Response to Reviewer 1

1 Overview

Suggestion: Major revisions

This manuscript examines the impact of increasing stratospheric radiative damping on the period of the QBO. The scale of the increase in stratospheric radiative damping is based on results from a radiative transfer model in Plass (1956). Radiative damping causes dissipation of vertically propagating waves, which can lead to mean flow accelerations and internal oscillations, namely the Quasi-Biennial Oscillation (QBO) of the tropical stratosphere. This paper investigates the sensitivity of the QBO to increased radiative damping rate in a classical one-dimensional model of the QBO. It is reported that increased radiative damping would decrease the height scale of wave dissipation, and would be expected to lead to modest decreases in the QBO period (by 5-15% depending on the model formulation). Comprehensive climate models do not produce robust projections of the future QBO period, disagreeing on the sign of any future change. This disagreement is primarily thought to arise from competition between increasing wave stress (which tends to reduce the period) and increasing upwelling (which tends to increase the period). The mechanism proposed in this paper is an additional process that could potentially impact the QBO period in the future, and could already be happening in reality and in comprehensive climate simulations of the QBO.

The identification and characterization of a new process that could lead to changes in the QBO period is a worthwhile endeavor, and is appropriate for publication in this journal. One-dimensional models of the QBO are appropriate tools for characterizing the existence, sign, and order of magnitude of this radiative-dynamical sensitivity. This paper is careful to show how the results are sensitive to the formulation of the model, and those sensitivities help contextualize the argument. This paper has good potential, although at present the approach requires more justification, and the presentation could benefit from easing some of the tension between competing objectives of interpretability and predictive value. First, the radiative damping projections cited in the paper are of questionable relevance to the work presented. Second, the focus in the manuscript on producing a deterministic prediction of future period change appears to be inconsistent with the uncertainty stemming from the formulation of the model. If the radiative damping can be grounded on a more reliable basis, and the emphasis in the paper can be shifted to focus on the interpretation of the hypothesized mechanism and its attendant uncertainties without asking too much of its predictive value, then the paper can be recommended for publication. As such, major revisions are recommended.

We thank you for your insightful comments and suggestions and will address them point by point as below.

2 Major revisions

To elaborate on the recommendations for major revision:

First, the manuscript relies on a projection of radiative damping from Plass (1956), who diagnosed radiative cooling rates with a fixed temperature profile in response to a doubling of CO_2. However, the connection between the Plass analysis and the radiative damping rate is not obvious. Radiative cooling rate has units of [K s^{-1}], whereas radiative damping rate has units of [s^{-1}]. The cooling rate results of Plass cannot be used to isolate a change in radiative damping rate because the Plass result also includes changes in radiative equilibrium temperature, both of which impact the radiative cooling.

In his last section (i.e., section 4), Plass (1956) mentioned "The change in the equilibrium temperature at the surface of the earth with CO_2 concentration…" in order to counter "The argument has sometimes been advanced that the CO_2 cannot cause a temperature change at the surface of the earth because the CO_2 band is always black at any reasonable concentration…"

When Plass (1956) calculated the radiative cooling rates in other sections, he didn't deal with radiative equilibrium temperature at all. Instead, he obtained them for $\frac{1}{2} \times \mathrm{CO_2}$, $1 \times \mathrm{CO_2}$, and $2 \times \mathrm{CO_2}$ with a fixed temperature profile.

Let's quote Dickinson (1973): "Thus we resorted to an entirely numerical approach for obtaining a Newtonian cooling coefficient $a_0(z)$ for small departures from the reference temperature profile $T_0(z)$. That is, if $Q(T)$ is the infrared cooling rate for a temperature profile T(z), then

$$a_0(z) = \frac{\boldsymbol{Q(T_0+\delta)-Q(T_0-\delta)}}{\boldsymbol{2\delta}}$$

where δ is a small temperature perturbation (we used $\delta = 0.1^oK$)."

Assuming $Q(T)$ for $1\times \mathrm{CO_2}$ or $2 \times \mathrm{CO_2}$ is a smooth function, we can infer that if $Q(T)$ for $2 \times \mathrm{CO_2}$ at some altitude level is approximately 50% larger than that for $1 \times \mathrm{CO_2}$ at the same altitude level, then $\mathrm{a}_0(z)$ for the former is also approximately 50% larger than that for the latter at that altitude level. Note that the shape of the profile $\mathrm{Q}(z)$ in Fig. 1 depicted by Dickinson (1973) is very similar to that of the profile $a_0(z)$ in his Fig. 3. In other words, the value of $a_0(z)$ is approximately proportional to that of $\mathrm{Q}(z)$.

