

# The Disappearing Quasi-Biennial Oscillation under Sustained Global Warming

Corresponding Author: Professor Fei Xie

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Nature Communications.

**This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.**

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This is my third time reviewing this paper. (The previous two times were for a different journal which regrettably decided to reject the paper.) Both previous times I asked for major revisions and requested numerous additional analyses which the authors have since performed. I think the paper is much stronger than the original version, and at this point I suggest only minor revisions for clarity.

1. There is a new paper that just came out of relevance to this paper: Garfinkel et al 2025. This paper focuses on the historical record and finds a weakening of the QBO and more disruptions under GHG forcings, while aerosols strengthen the QBO. They don't find any evidence for robust changes in period unlike the present paper, however the climate forcings in their paper are much weaker than the end of century SSP585. I suggest citing near e.g., line 56

line 34: "shift the tropospheric westerly jet" I don't think strong evidence is provided for this. The EOF pattern does shift, but that doesn't mean the jet actually shifts. It just means that the dominant mode of variability becomes the polar vortex (as I wrote in previous reviews). I suggest deleting these words

line 68-69: "experience a modest increase" I don't see any increase in the corresponding figure. Please clarify (or delete)

line 142-143: vertical advection inhibits downward propagation for both QBO phases. It is just that for wQBO the anomalous w associated with the QBO weakens the BDC, so the inhibition is less pronounced than for eQBO.

line 257-260: please rewrite for clarity

Garfinkel, C. I., Avisar, D., Osprey, S., & Smith, D. (2025). The response of the QBO to external forcings: Implications for disruption events. *Journal of Geophysical Research: Atmospheres*, 130(22), e2025JD044438.

Reviewer #2

(Remarks to the Author)

I have read the second revision of the manuscript, together with replies to three reviewers. The authors addressed my questions from the previous review. They included new results from a 1D model, which adds another dimension to the paper. They have shown that under severe GH gas loading the QBO is likely to have a reduced amplitude and reduced period and less of an influence on the STJs. The exploration of possible terraced future QBO regimes is interesting. I recommend publication with minor revision. I have some comments and suggestions below.

1. l27: "In recent years, the amplitude of the lower-stratospheric QBO has weakened" - Is this known for sure? Perhaps

mention in text and cite source if you this it is. (It is unusual to state something such as this in an abstract without mention in the text.)

2. I71-72: For completeness, suggest stating that the CESM2-WACCM did not show a significant reduction in amplitude, with values all within the range 4-7 m/s in panel b).
3. Figure 1: For panels 1c-f, if you deleted white space between and the y-axis label and expanded each window, readers could more easily see the data. Figures 3 and 4 would benefit from this treatment as well.
4. Discussion beginning I160: This exploration of very large CO<sub>2</sub> loadings is interesting. It invites consideration of what else in the climate system would also change. In such a large CO<sub>2</sub> loading scenario, what other changes do you think might occur in the earth system that could be larger than the effect that you found and possibly cause different results for the QBO?
5. Fig. 2: What causes the large growth in easterly GWD at 50 hPa after 2050 in WACCM (Fig. 2e)? Is this due to filtering by stronger mean westerlies at 100-70 hPa? Fig. 2f - please explain colors in caption. Why are R1, R2, R3 chosen to be slightly different from M1, M2, and M3?
6. Fig. 4 Please include descriptions of shading and purple contours.
7. Discussion following I272: The result in Fig. RA4 (panels a and i) shows that the QBO amplifies from 1 to 2x CO<sub>2</sub>, which may be in 2050. Perhaps this is worthy of mentioning? It would be in the opposite direction of the trend that you mentioned in the abstract.
8. Equation 8: It is highly likely that the changes in the wave forcing which led to changes in the QBO would also change the properties of the SAO. It is also likely that the annual cycle would not be fixed in an increasing loading scenario. Perhaps these aspects should be mentioned. This is in the same spirit as comment 4 above. If you discuss such future scenarios, then it seems reasonable to try to imagine other aspects of strong changes that would occur. I think it's fine to show the 1D model results as they are – it is too hard to parameterize changes in the SAO and annual cycle. But perhaps mention whatever caveats you think should be pointed out along these lines.

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We sincerely appreciate the editor and reviewers' constructive comments and valuable suggestions. Below, the reviewers' comments are included in black text, and our responses are presented in blue.

