruption. Please also refer to the response to the Specific Comment #6. [Fig. S6]

Specific Comments:

1. p2, l33: You may want to include more recent work on the effect of the QBO on surface weather and climate, for example Kidston et al. (2015), or Gray et al. (2018). Kidston, J., A. A. Scaife, S. C. Hardiman, D. M. Mitchell, N. Butchart, M. P. Baldwin, and L. J. Gray (2015), Stratospheric influence on tropospheric jet streams, storm tracks and surface weather, Nat. Geosci., 8, 433-440. Gray, L. J., J. A. Anstey, Y. Kawatani, H. Lu, S. Osprey, and V. Schenzinger (2018), Surface impacts of the Quasi Biennial Oscillation, Atmos. Chem. Phys., 18, 8227-8247.

Response: Thank you for suggesting more recent works. Those are included in the revised manuscript. [p.2, L33]

2. p3, l73: The reference Evan et al., JAS, 2012 does not fit here. Evan et al., JAS, 2012 discuss intermediate-scale tropical inertia-gravity waves of horizontal wavelengths in the 5000km range, and not the effect of small scale gravity waves. This reference should be replaced by Evan et al., JGR, 2012 which is a WRF simulation of the QBO forcing by small scale gravity waves of horizontal wavelengths >270km. In addition to the listed model studies you should also include observational evi-

dence of the effect of small scale gravity waves, for example Ern et al., JGR, 2014. Evan, S., M. J. Alexander, and J. Dudhia (2012), WRF simulations of convectively generated gravity waves in opposite QBO phases, J. Geophys. Res., 117, D12117, doi:10.1029/2011JD017302. Ern, M., F. Ploeger, P. Preusse, J. C. Gille, L. J. Gray, S. Kalisch, M. G. Mlynczak, J. M. Russell III, and M. Riese (2014), Interaction of gravity waves with the QBO: A satellite perspective, J. Geophys. Res. Atmos., 119, 2329-2355, doi:10.1002/2013JD020731.

Response: Thank you for pointing out the mistake and suggesting appropriate papers. The reference Evan et al. (JAS, 2012) is changed into Evan et al. (JGR, 2012). We also included observational evidence by Ern et al. (2014) as suggested. [p.3, L73]

3. p3, l88: "All" gravity waves is not correct! As stated in l.82/83, this paper discusses only small scale gravity waves of horizontal wavelengths <100-200km. However, there is also considerable QBO forcing by small scale gravity waves of horizontal wavelengths >200km, as can be seen from Evan et al., JGR, 2012.

Response: We agree with you, but the related sentence was removed during the revision process.

4. p4, l106: On the selection of WQBO cases: Please state more clearly that the definition of the WQBO as used in this paper focuses on QBO situations that are comparable to that of the 2016 QBO disruption. Generally, there should be WQBO periods or WQBO onsets also in other months.

Response: Thank you for your comment. We clarified that there should be WQBO phases in other seasons, but we only focus on NH winter to compare with the QBO disruption case. [p.4, L111–112]

5. p5, l134/135: As EPF is obtained by summing in the spectral space (l.142), applying cosine windows will lead to an underestimation of spectral amplitudes, and also of EPF, and EPD. See Main Comment (1).

Response: As discussed in the Main Comment #1, we included the sentence that EPF is multiplied by a scale factor of 3/2 to conserve its original variance. [p.5, L148]

6. p9, l253/254: I am not sure whether it is a reliable result that IG and MRG would act as a preconditioning at 40hPa in October 2015 before Rossby waves can take effect! Please note that Fig.4b (for MERRA2) and Fig.S5 in the supplement (for ERA-Interim) look quite different! The preconditioning effect that you suggest seems to be much weaker than the differences between the two reanalyses.

