Responses to Reviewer 1

This is my second review of Wang et al. One of my initial comments was that I found the stippling on the plots that are intended to indicate statistical significance confusing. On most figures, regions with no discernable anomaly are still stippled, while the strongest anomalies are often not stippled at all. One possible explanation is that there is a bug somewhere, however the authors are confident there is no such bug. I am not convinced. For example, Figure 2 indicates that the tropical TCO anomalies are not significant, while the much weaker anomalies in midlatitudes are. Similarly, Figure 9 implies that the wind anomalies in the tropics are not significant, however this is a QBO composite so how is that even possible?

I must admit that I cannot recommend acceptance of this paper with the figures in their current state, as they fail to pass a basic sanity check. If the authors are still confident in their results, I suggest that the editor ask for an additional reviewer.

I did not re-read section 3.4 as once I saw figure 9 I stopped reading the paper, however my major comments on earlier sections were addressed reasonably, and the ACP copy editor should be able to fix the word usage issues.
We thank the reviewer for the very helpful comments. Yes, the reviewer is correct. We finally find the bug in the codes for testing the significance of the results. We are really sorry for the mistake. We have corrected all the figures and revised the corresponding descriptions in the whole manuscript. More details of the revision can be found in the revised manuscript as well as the point-to-point response as follows. The comments are shown in black and our replies are marked in blue.

Minor comments:
Line 82 implies the TCO data is from 1984-2020 while line 77 implies 1979-2020. Please clarify
We apologize for the confusing description. We used three types of data, including TCO data (with dimensions of longitude and latitude), zonal mean data with different vertical levels (with dimensions of latitude and altitude) and the 3-D data (with dimensions of longitude, latitude and altitude) in the analysis. The TCO data is available from 1979 to 2020, while the zonal mean data with different vertical levels is available from 1984 to 2020 and the 3-D data is only available for the period 2002-2020. We have added more details of the data in the revised manuscript to make it clear.

Line 133 Please clarify the units for the PC1>0.5 and PC1<-0.5 (I assume standard deviations is the unit, but this should be stated)
Yes, the unit is the standard deviation. We have added it in the revised manuscript.

Figure 4 are the units ppmv or ppm by mass?
The unit is ppmv. We have updated it in the revised manuscript.

Responses to Reviewer 2

General comments
I understand that the authors made many efforts to improve the manuscript. Thank you for considering my comments and suggestions. However, I have still found a lot of errors, inconsistencies, typos, and insufficient explaining of logic in the revised manuscript; thus, I have several questions and comments (see the minor comments). I do not think the current version of the manuscript is acceptable as it is; the manuscript still seems to need corrections and refinements.
We thank the reviewer for the further comments. We have revised the manuscript carefully based on the comments and suggestions of the reviewer and hope that the manuscript has been improved significantly. More details of the revision can be found in the revised manuscript as well as the point-to-point response as follows. The comments are shown in black and our replies are marked in blue.

Major comments
First, I apologize to the authors for making comments and questions on points I did not mention in the original version. I have checked the manuscript more carefully in this round of review, and I have further comments and questions.
We thank the reviewer for the further comments which are very helpful to improve the manuscript.

According to the description in the Method section (2.4), QBOW is a phase with westerly winds around 10 hPa. Is it correct? If so, for me, and possibly some others, who are used to the definition of QBO at 50 hPa, the relationship between the QBO phase and TCO anomaly sounds reversed and may be confusing. Would you add some descriptions explaining this? For example, "…during QBOW phases (easterly around 50 hPa), as compared with QBOE (westerly around 50 hPa).
The QBO index used in this study is the first principal component of the Empirical Orthogonal Function (EOF) analysis on the equatorial zonal wind in the stratosphere (70-10 hPa), which is synchronized with the 20 hPa equatorial zonal wind with a correlation coefficient of 0.99. We thank the reviewer for the good suggestion and have added some descriptions correspondingly in the revised manuscript.

