Response to Reviewer 1

Suggestion: Major revisions

1 Overview

Here, I review the second submitted version of "The Impact of Increasing Stratospheric Radiative Damping on the QBO Period" by Zhou et al (2020). As in the first submitted manuscript, this paper proposes a new radiative-dynamical mechanism that could have implications for the future properties of the QBO. This paper proposes that increasing CO_2 is expected to increase the radiative damping rate in the stratosphere. Using an idealized 1D model of the QBO, this increase in the radiative damping rate would be expected to modestly shorten the period of the QBO.

I will refer to the first reviewed manuscript as V1 and the most recently submitted manuscript as V2. V2 is similar in scientific content and presentation to V1, with the exception of specific revisions made in light of reviewer comments. In my previous review of V1, I identified two items that required major revision: (1) the tension between interpretability and predictability and (2) the physical justification for using the Plass (1956) result to estimate future radiative damping. V2 has satisfactorily resolved the tension between interpretability and predictability, as will be elaborated upon further. Neither V2 nor the specific response to my previous review has satisfactorily resolved my concerns about the physical justification for using the Plass (1956) result, as will be elaborated upon further, and which leads me to recommend Major Revisions.

We are grateful for your thoughtful and thorough review, and regretful for the inadequacy of our previous revision. Hopefully, this version of manuscript has adequately addressed your concerns. Note that all line numbers refer to those in the tracked version.

2 Major Revisions from V1

2.1 Resolved in V2:

In reviewing V1, a tension between predictability and interpretability of the main results was identified. This tension resulted from the addition of realistic but minor predictive elements to the key simulations in the paper, which made it more difficult to isolate the QBO period changes owing to the proposed mechanism versus to the other minor elements (e.g., changes in buoyancy frequency). This tension has been largely resolved in V2, which focuses on the interpretability of the results and considers minor predictive elements only in the Discussion. It is clear in V2 that the minor predictive elements in V1 were not leading to significant changes in the basic QBO behavior (the N^2 changes alone appear to only change the QBO period from 30 months to 30.2 months on line 348). I am satisfied with this resolution of the tension between predictability and interpretability in favor of emphasizing interpretability.

We are glad that this segment of our revision is satisfactory.

2.2 Requires major revision in V2:

In reviewing V2, I commented that the usage of a 50% change in radiative damping rate in response to increasing CO_2 required further justification. It was unclear whether the Plass (1956) result was providing the appropriate justification. After reviewing the response to my review of V1 and reading V2, I remain concerned about the justification for the radiative damping changes used in the paper. I will elaborate on my concerns below so as to be clear about the source of my confusion.

If the radiative heating rate represents a linear damping with rate α [s^{-1}] of temperature T [K] relative to the local radiative equilibrium temperature T_E [K], then the radiative heating rate Q [K s^{-1}] obeys the Newtonian cooling equation:

$$Q = \alpha(T_E - T) \quad (1)$$

The accepted method for diagnosing radiative damping rate is to consider two states S1 and S2 where all atmospheric properties are held fixed except that temperatures are prescribed to vary between S1 and S2 and the radiative heating rate responds to the temperature profile. The radiative heating rates are calculated using a radiative transfer model. If the radiative heating rates satisfy the assumption of linearity, therefore obeying equation 1, then states S1 and S2 separately obey the following equations:

$$Q_1 = \alpha(T_E - T_1)$$

$$Q_2 = \alpha(T_E - T_2)$$

The above equation system has two equations in two unknowns (unknowns are α and T_E), so it is possible to solve for the unknowns. Solving for α yields:

$$\alpha = \frac{Q_2 - Q_1}{T_2 - T_1} \quad (2)$$

Note that the above Equation 2 is equivalent to Dickinson's Equation 1. Note that it is only possible to solve for α if α and T_E are assumed to be constant between states S1 and S2. The assumption of constant _ and TE in S1 and S2 is reasonable because α and T_E are thought of as functions of the atmospheric composition, which was held fixed.

