Review of *Explaining the period fluctuation of the quasi-biennial oscillation* by Young-Ha Kim

February 12, 2025

This paper offers a new explanation for the source of period fluctuations of the QBO. Such fluctuations are hypothesized to arise from seasonality in tropical convection, which launches small-scale gravity waves that are then filtered through the seasonally-evolving winds of the tropical tropopause layer to lead to a seasonally-varying gravity wave stress at the base of the QBO, which then contributes to a seasonally-varying descent rate. This contribution to the seasonally-varying descent rate is hypothesized to explain seasonality in the total descent rate, which in turn drives cycle-to-cycle variability of the QBO period. This cycle-to-cycle variability in the QBO period is noted to have implications for QBO understanding and predictability.

There are several steps to the argumentation in the paper, all of which must be true in order to justify the ultimate conclusions. Some of the steps are already convincing, but some would benefit from minor revisions in order to be convincing. Here are the key steps:

1. Variability in the QBO period is dominated by variability by in the descent rates of easterly shear zones (well-justified)

2. Variability in the descent rates of easterly shear zones is driven by variability in small-scale gravity wave stress propagating up through the tropopause (would benefit from minor revisions)

3. Seasonality in small-scale gravity wave stress propagating up through the tropopause results when waves that are launched by tropical convection are filtered through the seasonally-varying winds in the TTL (well-justified)

The paper should more forcefully disentangle the relative roles of seasonality in its newly-proposed mechanism (small-scale gravity wave stress) versus the commonly-accepted mechanism (residual upwelling). Towards that end, Step #2 must establish that variability in small-scale easterly gravity wave stress crossing the tropopause (F_{70}) is the primary driver of variability in the QBO period (e.g., as stated on Lines 254-255 of the Conclusions). The primary importance of F_{70} must be established counter to the commonly-accepted view that the primary driver is variability of residual upwelling (W). Numerous previous papers have put forward compelling evidence for the importance of W, including quantitative agreement between the annual cycle in W and the annual variations in the QBO descent rate (e.g., Coy et al., 2020).

The paper offers two broad pieces of evidence in favor of F70. The first broad argument is based on multi-linear regression in which F70 and W are used to predict descent rate, presented in Section 4.1, Eq. 3, and Fig. 3. This argument is not fully convincing because F70 and W appear to have very similar seasonal effects on QBO descent rate: comparing Fig. 1 from Coy et al. (2020) and Fig. 8c of the submitted paper makes it clear that both F70 and W contribute to minimum QBO descent rates in NH winter and August/September, and larger descent rates otherwise. Because F70 and W are strongly anti-correlated over the seasonal cycle, their effects are probably, at least in part, statistically degenerate. This plausible degeneracy calls into question the suitability and uncertainty of the multi-linear regression in Eq. 3 and plotted in Fig 3. If this correlational analysis is intended to be included, it is minimally necessary to do the following:

- plot the uncertainties in the regression coefficients in Fig. 3b, which hopefully take into account the effects of any collinearity. If the uncertainty bars are large and overlapping, this must change the interpretation of the results.
- plot the average seasonal cycles of F70 and W so that readers can visually distinguish how distinct they are.

Alternatives to this correlational analysis are possible. For example, I expected that the paper was going to take advantage of the descent rate formalism in Equation 2 to formulate a quantitative descent rate budget, i.e.,:

$$\frac{\partial \bar{u}}{\partial t}\left(\frac{\partial \bar{u}}{\partial z}\right)^{-1} = \frac{\nabla \cdot F}{\rho_0 a cos\phi}\left(\frac{\partial \bar{u}}{\partial z}\right)^{-1} - \bar{w}^* + \bar{v}^* \hat{f}\left(\frac{\partial \bar{u}}{\partial z}\right)^{-1} \tag{1}$$

Using such a budget, it would be possible to make quantitative arguments about the role of EPFD (potentially decomposed by wavenumber) and upwelling in driving descent rates. This argument would be stronger than the correlations, because the magnitudes of each term matter and not just their temporal structure. If it were found that the local EPFD at a given altitude is a dominant driver of the seasonality in local descent rates, then it would have to be subsequently established that the local EPFD at a given altitude scales with the incoming small-scale gravity wave stress crossing 70 hPa, per the assumed form of EPFD inspired by Lindzen and Holton (1968). This would form a stronger quantitative link in terms of temporal structure AND magnitude rather than the paper's current argument of Section 4.1 that is only in terms of temporal structure.

The magnitudes are considered in Section 4.2, which is good, although it shows wind tendencies, so to gain insight into descent rates, the reader must attempt to divide in their head by the hypothetical time-evolving vertical shear. Showing descent rates directly could be valuable. Also, is the EPFD plotted in Figs. 5/6/7 the total EPFD or just that from small-scale waves? Please ensure that all claims about small-scale waves are not based on any evidence from EPFD integrated across all wavenumbers as opposed to just the small scales.

Two last minor considerations:

- Line 142: "The spatial averaging applied to W aimed to reduce the effect of the QBO-induced local circulation on it (for details on the local circulation, see Plumb and Bell, 1982, Fig. 1)." Does "spatial averaging" refer to the averaging in the vertical (over 10-70 hPa) or the averaging in the horizontal (15S-15N)? In either case, the effects of the QBO-induced secondary circulation might not be particularly reduced. When averaging vertically, the secondary circulation can still project onto the domain-wide mean upwelling, because there is often only one strong shear zone in the domain (the other being stalled against the lower boundary). When averaging horizontally from 15S-15N, this only includes the tropical branch of the secondary circulation but not the extratropical branch, which is located poleward of roughly 15 degrees (e.g., Randel et al., 1999; Baldwin et al., 2001), so this will not reduce the effects of the meridional structure of the QBO-induced circulation.

- Lines 238-241 states: "Notably, while the above-mentioned flux variation (0.19-0.37 mPa) corresponds to ±32% of its annual mean, it leads to an even larger variation in the phase descent speed because upwelling reduces the descent speed on average (see Eq. (2)). This upwelling effect in amplifying the response of descent speed to the flux variations occurs regardless of the seasonal variability in upwelling." I am confused by this argument. Isn't descent rate linear in both upwelling and EPFD, per Equation 2? Perhaps the paper intended to refer to fractional variations in the phase descent speed?

References

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