Responses to Referee #1's Comments

The paper "Explaining the period fluctuation of the quasi-biennial oscillation" by Kim is an excellent work that addresses the long standing issue of explaining the variations in the length of the QBO period. Explaining variations of the QBO period are of great interest given the relevance of the QBO as one of the major modes of atmospheric interannual variability that has effects not only in the tropics, but also on the surface weather and climate in the extratropics. Several authors tried to explain variations of the QBO period by correlation with the solar cycle. Analysis of longer data sets, however, were less conclusive. The suggestion in this work, relating QBO period changes to variations in the forcing by small scale (gravity) waves, is therefore an interesting new explanation. The author presents a convincing chain of arguments for this mechanism.

The paper is very well written and the figures are adequate and of good quality.
The paper is therefore recommended for publication in ACP after minor revisions.
Further, the paper is recommended to be highlighted in ACP.

My main comment is that some more discussion should be added to further strengthen the paper and to provide a somewhat broader view.

Detailed comments are given below.

> >> I appreciate the reviewer's comments and assessment very much. The comments help improve the manuscript. My responses to each comment are given below.

[Minor comments]

(1) In the introduction:
It should be mentioned that several papers write about the QBO period depending on the 11-year solar cycle (e.g., Salby and Callaghan, J. Climate, 2000), while analyses using longer data sets are less conclusive (e.g., Fischer and Tung, JGR, 2008; Kren et al., ACP, 2014).
Variations of wave mean flow interaction as an alternative mechanism being responsible for QBO period variations is therefore quite convincing.

> >> Thank you for pointing out this aspect which I omitted in the original manuscript. This has been added in the revised manuscript [L48–51].

Do you think that wave momentum fluxes near the tropopause could be affected by the solar cycle and thereby contribute to QBO period variations related to the 11-year solar cycle?

> >> Addressing this question would require a very thorough analysis, as the activity of convective wave sources is influenced by numerous meteorological factors, with the 11-year solar cycle being a relatively minor one. Additionally, it remains uncertain whether observational or reanalysis data can capture solar-cycle related variations in wave momentum fluxes. Without such an analysis, I am unable to provide a definitive opinion on this matter.

Salby, M. and Callaghan, P.: Connection between the Solar Cycle and the QBO: The Missing Link, J. Climate, 13, 2652-2662, doi:10.1175/1520-0442(1999)012<2652:CBTSCA>2.0.CO;2, 2000.
Fischer, P. and Tung, K. K.: A reexamination of the QBO period modulation by the solar cycle, J. Geophys. Res., 113, D07114, doi:10.1029/2007JD008983, 2008.
Kren, A. C., Marsh, D. R., Smith, A. K., and Pilewskie, P.: Examining the stratospheric response to the solar cycle in a coupled WACCM simulation with an internally generated QBO, Atmos. Chem. Phys., 14, 4843–4856, https://doi.org/10.5194/acp-14-4843-2014, 2014.

(2) l.54, 55: Did you use ERA5 model level data, or pressure level data provided by ECMWF?

>> The model-level data are used for the QBO wind profiles in order to obtain the descent rates at as fine vertical resolution as possible. For all the other variables presented (wave momentum fluxes, momentum forcing terms, and upwelling velocities), the pressure-level data are used. This information has been added in the revised manuscript [L61–64].

(3) l.67-70:

Please explain in more detail how wave momentum fluxes and in particular phase speeds are determined. For phase speeds the wave frequency is needed.

In l.68 it is mentioned that 2D FFT was applied for this. Usually, this requires windowing in the time domain. How was this performed? How many days were combined to perform the 2D FFT?

>> The details regarding this have been added in the revised manuscript [L73–74].

(4) l.120: It could be mentioned that some observational evidence for critical level forcing as the main main mechanism for the gravity wave forcing of the QBO is seen in the gravity wave spectra shown in Ern et al., JGR, 2014.

>> Thank you for the advice. This has been added in the revised manuscript [L132–133].

(5) After l.212:

You should give some reasoning why the descent of the QBO westerly phase is much more continuous than the descent of the QBO easterly phase.

Do you think that this is a combination of:
(a) a weaker seasonality in the sources and low-altitude filtering of the small scale waves, and
(b) large scale Kelvin waves contribute significantly to the downward propagation of the QBO westerly phase (Ern and Preusse, GRL, 2009; Kim and Chun, ACP, 2015b). Possibly, large scale Kelvin waves may have a different seasonality than the small scale waves, and the dissipation mechanism of large scale Kelvin waves is mainly radiative damping, and not critical level filtering as for the small scale waves (Ern et al., ACP, 2009; Krismer and Giorgetta, JAS, 2014).