Unlike in this typical approach, the experiments in Plass (1956) consider the radiative heating rate [K s^{-1}] in an atmosphere with temperature held fixed but composition varied. When atmospheric composition is varied, it is not realistic to assume that α and T_E remain constant. Two states from the Plass (1956) approach, denoted $S1'$ and $S2'$, obey the following equations:

$$Q'_1 = \alpha'_1(T'_{E,1} - T')$$

$$Q'_2 = \alpha'_2(T'_{E,2} - T')$$

The above two equations have four unknowns (α'_i and $T'_{E,i}$ for I =1, 2). Therefore, α'_i and $T'_{E,i}$ are underdetermined. Adding experiments calculated with new concentrations of CO_2 would not make the equation set determined, because each additional equation adds two unknowns α'_i and $T'_{E,i}$. Therefore, it is not possible to solve for α'_i and $T'_{E,i}$, nor is it possible to solve for α'_1/α'_2.

If it is assumed that there is constant T'_E in the Plass (1956) experiments, the equation set now has three unknowns, and it is possible to solve for α'_1/α'_2 as follows:

$$\frac{\alpha'_1}{\alpha'_2} = \frac{Q'_1}{Q'_2}$$

V2 appears to use the above equality between the ratio of heating rates and ratio of radiative damping rates to justify the use of Plass (1956) to project the magnitude of future change in radiative damping rate. However, the above equality depends on the assumption that T_E is constant as CO_2 varies, an assumption which does not appear justifiable given that increasing CO_2 is expected to decrease T_E in the stratosphere (Manabe et al., 1967). Without assuming constant T_E, it appears that the changes in radiative heating rate cannot be used to constrain the changes in α.

Knowing that T_E changes in response to increasing CO2, one could alternatively assume that α stays constant with increasing CO2 while T_E changes, which would result in the following relationship:

$$\frac{T'_{E,1} - T'}{T'_{E,2} - T'} = \frac{Q'_1}{Q'_2}$$

In reality, there are probably changes in both T_E and α as CO2 concentrations are changed. These changes must be distinguished, and the component stemming from a change in α isolated, in order to obtain an order of magnitude estimate for the period change resulting from this mechanism. The 50% change in radiative heating rates from Plass (1956) in response to changes in CO2 does not necessitate that the changes in radiative damping rate are O(50%), as they could be much smaller (or larger).

In light of these concerns, the substance of my recommendation for major revisions from my previous review still stands: The usage of the Plass value of 50% [or 30% in V2] should be either (1) justified in light of these considerations or (2) an alternative reliable estimate should be provided of the radiative damping rate response to CO2 doubling (an order of magnitude estimate is fine). If no projection of radiative damping rate with CO2 doubling exists in the literature, then one should be produced (e.g., using a radiative transfer model). Such a projection of radiative damping rate, necessary for the arguments in the paper, would constitute a valuable contribution in its own right.

We thank you for your thoughtful and thorough explanations. Accordingly, we used a sophisticated radiative transfer model to evaluate how the Newtonian cooling coefficients change in response to the doubling of CO2 concentration, which is described in lines 208-239 of the latest version of the manuscript.

3 Minor comments

- It is stated that the semi-annual oscillation profile has zero curvature, so there is no diffusion of that profile (Line 263). However, there is a kink in u_{SA} at 28 km at the intersection of the constant (zero) value below and the linear profile above. The curvature at the kink is large and undefined, so the statement $\frac{\partial^2 \bar{u}_{SA}}{\partial z^2} = 0$ on Line 263 is not accurate. I would intuitively expect diffusion across this kink. The omission of the diffusion acting on $\bar{u}_{SA}$ from equation (5) therefore needs to be justified.

We thank you for your rigorous insight. Subsequently, we have removed Eq. (5) and revised that section.

- In light of the considerations in the previous item, but also more broadly, I remain unsure about what is gained by the decomposition of $\bar{u} = \bar{u}_{QBO} + \bar{u}_{SA}$. Given that the wave driving acts on both $\bar{u}_{QBO}$ and $\bar{u}_{SA}$ and (as argued above) the diffusion acts on both $\bar{u}_{QBO}$ and $\bar{u}_{SA}$, it is unclear whether $\bar{u}_{QBO}$ can be fruitfully isolated and treated in a separate analytical framework.