**Reviewer #1 (Remarks to the Author):**

This is my third time reviewing this paper. (The previous two times were for a different journal which regrettably decided to reject the paper.) Both previous times I asked for major revisions and requested numerous additional analyses which the authors have since performed. I think the paper is much stronger than the original version, and at this point I suggest only minor revisions for clarity.

**Response:** We appreciate the reviewer's careful reading of our manuscript and the constructive comments provided. These suggestions have helped us to improve the manuscript. Below, we respond to each point in turn.

1. There is a new paper that just came out of relevance to this paper: Garfinkel et al 2025. This paper focuses on the historical record and finds a weakening of the QBO and more disruptions under GHG forcings, while aerosols strengthen the QBO. They don't find any evidence for robust changes in period unlike the present paper, however the climate forcings in their paper are much weaker than the end of century SSP585. I suggest citing near e.g., line 56

**Response:** Thanks for the reviewer's suggestion. We have added a citation to Garfinkel et al. (2025) at the suggested location.

Reference:

Garfinkel, C. I., Avisar, D., Osprey, S., & Smith, D. (2025). The response of the QBO to external forcings: Implications for disruption events. *Journal of Geophysical Research: Atmospheres*, 130(22), e2025JD044438.

line 34: "shift the tropospheric westerly jet" I don't think strong evidence is provided for this. The EOF pattern does shift, but that doesn't mean the jet actually shifts. It just means that the dominant mode of variability becomes the polar vortex (as I wrote in previous reviews). I suggest deleting these words

**Response:** Thanks for the reviewer's suggestion. We have deleted this wording as recommended.

line 68-69: "experience a modest increase" I don't see any increase in the corresponding figure. Please clarify (or delete)

**Response:** Thanks for the reviewer's suggestion. We have deleted this statement accordingly.

line 142-143: vertical advection inhibits downward propagation for both QBO phases. It is just that for wQBO the anomalous  $w$  associated with the QBO weakens the BDC, so the inhibition is less pronounced than for eQBO.

**Response:** Thanks for the reviewer's comment. Considering the vertical advection term,  $-w \partial u / \partial z$ , it contributes a westerly acceleration in both shear zones. In the westerly shear region,  $w < 0$  and  $\partial u / \partial z > 0$ , so  $-w \partial u / \partial z > 0$ , which enhances westerlies and facilitates their downward propagation. In the easterly shear region,  $w > 0$  and  $\partial u / \partial z < 0$ , again yielding  $-w \partial u / \partial z > 0$ ; this westerly tendency opposes the development and descent of easterlies, thereby reducing the descent rate of the easterly shear zone relative to the westerly shear zone.

This interpretation is consistent with Reviewer#1's Comment#1 from the previous review round and with the description in Baldwin et al. (2001), who noted that "downward advection of momentum associated with the westerly shear zone enhances the descent of the westerlies, while the upward advection of momentum associated with easterly shear inhibits the descent of easterlies." We therefore retain our original wording.

Reference:

[1] Baldwin M P, Gray L J, Dunkerton T J, et al. The quasi-biennial oscillation[J]. Reviews of Geophysics, 2001, 39(2): 179–229.

line 257-260: please rewrite for clarity

**Response:** Thanks for the reviewer's suggestion. We have rewritten this passage for clarity. The revised text now reads:

**"The first EOF mode of CESM2-WACCM simulations under the extended SSP5-8.5 scenario during the first 50 years, when the QBO is present (Fig. 4i), closely resembles the corresponding sensitivity experiment (Fig. 4g). During the last 50 years, when the QBO disappears (Fig. 4j), it similarly resembles the corresponding sensitivity experiment (Fig. 4h)." (Page 11, lines 240–245 in the revised manuscript.)**

## Reviewer #2 (Remarks to the Author):

I have read the second revision of the manuscript, together with replies to three reviewers. The authors addressed my questions from the previous review. They included new results from a 1D model, which adds another dimension to the paper. They have shown that under severe GH gas loading the QBO is likely to have a reduced amplitude and reduced period and less of an influence on the STJs. The exploration of possible terraced future QBO regimes is interesting. I recommend publication with minor revision. I have some comments and suggestions below.

**Response:** We sincerely thank the reviewer for the time and effort devoted to evaluating our manuscript. The reviewer's comments and suggestions have been very helpful in improving the quality and clarity of the manuscript. Below, we provide a point-by-point response to each comment.

1. 127: "In recent years, the amplitude of the lower-stratospheric QBO has weakened" - Is this known for sure? Perhaps mention in text and cite source if you think it is. (It is unusual to state something such as this in an abstract without mention in the text.)