The authors' analyses were performed by evaluating the difference between the composites of QBOW and QBOE (QBOW–QBOE), and they gave descriptions like "Zonally asymmetric features are seen in the polar regions…". I think they should state more clearly "zonally asymmetric features of the difference".
We thank the reviewer for the good suggestion. We have updated the descriptions to "zonally asymmetric features of the QBO signals (QBOW-QBOE)" or "zonally asymmetric QBO signals (QBOW-QBOE)" to make it more clearly.

Making the difference between QBOW and QBOE is a standard practice to examine QBO effects, but my concern is that in Figs. 9, 10, 11, 12, and 13b, the contours are

climatological means, while the colors are the difference between QBOW and QBOE. If the figures depicted the contours of QBOE means, I could easily understand how the QBOW changed the fields from QBOE; or if they used colors to show the difference between QBOW and the climatology along with the climatology contours, it would also be easy to understand the physical meaning. Do the authors assume that the climatology is near the mid-point between QBOW and QBOE?
Thanks for the good comment. We have changed the contours from climatological mean to QBOE means in the corresponding figures.

In association with this, I am confused with the results shown in Fig. 12, although this figure seems to be interesting. The authors state that the QBO related wavenumber-1 anomalies (QBOW–QBOE) are generally out of phase with the climatological pattern. Therefore, in the QBOW phase, the zonal asymmetry is smaller and it is expected that wave activity is less than that in the QBOE. Is this correct? My understanding of E-P flux anomaly associated with the QBO phases is that more wave flux goes into the high latitude stratosphere when the tropical zonal wind around 50 hPa is easterly, thus polar vortex is weaker, and vice versa (less wave flux goes into the high latitude stratosphere when the tropical zonal wind around 50 hPa is westerly, thus polar vortex is stronger.) This relationship is the Holton and Tan relationship and several other works also showed this. I am wondering if the anomalies in Fig. 12 is consistent with the results from the preceding studies based on the zonal-mean zonal wind and E-P flux. Would you represent Fig. 12 by making difference between QBOW and climatology (QBOW–climatology) and between QBOE and climatology (QBOE–climatology), and depict the differences from by colors? Then you can check whether the anomaly distribution is "in-phase" or "out of phase" compared to the climatological mean geopotential height distribution for each QBO phase. I think that "out of phase" reduces the zonal asymmetry and corresponds to less E-P flux, then zonal winds in the tropics and 50 hPa should be westerly (QBOE in the author's definition?).
We thank the reviewer for the comments. Figures R1 and R2 show the anomalies in the QBOW and QBOE phases separately as the reviewer suggested. As shown in Figure R1, the anomaly is out of phase with the climatology from eastern North America to western Europe and from eastern Eurasia to the North Pacific (about 60% of the focused area), but in phase with the climatology in other regions. Therefore, maybe it is inaccurate to say that the QBO-related wavenumber-1 anomalies (QBOW–climatology) are generally out of phase with the climatological pattern, since the phase shift of the anomaly compared to the climatology is more evident. For QBOE, the anomalies are opposite in sign with the QBOW (Figure R2) and therefore have more areas (about 60%) in phase with the climatology. We agree with the reviewer that there are less wave activities in the regions where the anomaly is out of phase with the climatology. However, this does not mean our results are inconsistent with the Holton-Tan mechanism. The QBO index used in this study (with a correlation of 0.99 to the U at 20 hPa) is not simply the opposite of the zonal winds at 50 hPa in the tropics (with a correlation of -0.18). Figure R3 shows the geopotential height anomalies during the QBOE using the 50 hPa U index, which is significantly different from the results using

the QBO index in this study during QBOW (Figure R1a). The geopotential height is anomalously high (indicating a weaker polar vortex) during QBOE (at 50 hPa) due to the Holton-Tan mechanism, with neglectable zonal asymmetry features. The anomalies with different wave numbers (Figures R2b-d) can not fully explain the overall anomalies (Figure R3a). Note that as introduced in the Method section, the sample size of QBOW and QBOE is nearly equal to each other in our QBO index, while the QBOW size is usually much larger than the QBOE size using the 50 hPa QBO index (Fig. 1). This may be one possible reason of the different zonal asymmetric features using different QBO index, however, the exact reason awaits further studies is out of the scope of this study.