Ern, M. and Preusse, P.: Quantification of the contribution of equatorial Kelvin waves to the QBO wind reversal in the stratosphere, Geophys. Res. Lett., 36, L21801, https://doi.org/10.1029/2009GL040493, 2009.
Ern, M., Cho, H.-K., Preusse, P., and Eckermann, S. D.: Properties of the average distribution of equatorial Kelvin waves investigated with the GROGRAT ray tracer, Atmos. Chem. Phys., 9, 7973-7995, https://doi.org/10.5194/acp-9-7973-2009, 2009.

Krismer, T. R. and Giorgetta, M.: Wave forcing of the Quasi-Biennial Oscillation in the Max Planck Institute Earth System Model, J. Atmos. Sci., 71, 1985-2006, https://doi.org/10.1175/JAS-D-13-0310.1, 2014.

>> A new paragraph discussing this issue has been added in the revised manuscript [L242–252], along with modifications to an existing paragraph [L232–241], to provide insights on the westerly-phase descent. As commented in (a) by the reviewer, we found a weaker seasonality in the eastward momentum flux, compared to the westward momentum flux. Another reason for the continuous westerly-phase descent is overall weak local upwelling (w*) where the westerly phase descents, which therefore does not amplify the effect of the flux variation on the descent speed variation (unlike for the easterly-phase descent, as described in L238–241; 247–249). The difference in the dissipation mechanism between Kelvin and gravity waves, pointed out in (b) by the reviewer, has also been included in this paragraph.

(6) l.189/190: This is not entirely true:

It has been pointed out before by Ern et al. (2014) that during easterly shear gravity wave forcing in satellite observations and in estimates from reanalysis occurs in a series of bursts in accordance with the stepwise descent of the easterly QBO phase, while gravity wave forcing acts more continuously during westerly shear.

>> As this sentence refers to direct estimates of wave forcing from reanalyses or observations, not the indirect estimates (or 'missing drag' in the terminology of Ern et al. 2014), the most relevant result in the study of Ern et al. may be their Fig. 3c and 3d (SABER and HIRDLS estimates, respectively). I have revised the text [L211–213], citing their Fig. 3c to encourage readers to make a comparison.

(7) l.227-232: Some discussion about how realistic ERA5 resolved small scale waves are, and whether this matters, should be added:

It was shown by Preusse et. al. (2014) that convective gravity waves in the ECMWF model are not very realistic and could not be traced back to potential sources. Similarly, Okui et al. (2023) showed that in another gravity wave permitting model (JAGUAR) the agreement between model and AIRS observations in convectively dominated regions is poor.

Therefore it should be noted that for the mechanism steering the QBO period it is likely not required that the representation of convective gravity waves in ERA5 is overly realistic, as long as the spectrum of gravity waves contains the range of phase speeds interacting with the QBO. This is also confirmed by Fig.5d showing the zonal wind tendency with the contribution of advection subtracted.

Preusse, P., Ern, M., Bechtold, P., Eckermann, S. D., Kalisch, S., Trinh, Q. T., and Riese, M.: Characteristics of gravity waves resolved by ECMWF, Atmos. Chem. Phys., 14, 10483-10508, doi:10.5194/acp-14-10483-2014, 2014.
Okui, H., Wright, C. J., Hindley, N. P., Lear, E. J., & Sato, K. (2023). A comparison of stratospheric gravity waves in a high-resolution general circulation model with 3-D satellite observations. Journal of Geophysical Research: Atmospheres, 128, e2023JD038795. https://doi.org/10.1029/2023JD038795.

>> Following the reviewer's suggestion, the discussion on this issue has been added in the revised manuscript [L272–276].

(8) l.239, 240:

Please add data availability statements for ERA-Interim and MERRA2 that are used in Appendix B. Further, a statement for JRA-3Q should be added if JRA-3Q results will be provided in the revised manuscript.

>> They have been added in *Data availability* in the revised manuscript.

[Technical comments]

l.132: in independece variables -> in additional independent variables

>> This sentence has been removed.

Responses to Referee #2's Comments

The paper *Explaining the period fluctuation of the quasi-biennial oscillation* is an interesting work investigating the connection between variations in the length of the QBO period and variation of small-scale waves.