Response: Thank you for your comment. Please note that the time series of Fig. S5 in the original supplement (Fig. S7 in the revised supplement) is not for ERA-I but for MERRA-2 averaged over 5°S–10°S. As discussed in the Main Comment #2, the sum of MRG wave forcing and IG wave forcing is quite similar between MERRA-2 and ERA-I data in October–November 2015. The wave forcing by IG waves using ERA-I is somewhat smaller than that using MERRA-2, possibly due to a coarser horizontal resolution (0.75°). The related sentence is included in the revised manuscript. We also clarified in the figure caption that Fig. S5 (Fig. S7 in the revised manuscript) is for MERRA-2 data. [p.10, L306–307; Figs. S6–S7]

7. p10, l281-285: Another reason for this difference could be the part of the gravity wave spectrum that is neither covered by the CGW scheme, nor resolved or parameterized in MERRA2. It should be emphasized that MERRA2 is not a free-running model! There will be model imbalances that are caused by data assimilation. Data assimilation can therefore correct misrepresentations of the gravity wave forcing by the nonorograpic GWD parameterization.

Response: We agree with you that data assimilation corrects misrepresentations of gravity wave forcing by GWD parameterization which could stem from the missing GW spectrum in the parameterization. However, this fact may not be strongly related to the difference between the parameterized GWD from MERRA2 and CGWD from the offline parameterization, because the analysis increment is not included in the parameterized

GWD but is provided separately as a different variable named “total eastward wind analysis tendency” in MERRA-2 (GMAO, 2015).

8. Fig.6: Here you identify source regions of Rossby waves by positive EPD coinciding with upward directed EPF. At 15N around 150hPa EPF is directed downward coinciding with negative EPD. Do you think this is another source region of Rossby waves? As EPF is directed downward, it looks like these waves cannot propagate into the stratosphere.

Response: We agree with you, and the related sentence is deleted in the revised manuscript.

9. p11, l327: that the positive EPD region is a source region of the westward-propagating waves. -> that the positive EPD region should be a source region of westward-and upward propagating waves.

Response: Thank you for your suggestion. It is modified. [p.12, L358]

10. p12, l370: It should be mentioned that Eq.(3) includes both barotropic and baroclinic instability. Did you check which term is stronger - the barotropic term (meridional gradients), or the baroclinic term (vertical gradients)? Coy et al., 2017 claimed that barotropic instability would be stronger. In your Fig.8, this does not fit the title and the figure caption saying "baroclinic instability".

Response: Thank you for your suggestion. In the original manuscript, it was mentioned that the q_phi term is dominated by the barotropic instability; but it was not highlighted. Figure A1 below shows the same as Figs. 8a–b but excluding the third term in the right-hand side of Eq. (3), which is very similar to Figs. 8a–b. This implies that the barotropic term is much larger than the baroclinic term. This result is consistent with the finding of Garcia and Richter (2019), who showed that the reversal of the vorticity gradient was almost completely dominated by the behavior of the barotropic part, specifically by the behavior of the meridional curvature of the zonal wind. We moved explanation of Eq.

(3) to the methods section (Sect. 2.5 in the revised manuscript) and stated clearly that the Eq. (3) is dominated by the barotropic instability. Also, “baroclinic instability” in Fig. 8 is changed into “barotropic instability”. [p.7, L198–199; Fig. 8]

11. p17, l513: barotropic instability -> barotropic and/or baroclinic instability

Response: As discussed in the Specific Comment #11, we stated clearly in the revised manuscript that q_phi term is dominated by the barotropic instability. Therefore, we would like to keep “barotropic instability” here.

Technical comments:

1. p5, l124: the parameter \hat{f} is not used in Eq.(1)

Response: Thank you for pointing out this error! “Eq. (1)” is changed to "Fphi and Fz" in the revised manuscript. [p.5, L128]

2. p6, l175: is lower than 700 hPa -> is at altitudes lower than 700 hPa

Response: Thank you! It is corrected. [p.6, L182]

3. Fig.1: colorbar should be m/s, but is m/s/month

Response: Thank you! It is corrected. [Fig. 1]

4. Fig.3: one of the colorbars should refer to the wind in m/s, but both colorbars give tendencies in m/s/month

Response: Thank you! It is corrected. [Fig. 3]

5. p8, l222: (Fig. 3a) -> (Fig. 3c)

Response: Thank you! It is corrected. [p.8, L244]

6. p9, l256/257: Figure S4 is the same figure with Fig. 3 but using ERA-I data. -> Figure S4 shows the same as Fig. 3 but using ERA-I data.