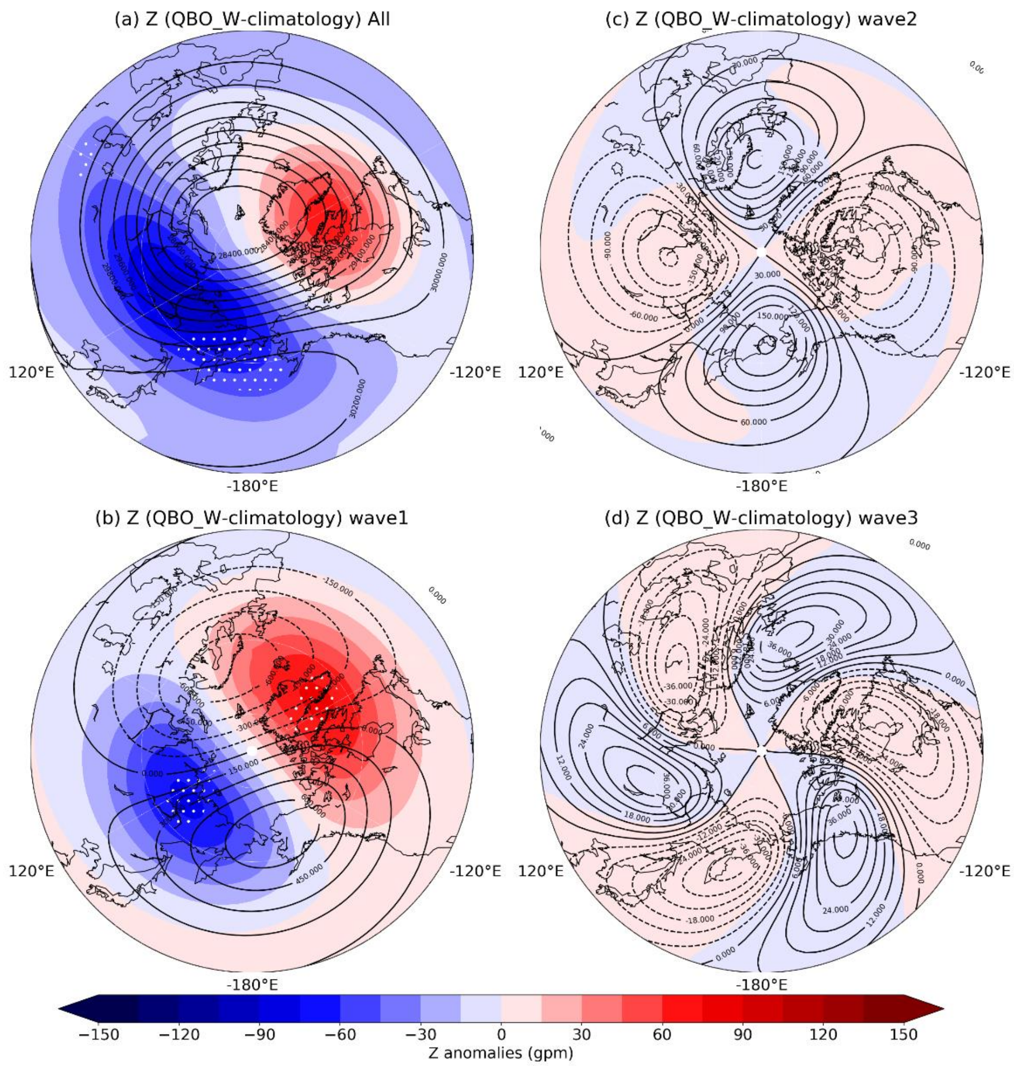

Figure R1. (a) Influences of QBOW (QBOW-climatology) on geopotential height (Z at 10 hPa) in the northern hemisphere winter (DJF) based on ERA5 data for the period 1979-2020. **(b-d)** The corresponding changes of geopotential height associated with QBO in wave numbers 1-3. The climatological values of geopotential height in winter as well as the climatological patterns of wave numbers 1-3 are also shown (contourlines). Stippled areas indicate results that are statistically significant over the

95% level, using the two-tailed Student's t-test.

