

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

This is a very interesting, and valuable, paper. While many previous papers have looked at the influence of the stratospheric quasi-biennial oscillation (QBO) on various aspects of atmospheric behavior, such as on stratospheric jet structure, surface weather, and tropical precipitation, this paper examines all of these in a holistic manner. Furthermore, it seeks to shed some light on the possible mechanisms whereby the QBO may exert its influence. It does this by using multivariate linear regression analysis of the QBO with the analyzed fields. It includes other influences in this regression analysis, such as stratospheric volcanic aerosol abundance, ENSO, solar activity, and a long-term trend. Other interesting aspects to their analysis is that, rather than examining time series of the influence of the QBO winds at individual levels, they utilize time series of the first two QBO empirical orthogonal functions (EOFs) between 70 and 10 hPa. Noting that EOF1 maximizes in the upper part of this height range, while EOF2 maximizes in the lower portion, they make conclusions about which height range of the QBO influences various atmospheric fields.

There are a number of different pathways by which the QBO may influence atmospheric behavior. The QBO variation in equatorial winds modulates the winter waveguide for extratropical planetary waves, which can affect the strength of the polar vortex and stratospheric polar temperatures. The authors call this the “polar route.” The QBO modulation of stratospheric equatorial winds is accompanied by meridional circulation, as is required by the thermal wind relationship. This, in turn, modulates equatorial stratospheric temperatures all the way down to the region below the tropical tropopause. This modulates tropical rainfall and tropical deep convection. This has been hypothesized in various observational analysis papers, and has also been shown in cloud-resolving modeling studies. The authors call this the “tropical route.” The QBO equatorial meridional circulation has equatorial upwelling that decreases tropical temperatures in easterly shear conditions, while the return circulation increases temperatures through downwelling in the subtropics. This affects the equatorial-subtropical temperature gradient, which in turn affects the wind shears in that region, which affects baroclinic waves and planetary waves. The authors call this the “subtropical route.”

As mentioned previously, many previous authors have looked at QBO influences on the atmospheric circulation, but this paper differs from these in various ways. One is in the length of the data record being analyzed. The zonal wind fields they analyze is the combined ERA-40 and ERA-Interim analyses that, together, extend from 1958-2016, while the precipitation is only the ERA-Interim analysis (1979-2016). The mean sea level pressure fields are from the Hadley Center for the period 1958 onward.

One interesting feature of their analysis is when they include both QBO and polar vortex indices in their regression analyses. This serves to separate effects

where the QBO influence is through the “polar route,” since QBO influences that remain after including the polar vortex index as a separate term in the regression analysis, would not likely involve the “polar route.”

One shortcoming of this paper is its identification of QBO influences on so many aspects of the atmospheric circulation, that it is difficult to recall all of them. I suggest a summary table that includes all of the identified variables they identify as being influenced by the QBO along with their conclusions about the height range of the QBO that they identify as being key and also which of the three routes they believe to be most likely responsible.

These are my global comments. In the following, I make my more specific comments.

Specific Comments

1. Page 1, Lines 27-29: Given the fact that both QBO and solar indices are used in the authors’ regression, I find it odd that Labitzke’s work is not mentioned here. The Holton Tan mechanism seems to be opposite, depending on the phase of the solar cycle. This is particularly so since several of the authors have written on this subject. Also, Labitzke et al. (2006) is in the reference list, but I can’t find it in the text.
2. Page 3, line 29: It seems to me that the reason why previous authors focus on the QBO at 40-50 hPa is that is where the amplitude of the QBO is maximum. That should probably be mentioned.
3. Page 4, line 32: A reference is needed for this statement.
4. Page 5, line 4: It’s interesting that 615 stations have provided data for more than 100 years. It doesn’t seem that data for all those years are used in this paper though.
5. Page 5, line 24: I think that the statement “that these indices are independent of one another” is too strong. Certainly, this is contradicted by statements in Garfinkel and Hartmann (2007), Salby (1996), and Taguchi (2010). It is sufficient to say that the inclusion/exclusion tests were done.
6. Page 6, lines 4-11: Both EOFs have opposite signs at different altitudes. This is contrary to the implication that only EOF1 has this character.
7. Page 6, lines 13-16: Mightn’t this misrepresentation of stalling affect correlations where the influence might occur via the QBO in the lowermost stratosphere (i. e., at or just above the tropopause).
8. Page 10, line 10: Throughout this paper, the authors are careful to distinguish between correlations and cause-effect mechanisms. This is an exception.
9. Page 11, line 13: This presupposes that only one of the possible mechanisms is in play.
10. Page 12, line 16: Doesn’t their figure 2 only show statistical significance in January?

11. Page 14, lines 21-24: It's good that this is mentioned here, but I don't think this mechanism gets enough mention in this paper. Given evidence for QBO influencing tropical precipitation, this likely affects Rossby wave trains that connect the tropics and extratropics. Indeed, Ho et al. (2009) present evidence for such a wave train influencing typhoon tracks. This should be mentioned on page 17, lines 15-19.
12. Page 16, line 16: Liess and Geller (2012) also examined weather states from ISCCP that characterize active and mature deep convection.
13. Again, this paper would benefit from a summery table of QBO effects, levels of QBO most highly correlated, and possible mechanisms.

Editorial Comments

1. Page 1, line 20: Insert "respectively" at end of line.
2. Page 2, line 14: "mechanism" -> "mechanisms" "is" -> "are"
3. Page 2, line 24: "may" -> "seems to"
4. Page 7, lines 27-31: This is a repetition of what is said near the top of this page.
5. Page 8, line 11: Incomplete statement in parentheses.
6. Page 10, line 15: The word "amended" seems odd. Perhaps altered?
7. Page 10, line 23: Only reduces it a bit.
8. Page 11, line 32: Figure 5 doesn't show results including the polar vortex term.
9. Page 12, line 17: Figure 76?
10. Page 16, lines 19-21: Still another repetition.