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Interactive comment on "Response of Trace Gases to the Disrupted 2015–2016 Quasi-Biennial Oscillation" *by* Olga V. Tweedy et al.

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Received and published: 3 May 2017

Response to comments: Anonymous Referee 1
Comments received and published: 10 March 2017

We thank the reviewer for very helpful comments. We have taken all the points raised into consideration. Our specific responses / changes are below (in *italic*).

The QBO showed a behavior in 2015-216 which has never been seen before. The development of the meteorological fields has been described elsewhere but the present paper contributes by describing how ozone and HCL changes during the event. I find that the paper contributes with new information and that it is well written. However, I have a few relatively minor points that the authors should consider before

the paper is accepted.

Major comments: In the introduction, the QBO in ozone is described. However, I find this description somewhat confusing. First of all, I miss a statement about if the ozone QBO is in phase with the QBO in the zonal mean wind. I am also confused about the statements about the seasonal synchronization (line 33 and 57). There is only a weak seasonal signal in the QBO in the zonal wind.

We have rewritten this part of the Introduction to make it clearer. Deseasonalized ozone anomalies are out-of-phase between the tropics and extratropics; the QBO in equatorial winds are in-phase with tropical ozone anomalies but out-of-phase with extratropical ozone anomalies. "Seasonal synchronization" means the QBO influence on deseasonalized ozone anomalies in the extratropics are observed mainly in winter-spring of each respective hemisphere.

There is only very little mention of statistical significance (line 214). The statistical significant regions should be indicated in Figs. 1 and 3 and the method to calculate the significance should be described in more details.

The statistical significant regions are highlighted in Figure 1c and 3c,f and method to calculate the significance is described in Methods section. Since we have very limited number of QBO cases in the observational record (5 QBO cycles in MLS and 14 cycles in the SBUV total ozone, see Table 1), we don't perform any sophisticated statistical tests (degree of freedom is very small) and simply indicate regions where absolute difference between last QBO cycle and the composite larger than 2 standard deviations.

Minor comments:
l49: downward -> downward propagating? - *changed*

C2

l88: How can temperature and ozone have different vertical resolutions (3 and 4 km) when they both are reported on 12 pressures per decade?

There is a difference between the vertical grid that data are reported and the actual vertical resolution. The vertical grid is usually finer than the actual instrumental vertical resolution. The vertical resolution is defined by the number of independent measurements (degrees of freedom for signal or DFS) that the instrument makes, and this varies between MLS temperature and ozone measurements [see MLS Version 4.2x Level 2 data quality and description document for more details]. Clarifications were made in the first paragraph of the Methods section.

l186: The authors could be more specific here. Will the interfering make it more difficult to determine the trends? In fact, one could argue that the disruption will make it easier to establish the connection between QBO and ozone and therefore easier to determine the residual trend.

This sentence was edited to make it more specific. Certainly, a series of similar disruptions would make it more difficult to determine the residual trends because we won't be able to rely on two EOFs to remove QBO variability in ozone timeseries. EOF 1 and 2 typically explain 96 percent of the variance while during the disruption it falls to only 71 percent. Thus, the first two EOF patterns don't match the disruption very well, with the lowest percent variance explained by the two EOFs in the entire data record occurring during the disruption.

Figure 2. I am not sure this figure helps and I cannot see that this analysis is used elsewhere in the paper. I would suggest that it is removed or, if the authors find it important, that also the EOFs are shown and the amount of variance they explain is mentioned.

We believe this figure is relevant and we have decided to keep it. It demonstrates how unusual and unprecedented this current QBO disruption event is. Based on Wallace et al. (1993), the first two EOFs explain 95.5 percent of the normalized variance of the deseasonalized smoothed time series of zonal winds at seven pressure levels between 70 to 10 hPa combined. We don't think additional EOF plots are necessary for this paper but discussion about the recent QBO variances is added (in Methods section)

Actually, a similar figure was shown in Dunkerton 2016 (GRL 10.1002/2016GL070921) which should be cited.

Our calculations follow the standard Wallace et al. (1993) EOF structures. The results look quite different from the figure in Dunkerton 2016, which came from a blog/twitter site.We are unsure exactly how the calculations were done in his case (and calculations are not clearly explained in Dunkerton 2016, on twitter, or the blog site). Our plot really doesn't suggest a 'death spiral' and plot in Dunkerton 2016 plot shows *NO* anomaly for the 2015-16 period! Since we don't understand this figure, how it was produced, and it was not published in peer reviewed source (except for the twitter post in Dunkerton 2016 article), we think it is best not to cite it.

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