**Response:** Thanks for the reviewer's comment, and we apologize for the inaccurate wording in the previous version. We have corrected the words accordingly. The study by Kawatani et al. (2013) reports a weakening of the lower-stratospheric QBO amplitude over the past several decades. The sentence has now been revised to read:

**"Over recent decades, the amplitude of the lower-stratospheric QBO has weakened, and in recent years the QBO has experienced unprecedented disruptions."** (Page 2, lines 26–28 in the revised manuscript.)

In addition, detailed discussion and relevant references are provided in the Introduction section (Page 3, lines 47–51 in the revised manuscript.).

Reference:

[1] Kawatani Y, Hamilton K. Weakened stratospheric quasi biennial oscillation driven by increased tropical mean upwelling[J]. Nature, 2013, 497(7450): 478–481.

2. 171-72: For completeness, suggest stating that the CESM2-WACCM did not show a significant reduction in amplitude, with values all within the range 4–7 m/s in panel b).

**Response:** Thanks for the reviewer's suggestion. Although the amplitude change in CESM2-WACCM is indeed weaker compared with other models, we have calculated its linear trend, which shows a statistically significant decrease at the 99% confidence level. The sentence has now been revised to read:

**"Notably, CESM2-WACCM shows a comparatively modest reduction in amplitude relative to the other models, with values remaining within 4–7 m s<sup>-1</sup>."** (Page 4, lines 73–75 in the revised manuscript.)

3. Figure 1: For panels 1c-f, if you deleted white space between and the y-axis label and expanded each window, readers could more easily see the data. Figures 3 and 4

would benefit from this treatment as well.

**Response:** Thanks for the reviewer's suggestion. We have revised Fig. 1 by removing the white space between the panels and the y-axis labels and by expanding each panel to improve the visibility of the data. The same adjustments have also been applied to Figs. 3 and 4. Please refer to the Figs. 1, 3 and 4.

4. Discussion beginning 1160: This exploration of very large CO<sub>2</sub> loadings is interesting. It invites consideration of what else in the climate system would also change. In such a large CO<sub>2</sub> loading scenario, what other changes do you think might occur in the earth system that could be larger than the effect that you found and possibly cause different results for the QBO?

**Response:** We thank the reviewer for the comment. We acknowledge that under a large CO<sub>2</sub> loading scenario, many components of the Earth system may change, including, as noted by the reviewer in Comment #8, the annual cycle and the semiannual oscillation. As the reviewer pointed out, it is challenging to parameterize changes in the SAO and the annual cycle. We therefore followed the reviewer's suggestion and have clarified this limitation in the Methods section:

**“Here, we assume fixed periods of 360 days for the annual cycle and 180 days for the semiannual cycle, and the results from the one-dimensional QBO model should therefore be interpreted within this framework.”** (Page 16, lines 370–372 in the revised manuscript.)

In addition, the disappearance of the QBO under high CO<sub>2</sub> forcing is supported by both the CMIP6 simulations and our sensitivity experiments. In the sensitivity experiments, the imposed CO<sub>2</sub> increases were designed to follow the CO<sub>2</sub> trajectory under the SSP5-8.5 scenario. When CO<sub>2</sub> reaches four times its baseline concentration (approximately corresponding to SSP5-8.5 around 2100), the QBO period is already substantially weakened (Supplementary Fig. 4c,d). When CO<sub>2</sub> reaches six and eight times its baseline concentration, roughly corresponding to SSP5-8.5 around 2150 and 2200, respectively, the QBO disappears entirely (Supplementary Fig. 4e–h). The mechanisms analysed in the manuscript are consistently supported by the CMIP6 models, the sensitivity experiments, and the one-dimensional QBO model. We therefore conclude that the mechanisms discussed in the manuscript play an important role in the disappearance of the QBO.

We note that, as the reviewer suggested, additional changes under high CO<sub>2</sub> loading (for example, changes in the annual cycle and the semiannual oscillation) may also influence the QBO and could contribute to its disappearance. However, given the scope and length constraints of the present manuscript, and the complexity of quantifying these effects, we do not pursue a detailed discussion here.

5. Fig. 2: What causes the large growth in easterly GWD at 50 hPa after 2050 in WACCM (Fig. 2e)? Is this due to filtering by stronger mean westerlies at 100–70 hPa?

**Response:** Thanks for the reviewer's comment. We have added this discussion to the

revised manuscript:

**“The enhanced easterly gravity-wave forcing at 50 hPa after 2050, together with the lack of a significant change in westerly forcing (Fig. 2e), may be consistent with stronger filtering by the intensified westerlies in the 100–70 hPa layer (Supplementary Fig. 2).”** (Page 7, lines 151–154 in the revised manuscript.)