Response: It is modified as suggested. [p.10, L283]

7. p9, l264: (Fig. 4c) -> (Fig. 4a, dotted line)

Response: Thank you! It is corrected. [p.10, L291]

8. caption of Fig.7, l795: (c) January 2016, -> (c) December 2015

Response: Thank you! It is corrected. [Fig. 7]

9. caption of Fig.7, l799: where the EPD is smaller than -> where the EPD is stronger than

Response: Thank you for pointing out the ambiguous expression. It is changed to “where the EPD is algebraically smaller (more negative) than the climatology”, not only in the caption of Fig. 7 but also in that of Figs. 5 and 11 for consistency. [Figs. 5, 7, 11]

10. p13, l390: affects -> affect

Response: Thank you! It is corrected. [p.14, L417]

11. caption of Fig.9, p35, l814: January 2016, and (b) -> January 2016, and (d)

Response: Thank you! It is corrected. [Fig. 9]

References

Global Modeling and Assimilation Office (GMAO): MERRA-2 tavg3_3d_udt_Np: 3d,3-Hourly,Time-Averaged,Pressure-Level,Assimilation,Wind Tendencies V5.12.4, Greenbelt, MD, USA, Goddard Earth Sciences Data and Information Services Center (GES DISC), accessed 28 September 2020, 10.5067/CWV0G3PPPWFW, 2015.

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Please also note the supplement to this comment:

https://acp.copernicus.org/preprints/acp-2020-791/acp-2020-791-AC2-supplement.pdf

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2015–16	Oct 2015	Nov 2015	Dec 2015	Jan 2016	Feb 2016	Mar 2016
MRG	-0.7 (41%)	-0.8 (32%)	-0.8 (24%)	-0.9 (18%)	-1.5 (27%)	-1.1 (28%)
IG	-0.3 (19%)	-0.5 (21%)	-0.3 (9%)	-0.1 (2%)	-0.3 (6%)	-0.6 (16%)
Rossby	-0.6 (40%)	-1.2 (47%)	-2.2 (67%)	-4.0 (80%)	-3.7 (67%)	-2.2 (56%)
Kelvin	1.1	0.7	1.0	1.6	1.2	0.9
Rossby-Y	-0.5 (34%)	-1.0 (38%)	-1.9 (58%)	-3.4 (70%)	-3.2 (58%)	-1.9 (48%)
Rossby-Z	-0.1 (6%)	-0.2 (9%)	-0.3 (9%)	-0.5 (10%)	-0.5 (9%)	-0.3 (8%)

Fig. 1. Table A1. The same as Table 1 but using ERA-I data except for the climatology.

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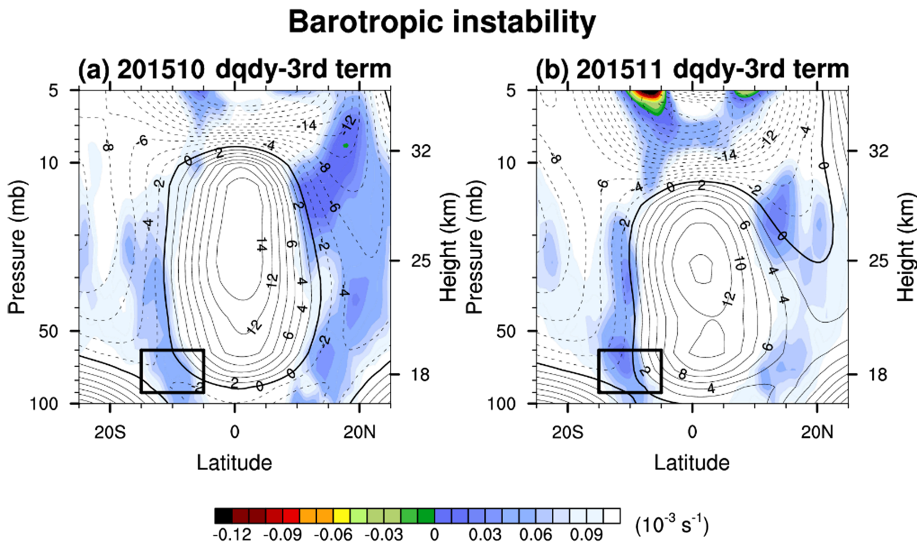

Fig. 2. Figure A1. The same as Figs. 8a–b but for q_phi excluding the third term in the righthand side of Eq. (3).

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