  As mentioned above, we have removed that unfruitful segment.

- Line 296 states that "unphysical behavior" arises when the period does not change smoothly as a function of the magnitude of G. It is not clear to me why this behavior is unphysical. On the one hand, it is conceivable for G to be weak enough that it would not impact the period of the QBO, and therefore the period of the QBO would match the G = 0 state. It is also conceivable that there is phase-locking between the semi-annual oscillation and the QBO, such that if G is varied over a small enough set of values, then the SAO might maintain the same phase relationship with the QBO, and therefore not exhibit a period response. (In a totally different context, such phase-locking across a range of parameter values of the QBO was reported in response to upwelling variations in Rajendran et al. (2016).) In light of these plausible explanations, I recommend that either some insight be gained regarding the phase locking, or the language “unphysical behavior" be softened, e.g., to “unexpected behavior".

We have changed “unphysical behavior" into “unexpected behavior" as suggested.

- Line 367 provides an estimate for the shortening of the QBO period stemming from the doubling of CO_2 of 7.4% ± 2.5%. The error estimate of 2.5% was estimated by propagating through the 30% error estimate from Plass (1956) on the radiative damping rate. This error statement is therefore a statement on the estimated error in QBO period change owing to errors in the radiative damping rate. As presented, the error estimate appears to be a more comprehensive statement concerning error in the 1D results. However, V2 presented substantial systemic uncertainty, another source of error. It was shown that the period change was 15% when using the Plumb model, which is a plausible formulation of the problem and far outside the error margin on Line 367. I recommend that the narrow scope of the error margin on Line 367 be stated, by qualifying that they refer only to expected errors from uncertainty in radiative damping rate. The much larger error owing to the formulation of the QBO model should be noted in any statement regarding uncertainty in the magnitude of the period change. (Note: The error estimate will ultimately need to be consistent with the resolution of my major comment regarding the estimate of the radiative damping rate.)

  We have added “and taking into account the uncertainty due to the radiative damping rate” in line 442.

References

Manabe, S. and Wetherald, R. T.: Thermal equilibrium of the atmosphere with a given distribution of relative humidity, J. Atmos. Sci., 24, 241–259, https://doi.org/10.1175/1520-0469(1967)024%3C0241:TEOTAW%3E2.0.CO;2, 1967.

Plass, G. N.: The influence of the 15µ carbon-dioxide band on the atmospheric infra-red cooling rate, Quart. J. Roy. Meteor. Soc., 82, 310–324, https://doi.org/10.1002/qj.49708235307, 1956.

Rajendran, K., Moroz, I. M., Read, P. L., and Osprey, S. M.: Synchronisation of the equatorial QBO by the annual cycle in tropical upwelling in a warming climate, Q. J. Roy. Meteor. Soc., 142, 1111–1120, https://doi.org/10.1002/qj.2714, 2016.

Response to Reviewer 2

This is the review of the revised manuscript "The Impact of Increasing Stratospheric Radiative Damping on the QBO Period"

The results are interesting and relevant to Atmospheric Chemistry and Physics. However, I do not understand their explanations about ozone effect on the QBO period, so I recommend a minor revision.

We appreciate you for your helpful comments and will address this specific issue below. Note that all line numbers refer to those in the tracked version.

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\>> 2. Scale height.
I understand that the sensitivity of scale height is smaller than that of the radiative dumping.

We thank you for your approval.

\>> 3. Ozone
\>ozone does affect the QBO period,
\> According to the latest Scientific Assessment of Ozone Depletion completed in 2018, ozone will heal completely before the CO2 concentration is doubled.

We completely agree with you.

I think without ozone depletion by ODS, ozone at above 20 hPa does affect the QBO because in this region ozone is dominated by temperature-dependent photochemistry.

We have added one paragraph (lines 405-415) and slightly modified line 435 to address this issue.