Fig. 2f - please explain colors in caption.

**Response:** Thanks for the reviewer’s comment. We have revised the figure caption as follows:

**“The prescribed upwelling values are grouped into three sets: orange circles indicate  $w_c = 0.3 \text{ mm s}^{-1}$ , and  $w_a = 0.2 \text{ mm s}^{-1}$  and  $w_h = 0.1 \text{ mm s}^{-1}$ ; blue squares indicate  $w_c = 0.4 \text{ mm s}^{-1}$ , and  $w_a = 0.3 \text{ mm s}^{-1}$  and  $w_h = 0.2 \text{ mm s}^{-1}$ ; and red triangles indicate  $w_c = 0.5 \text{ mm s}^{-1}$ , and  $w_a = 0.4 \text{ mm s}^{-1}$  and  $w_h = 0.3 \text{ mm s}^{-1}$ .”** (Caption for Fig. 2 in the revised manuscript.)

Why are R1, R2, R3 chosen to be slightly different from M1, M2, and M3?

**Response:** We thank the reviewer for the comment. The key regions were identified based on an EOF-based reconstruction of the QBO-induced tropospheric zonal-wind pattern. The R regions were derived from the ERA5 reanalysis and are used to assess zonal-wind changes across multiple models, whereas the M regions were defined using CESM1-WACCM only. To better present the sensitivity-experiment results, the M regions differ slightly from the R regions, although their core locations are consistent. We have clarified this in the revised manuscript.

**“We then applied the same reconstruction approach to the QBO\_T sensitivity experiment (Supplementary Fig. 22b) to identify the corresponding key regions in the sensitivity experiment where the QBO affects zonal wind.”** (Page 21, lines 489–492 in the revised manuscript.)

6. Fig. 4 Please include descriptions of shading and purple contours.

**Response:** Thanks for the reviewer’s comment. We have added the following description to the figure caption:

**“In g–j, shading denotes the first EOF mode, and purple contours indicate the climatological zonal-mean zonal wind over the corresponding periods. The contour interval is  $5 \text{ m s}^{-1}$ , with the outermost contour at  $10 \text{ m s}^{-1}$ .”** (Caption for Fig. 4 in the revised manuscript.)

7. Discussion following l272: The result in Fig. RA4 (panels a and i) shows that the QBO amplifies from 1 to 2x CO<sub>2</sub>, which may be in 2050. Perhaps this is worthy of mentioning? It would be in the opposite direction of the trend that you mentioned in the abstract.

**Response:** Thanks for the reviewer’s suggestion. We acknowledge that our previous wording may indeed have led to a misunderstanding by readers. We have added this

discussion to the revised manuscript:

**“The carbon dioxide (CO<sub>2</sub>) increasing sensitivity experiments (see Methods) show that the QBO begins to disappear when CO<sub>2</sub> concentration reaches 6 or 8 times the pre-industrial level (Supplementary Fig. 4a–h). Supplementary Fig. 4i–j further show that the QBO also disappears when atmospheric CO<sub>2</sub> is held at the pre-industrial level, while sea-surface temperatures are warmed to those induced under six times CO<sub>2</sub>. Together, these results suggest that QBO disappearance under global warming is not driven by a direct CO<sub>2</sub> effect on the QBO, but instead is associated with a cascade of responses to CO<sub>2</sub>-induced sea-surface warming.”** (Page 5, lines 106–113 in the revised manuscript.)

8. Equation 8: It is highly likely that the changes in the wave forcing which led to changes in the QBO would also change the properties of the SAO. It is also likely that the annual cycle would not be fixed in an increasing loading scenario. Perhaps these aspects should be mentioned. This is in the same spirit as comment 4 above. If you discuss such future scenarios, then it seems reasonable to try to imagine other aspects of strong changes that would occur. I think it’s fine to show the 1D model results as they are – it is too hard to parameterize changes in the SAO and annual cycle. But perhaps mention whatever caveats you think should be pointed out along these lines.

**Response:** We thank the reviewer for this helpful suggestion. We have therefore added a clarification in the Methods section. Specifically, we now state:

**“Here, we assume fixed periods of 360 days for the annual cycle and 180 days for the semiannual cycle, and the results from the one-dimensional QBO model should therefore be interpreted within this framework.”** (Page 16, lines 370–372 in the revised manuscript.)