The paper is well-written and presents a compelling argument that variations in small-scale waves are the dominate drivers of the variation in QBO descent rates. After minor revisions towards justifying choices made in the analysis, the paper is recommended for publication in ACP.

>> I appreciate the reviewer's comments very much, which help improve the manuscript. My responses to each comment are given below.

[Specific comments]

- Hampson and Haynes 2004 posits the SAO to be a possible of the period fluctuation of the QBO (as well as upwelling and wave forcing). Can this analysis also investigate this possibility?

>> In the perspective of momentum forcing, we demonstrate that wave forcing is the primary factor for the observed period fluctuation, especially at high altitudes (Fig. 6). Therefore, the downward effect of the SAO would be reflected through the zonal-wind shear to which the wave forcing is proportional. However, temporal variations in the shear due to the SAO at 5–10 hPa (the uppermost layer of the QBO) are not in phase with the wave forcing. For instance, the SAO shear at this layer reaches its negative maximum in February whereas the maximum westward wave forcing occurs in April–May (Fig. 6e). Similarly, the second peak of the negative SAO shear (August) does not coincide with that of the wave forcing (October–November). Therefore, we think the downward effect of the SAO on the period fluctuation may be minor, although a more thorough investigation is needed to draw a definitive conclusion.

- Figure 2: To further emphasize the argument, it would be nice to see the W-descent period plotted here as as well, even if varies little.

>> Following the reviewer's suggestion, it has been added into Fig. 2, with modifications to the text in the revised manuscript [L112–123] to discuss this additional result.

- Why use monthly rather than weekly means? In (page 5, line 107) the limitations of lower frequency sampling is noted.

>> Ultimately, we relate the descent speed to the 70-hPa wave flux, using monthly data. If this analysis were conducted on a weekly basis, it would be necessary to account for the time lag between the two time series, as it can take up to a few days for waves to travel from 70 hPa to ~10 hPa. However, the travel time varies largely, depending on wave scales (e.g., horizontal wavelength) and mean-flow states along the propagation paths, which would complicate the regression analysis. By using monthly means, we can simply disregard time lags of up to a few days.

- 4.1, page 6, 120-133: Please justify choice of F70 (rather than say, F50). Do these show similar annual evolution? Similarly, comment on column averaged w* rather than w* at several levels. In particular, I would naively expect that height at which w* is measured/regressed would matter for descent rates.

>> In order to explain the dependent variable using independent variables via a regression analysis, the independent variables should not contain variability arising from the dependent variable (otherwise, the causality may not be clear between the variables). In our regression, this means that the wave flux and upwelling velocity should not contain QBO-related variations so that the descent rate (i.e., QBO phase speed) is explained by the two independent quantities. The 70-hPa altitude is chosen as the highest level with a little impact of the QBO on the wave flux (and also it is conventionally the bottom of the tropical stratosphere, as required in Eq. (2); refer to the definition of $F(c)$). In the same reason, w* has been averaged to reduce the effect of the QBO-induced local circulation on it. This is also in line with the argument of the previous studies (introduced in Section 1) that the seasonal cycle of the "stratospheric upwelling" that is driven by extratropical planetary-wave forcing could modulate the QBO descent speed.

Yes, a similar annual evolution is maintained in F50 (see Fig. R1). Regarding the use of local w*, interestingly, it did not yield an overall better regression score compared to W (see Fig. R2; cf. Fig. 3b). The text has been revised to better clarify the rationale of using F70 and W [L144–146].