In addition, we don't need to know how radiative equilibrium temperatures change in response to increasing CO_2 concentration when we study how the wave-mean flow interactions generate the QBO, because the temperature fields associated with atmospheric waves relax back to the zonal mean temperatures rather than radiative equilibrium temperatures.

Finally, Dickinson (1973) implied that below the 0.2 hPa level the value of an estimated Newtonian cooling coefficient $a(z)$ is not sensitive to how a temperature profile $T(z)$ is chosen (refer to his Eqs. (2) and (3)).

The usage of the Plass value of 50% 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.

The maximum value we used is **30%** rather than the Plass value of 50% (refer to lines 215-219 in the revised manuscript). Plass (1956) claimed "The probable error of the cooling rate is estimated by introducing arbitrary variations into the original transmission functions and calculating their influence on the final result. The probable error obtained in this manner is about 10 per cent below 20 km, increasing to 30 per cent at 50 km and becoming rather uncertain above 60 km. Again, the relative differences between the various curves should be considerably more accurate than their magnitude."

Even if we regard the probable error of the cooling rate is 30 percent, the relative differences between the various cooling rates calculated by Plass (1956) should be considerably smaller 30 percent. Since the relative differences in cooling rates calculated by Plass (1956) are around 50% above 35 km, our value of 30% is smaller than the lower bound of uncertainty, i.e., 50% - 50%*30% = 35%. In other words, our choice of 30% is a conservative estimate.

Second, there is tension in the manuscript between the interpretability and predictive value of the results. Using the 1D model makes a strong decision in favor of interpretability, which is well justified by the approach and the results. Noting that the QBO period in the basic state and the response to changes in radiative damping are both highly sensitive to minor changes in the model formulation, it appears that the 1D model can provide, at best, the sign and order of magnitude of the period change in response to an increase in radiative damping. Therefore, modeling decisions that sacrifice the interpretability of the final results without impacting the sign or order of magnitude of the final result should be justified or avoided. Two example decisions in the paper are as follows. For each, the sacrifice of interpretability should be (1) justified in terms of predictive value or some other objective or (2) the simpler case should be considered:

• The Holton (1972) formulation used in this paper is driven by asymmetric waves (a Kelvin wave and a Rossby wave with different dispersion relations and wavenumbers). The Plumb (1977) formulation is driven by symmetric wave stress (equal and opposite gravity waves), allowing for clear interpretation of the model dynamics in terms of a small number of dimensionless parameters. Is there a benefit to using asymmetric wave forcing in this paper that justifies it at the cost of sacrificing the interpretability of the symmetric formulation?

Plumb (1977) provided a simpler and elegant theoretical framework to illuminate the essence of the QBO. What is more, Plumb (1977) paved the way for the experimental tour de force (Plumb and McEwan 1978) guided by "a small number of dimensionless parameters". Plumb and McEwan (1978) demonstrated how a standing internal wave with sufficiently large amplitudes forced at the lower boundary of an annulus of salt-stratified water generated an oscillatory mean flow with relatively long periods compared to the period of the internal wave. This incarnation of the QBO analog cleared up any lingering doubts about the theory of wave-mean flow interactions.

However, the Plumb (1977) formulation is best suited to study non-rotating systems such as those in laboratories rather than the planetary-scale rotating Earth system. Although some authors used it to illustrate the stratospheric QBO by introducing a Kelvin wave and an "anti-Kelvin wave", there is no "anti-Kelvin wave" in the terrestrial atmosphere. The Holton (1972) formulation was based on the observations that planetary-scale waves in the Equatorial lower stratosphere are dominated by Kelvin waves of zonal wavenumber 1-2 and mixed Rossby-gravity waves of wavenumber 4 (Andrews et al. 1987).

