

***Interactive comment on* “Surface impacts of the Quasi Biennial Oscillation” by Lesley J. Gray et al.**

Anonymous Referee #2

Received and published: 10 January 2018

Review of “Surface Impacts of the Quasi Biennial Oscillation,” By Lesley J. Gray, James A. Anstey, Yoshio Kawatani, Hua Lu, Scott Osprey, and Verena Schenzinger

The authors document the variability in zonal wind, precipitation, and SLP outside of the tropical stratosphere that are associated with the QBO. They do so by performing multivariate linear regression analysis of the QBO with the analyzed fields, and by testing the sensitivity to including a polar vortex index in their MLR, they are able to assess whether a given connection occurs predominantly through the vortex or through an alternate impact of the QBO. They find a QBO influence on the vortex and subsequently on the troposphere in January. In addition to this pathway, they also find a second significant signal in the North Pacific in February/March, and a third significant signal in the east North Atlantic and the Pacific unrelated to vortex variability in December. A fourth pathway is between the QBO and tropical convection/precipitation.

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This paper could eventually become a valuable contribution to the field, but there are some major issues that need to be addressed as detailed below. Briefly, the authors need to convince the reader that the responses seen are not due to aliasing of SST variability (that may or may not have anything to do with ENSO). Furthermore, the assumption of nonlinearity inherent to MLR is somewhat suspect given the results of previous compositing studies.

General comments:

1. The authors need to nail down exactly why their precipitation response is so different from previous work (e.g. Leiss and Geller). I see three possibilities. First, there is a tendency, especially since 1979, for WQBO at 50hPa to occur simultaneous with El Nino (see Garfinkel and Hartmann 2007 and Leiss and Geller 2012). Both of these studies took a compositing approach to removing possible contamination of the ENSO signal from the QBO signal, and it is possible that the present study is aliasing an influence from SST variability (see general comments 3 and 4). Another possible difference between Leiss and Geller 2012 and the present study is the period examined: Leiss and Geller 2012 end their analysis in 2011 while the present study continues to 2015. If the authors end their analysis in 2011 (to match Leiss and Geller 2012), are the results more similar? A third possibility is the choice of the QBO index used (i.e. EOF vs pressure level based). Considering the authors already compare an EOF approach to a pressure level approach for SLP in a supplemental figure 5, I suggest they create a similar figure but for precipitation.

2. I suggest that the authors compare their precipitation patterns to those simulated in Garfinkel and Hartmann 2011 (part 2 using WACCM). Their various experiments have the same SSTs, and hence any differences in precipitation must arise via the QBO and not via aliasing of SSTs. I also note that GH11 conclude that the springtime SLP and wind anomalies in the North Pacific are not due to convection but rather due to the QBO affecting extratropical eddies directly (relevant to page 19 line 2). This paper should also be added to the list on p.3 line 11. Finally, this paper is also relevant to p. 10 line

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31, as this paper proposes an answer to the question the authors raise; specifically, GH11 argue that the North Pacific circulation is more sensitive to external forcings in spring as compared to midwinter.

3. A single SST index cannot characterize the possible association between SSTs and the QBO. If one simply regresses (or composites) SST anomalies during different QBO phases (Huang et al 2012; Hu et al 2012), differences will appear in more than just the Nino3.4 region. Any apparent association between SSTs and the QBO is likely by chance, but the SST anomalies that necessarily will be present for any specific QBO phase could contribute to the extratropical response. I recommend that the authors create a figure analogous to their figure 8 but for SST anomalies. Such a figure could then be used to interpret the precipitation anomalies in figure 8, 9, and 10 (and also the SLP anomalies in the North Pacific in figure 6 and 7 which the authors relate to tropical convection). Specifically, are these convection and SLP anomalies just driven by SST anomalies that are present during these specific QBO phases, or can the convection and SLP anomalies be truly linked to the QBO?

4. The authors include an ENSO index in their MLR with the presumed intention that teleconnections of ENSO can be isolated from that of the QBO. However, MLR cannot account for nonlinear influences of ENSO on the remote response to the QBO. Similarly, MLR cannot account for nonlinear influences of the solar cycle on the remote response to the QBO. Previous work has indicated that such nonlinearities exist (Garfinkel and Hartmann 2007, Wei et al 2007, Labitzke 1987, Labitzke 2005). I suggest that the authors perform their MLR separately for El Nino years and La Nina years; the connection between the QBO and the vortex should be mainly present during La Nina if Wei et al and Garfinkel and Hartmann 2007 are indeed correct. Similarly I suggest that the authors perform their MLR separately for solar max years and solar min years; the connection between the QBO and the vortex should be reversed between these two assuming the Labitzke effects are robust.

Relatively minor comments 1, The authors seem convinced that an EOF approach

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to characterizing the QBO is clearly better than a pressure level approach, but have failed to convince me. It is true that an EOF approach can characterize variability at different levels simultaneously better than a single level. However given the strong relationship between zonal wind anomalies at a given pressure level with any other pressure level (except in 2016, which was tough on EOF methods as well), this effect can be described by both EOF and pressure level methodologies. EOF methodologies have a downside: they are less intuitive and the specific choice on what phase angle corresponds to which wind profile is a subjective decision that can differ among studies. This can lead to confusion: p.7 line 23 states the relationship between the phases opposite to what is stated in the rest of the paper. As p.7 is the first time this relationship is stated, I wrote it down, only to become completely confused as I read the paper in my first pass. Eventually I figured out that p.7 line 23 was incorrect, but the fact that such a typo could occur in the first place illustrates the danger in using an EOF approach.

In my view, the main advantage of the EOF method is that it allows one to characterize (in principle at least) an infinite number of QBO phases. In the notation of the authors, one can create a QBO index for +60degrees, +30, 0, -20, -31.4159265359 degrees, etc. This allows for more flexibility in formulating the MLR. In contrast radiosonde data is available on some 7 pressure levels, and hence there are limitations on how once can test different phases. The authors don't mention this advantage, but I suggest they do so.

My intuition is that at the end of the day, it doesn't really matter which approach one follows, as is evidenced by their supplemental figure 5.

Technical comments p.2 line 8 There are earlier papers than that of Richter et al showing this (eg. Manzini et al 2006).

p.8 line 11 "hence our choice to show results at ****"

p 9 line 20 What is the linear correlation of the polar vortex index and the QBO? The correlation will certainly depend on the precise phase angle chosen, but as the authors

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find a similar Holton-Tan effect for a wide range of phases I suspect the correlation will be similarly insensitive.

p. 10 The sentences starting on lines 5 and 8 seem to contradict as currently written. Please clarify.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-1065>,

2017.

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