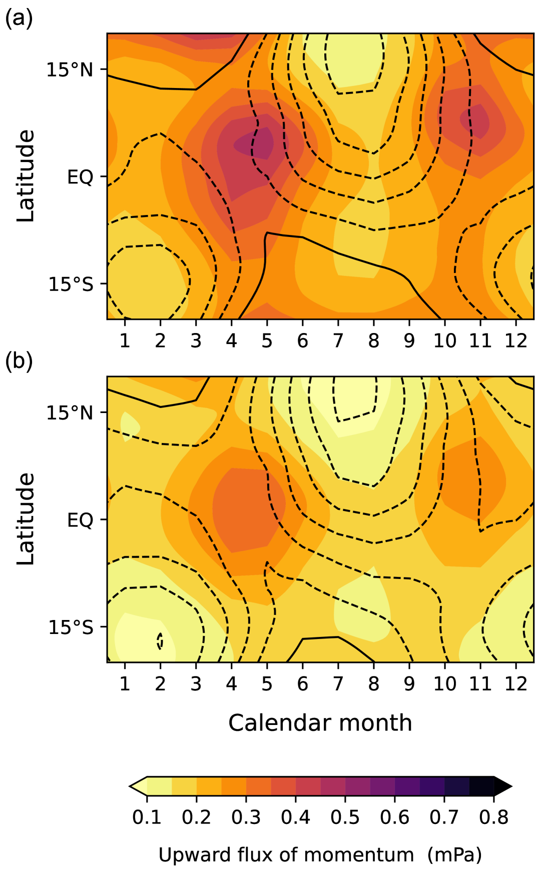

Fig. R1. As in Fig. 8 except at (a) 70 hPa and (b) 50 hPa, with a different color scale.

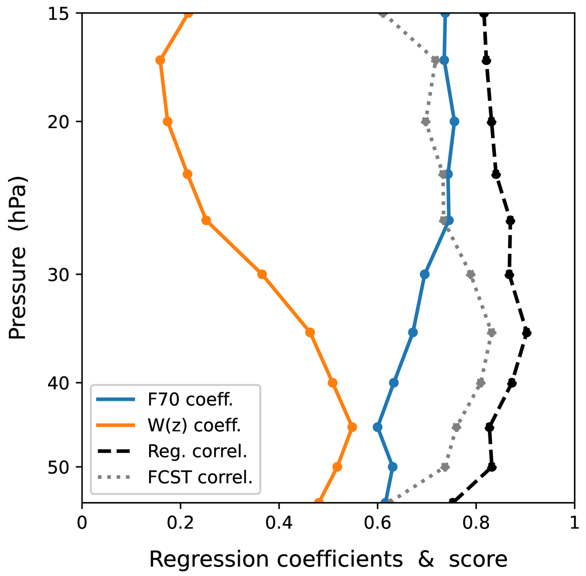

Fig. R2. As in Fig. 3b except using w* at each altitude.

- page 6, 138-139: Please clarify the phrase "the selected time series were standardized."

>> This has been clarified in the revised manuscript [L153].

- Figure 5 (and similar): Could the y-axes be adjusted to be consistent throughout the figure? I think this would aid in comparison between panels 5d and 5e greatly.

>> The y-axes for the tendency variables share the same scale interval except the one in panel (e) (which uses the 50% smaller scale interval). This has been clarified in the figure caption in the revised manuscript: "*Note that the y-axes in (b)–(d) use the same scale interval, and the scale in (e) is half that.*" I believe this is the best way to compare the magnitudes across the variables without wasting spaces.

- Figure 5 (and similar): Can comment on why the wave forcing estimates (panels d and e) appear to be "out of phase" with one another in the first half of the year? If the variations in wave-forcing are primarily driven by the seasonal cycle, I would expect this not to be the case. (Something like ENSO perhaps?).

>> I could not observe an out-of-phase relationship between the two estimates (panels d and e). Instead, my response assumes that the reviewer referred to the substantial spread among cycles within each group in a given calendar month.

Wave forcing depends not only on variations in wave flux but also significantly on the wind and its vertical shear associated with the QBO phase, given the critical-level filtering process (refer to Section 4.1, first paragraph). Even with similar wave fluxes at the tropopause, small differences in the detailed sheared-wind structures due to the QBO can lead to substantial differences in wave forcing. Thus, the notable spread within each group (thin lines of the same color) for a given calendar month does not undermine the importance of the seasonal cycle in wave flux variability for QBO phase evolutions. On the contrary, we emphasize its critical role, as shown in the prediction using monthly climatologies (Fig. 3b, grey dotted lines). Even so, variability beyond the seasonal cycle (such as ENSO) may also partly contribute to these variations (as mentioned in L259–260 in the revised manuscript), and this warrants further investigation in future studies.

- Appendix A: I don't understand why we get to separate the purple and orange lines, as they seem to "start" at the same place.

>> It is because the wind shear is in different states between the two groups when they begin to diverge (e.g., around March, Fig. 5a), although the wind itself is similar at that timing. The wave forcing depends on both the wind and shear, as described in Section 4.1 (the first paragraph). The larger shear in the purple group (due to faster phase evolutions at higher altitudes, although not identifiable in Fig. 5) results in larger wave forcing around March (Fig. 5e), leading to earlier phase changes at the target altitude, compared to the orange group. This explanation has been included in Appendix A in the revised manuscript [L296–300].