• Another source of the tension is in the choice to include changes in the buoyancy frequency N in the projections of the model response to radiative damping changes. Including the small (2.5%) changes in N seems to be so marginal that its effects are primarily to sacrifice interpretability without a clear benefit. It can still be useful to include a sensitivity study to changes in N, but this sensitivity study should be distinguished from the main line of argumentation in the paper. Note that in the 1D model, the buoyancy frequency N and radiative damping μ are always multiplied together, such that their combined effects are tantamount to considering a ($1.5 * 1.025 \rightarrow$) ≈54% change in radiative damping (or buoyancy frequency) alone, not significantly different than the 50% change in radiative damping.

Following your suggestion by excluding the small changes in N, we have redone a lot of experiments. Subsequently, figures 3, 5, and 6 have been re-plotted and the manuscript has been revised accordingly.

3 Minor comments

It would be appreciated if the following minor comments regarding the content and structure of the paper were addressed:

• The decomposition of $\overline{\mathrm{u}} = \overline{\mathrm{u}}_{\mathrm{sa}} + \overline{\mathrm{u}}_{\mathrm{QBO}}$ in equation (5) gives the impression that the evolution of $\overline{\mathrm{u}}_{\mathrm{QBO}}$ depends only on $\overline{\mathrm{u}}_{\mathrm{QBO}}$, and that the influence of $\overline{\mathrm{u}}_{\mathrm{sa}}$ has been factored out. Yet $\overline{\mathrm{u}}_{\mathrm{sa}}$ still impacts the evolution of $\overline{\mathrm{u}}_{\mathrm{QBO}}$ through the wave forcing $\overline{\mathrm{F}}_{\mathrm{i}}$. Because $\overline{\mathrm{F}}_{\mathrm{i}}$ is nonlinear in the zonal wind profile, the SAO will impact the wave forcing, which changes the mean wind and then alters the diffusion profile. So, the dynamics are not simply resulting from the sum of a linear QBO dynamic and a linear SAO dynamic. Given the limitations of this decomposition, what benefit is provided by its inclusion?

Yes, we agree with your reasoning and expected that the simulated QBO periods should be sensitive to G, the imposed semiannual forcing. However, the simulated QBO periods are not sensitive to the imposed semiannual forcing provided that G does not exceed the values employed by HL (refer to lines 314-316 of the version with track changes).

We used "the decomposition of $\overline{u} = \overline{u}_{sa} + \overline{u}_{QBO}$ in equation (5)" to highlight this bizarre behavior and further illustrated that the deficiency is closely related to the insufficient height of the model top.

• Lines 301-312 list all numerical values from Figure 6. Is it necessary to list all numerical values when the figure provides their approximate value? If so, then perhaps a table can be supplied instead of the figure or in the supplementary information. I suspect that the relevant information from Figure 6 can be conveyed in a more concise way.

We have eliminated this unpleasant verbosity.

• In the Introduction, the following question is raised: "Does the competing effect between [upwelling and enhanced wave stress] leave some room for increasing stratospheric radiative damping to exert an influence on the QBO period?" (Line 137) In the Conclusions, comprehensive QBO models are noted to have significant variance in their projections of period. A quantitative comparison would be useful here; allowing that the 1D model provides at best an order of magnitude estimate, is there reason to believe that period changes in GCMs are small enough that radiative damping could potentially impact their sign? Their magnitude? Or on the contrary, are the period changes in GCMs large enough in magnitude that radiative damping would not be expected to have a significant bearing on the sign or magnitude of the change?

We are not in position to answer those important questions. This report aims to provoke the readers to think and/or conduct more researches to answer them.

• Lines 343 - 345: Recently, doubt has been cast on the role of upwelling on QBO amplitude (Match and Fueglistaler, 2019, 2020). A more nuanced assessment would be appreciated than "The amplitude decrease is associated with a strengthened residual mean circulation, also consistent with the literature, although the vertical structure of the circulation response is nontrivial."

Done.

4 Edits

(166) “mixing Rossby-gravity wave” should be “mixed Rossby-gravity wave”

Done.

References

Andrews, D. G., Holton, J. R., and Leovy, C. B.: Middle Atmosphere Dynamics, Academic Press, 489 pp, 1987.

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Plumb, R. A.: The interaction of two internal waves with the mean flow: Implications for the theory of the quasi-biennial oscillation, J. Atmos. Sci., 34, 1847–1858, https://doi.org/10.1175/1520-0469(1977)034<1847:TIOTIW>2.0.CO;2, 1977.

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