SD2005 indicated that the QBO with non-interactive (interactive) ozone reproduces a 27-month (31-month) period, which results in increase by 15%. This amount is much larger amount than the QBO shortening by 7.4% due to enhanced radiative damping of your finding.

It is an interesting and puzzling research topic which is beyond the scope of this paper due to the following two reasons.

(1) SD2005, i.e., Shibata and Deushi (2005) reported that "Two control runs are designed to simulate the QBO with realistic period of the oscillation: the one with non-interactive ozone reproducing a 27-month period and the other with interactive ozone yielding a 31-month period. Two experiment runs are made by switching on and off the ozone radiative feedback for the non-interactive and the interactive control runs, respectively. The QBO period is prolonged from 27 to 48 months in the switched-on run, while it is shortened from 31 to 20 months in the switched off run, demonstrating that the interactive ozone does prolong the QBO period."

In Shibata and Deushi (2005), they showed that the QBO periods simulated with interactive ozone are longer than those simulated with non-interactive ozone. However, their short report in Geophys. Res. Lett. did not mean to be comprehensive or exhaustive. For example, we don't know whether the climatologies of interactively generated ozone in Shibata and Deushi (2005) are close to those of the specified ozone in their non-interactive runs. Bushell et al. (2010) illustrated that the differences between the two ozone climatologies gave rise to differences in tropical upwelling between 100 and

4 hPa which led to a 12-month difference between the simulated mean QBO periods with the different ozone climatologies. We believe that many issues need to be explored to further our understand the discrepancies of the QBO periods between the non-interactive and interactive ozone experiments, which is beyond the scope of this paper.

(2) Using a mechanistic model with a one-dimensional representation for mean flow and a three-dimensional depiction for Kelvin and Rossby-gravity waves, Cordero et al. (1998) demonstrated that ozone feedbacks lengthened the QBO period by about 2 months. Cordero and Nathan (2000) further developed a more sophisticated mechanistic model with a two-dimensional representation for mean flow and a three-dimensional depiction for Kelvin and Rossby-gravity waves, and surprisingly found that ozone feedbacks had little influence on the QBO period. We believe that more studies should be conducted to reconcile those discrepancies between the results from those two mechanistic models.

Also, the stratospheric cooling indirectly increases ozone because it reduces the ozone loss rate in the upper stratosphere, owing to the strong positive temperature dependence of the Chapman reactions and the NOx cycle on the ozone loss rate. So, I think that you should evaluate the influence of ozone on the QBO period, compared to the QBO shortening due to radiative damping.

As mentioned above, we have added one paragraph (lines 405-415) to address this issue. However, some other interesting issues are beyond the scope of this paper and warrant further research.

Minor comments
L410-431. Redundant descriptions. You can omit most of them.

This part was added to satisfy Reviewer 1 and Petr Šácha (who made a short comment during the Discussion stage). It is hard to make everyone happy.

References

Bushell, A. C., Jackson, D. R., Butchart, N., Hardiman, S. C., Hinton, T. J., Osprey, S. M., and Gray, L. J.: Sensitivity of GCM tropical middle atmosphere variability and climate to ozone and parameterized gravity wave changes, J. Geophys. Res., 115, D15101, https://doi.org/10.1029/2009JD013340, 2010.

Cordero, E. C., and Nathan, T. R.: The influence of wave- and zonal-mean ozone feedbacks on the quasi-biennial oscillation, J. Atmos. Sci., 57, 3426–3442, https://doi.org/10.1175/1520-0469(2000)057%3c3426:TIOWAZ%3e2.0.CO;2, 2000.

Cordero, E. C., Nathan, T. R. and Echols, R. S.: An analytical study of ozone feedbacks on Kelvin and Rossby–gravity waves: Effects on the QBO, *J. Atmos. Sci.*, 55, 1051–1062, https://doi.org/10.1175/1520-0469(1998)055<1051:AASOOF>2.0.CO;2, 1998.

Shibata, K., and Deushi, M.: Radiative effect of ozone on the quasi-biennial oscillation in the equatorial stratosphere, Geophys. Res. Lett., 32, L24802, https://doi.org/10.1029/2005GL023433, 2005.