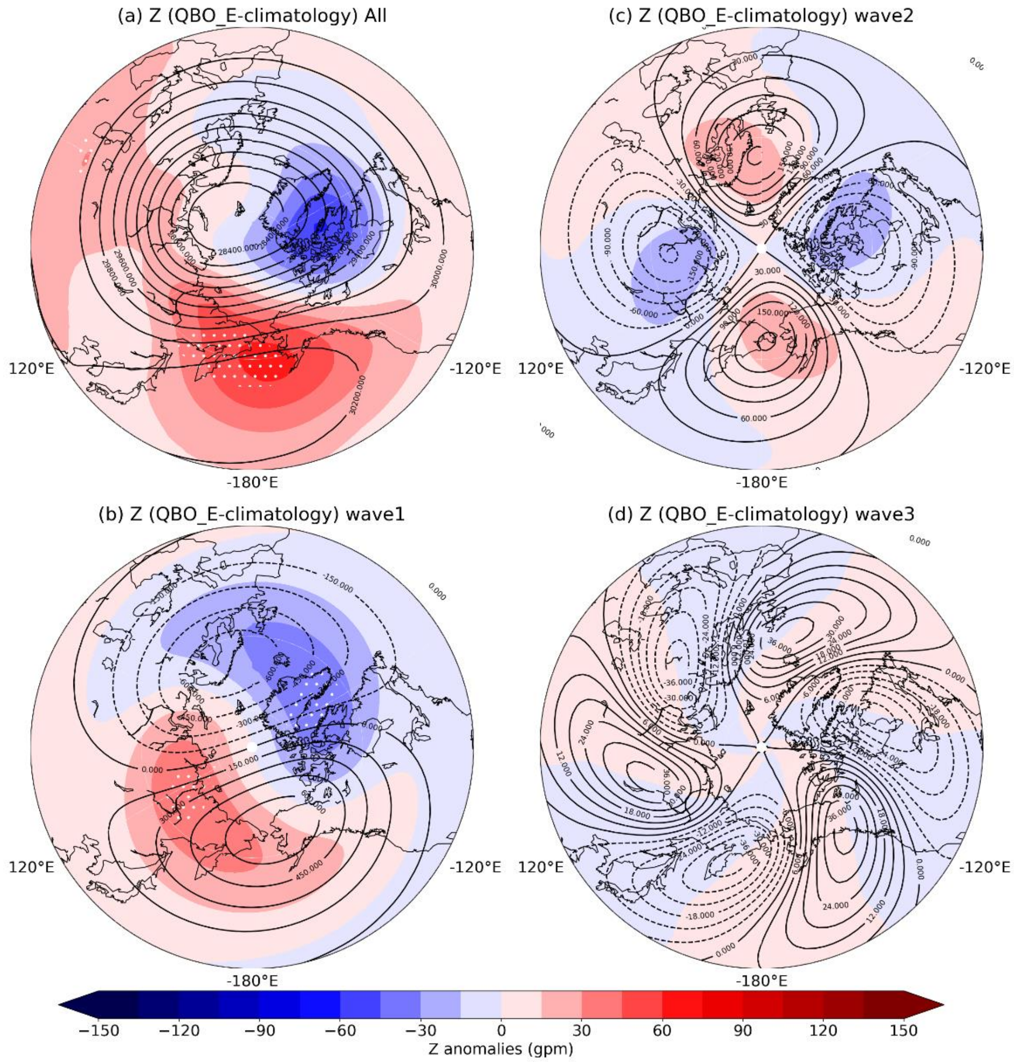

Figure R2. (a) Influences of QBOE (QBOE-climatology) on geopotential height (Z at 10 hPa) in the northern hemisphere winter (DJF) based on ERA5 data for the period 1979-2020. **(b-d)** The corresponding changes of geopotential height associated with QBO in wave numbers 1-3. The climatological values of geopotential height in winter as well as the climatological patterns of wave numbers 1-3 are also shown (contourlines). Stippled areas indicate results that are statistically significant over the 95% level, using the two-tailed Student's t-test.

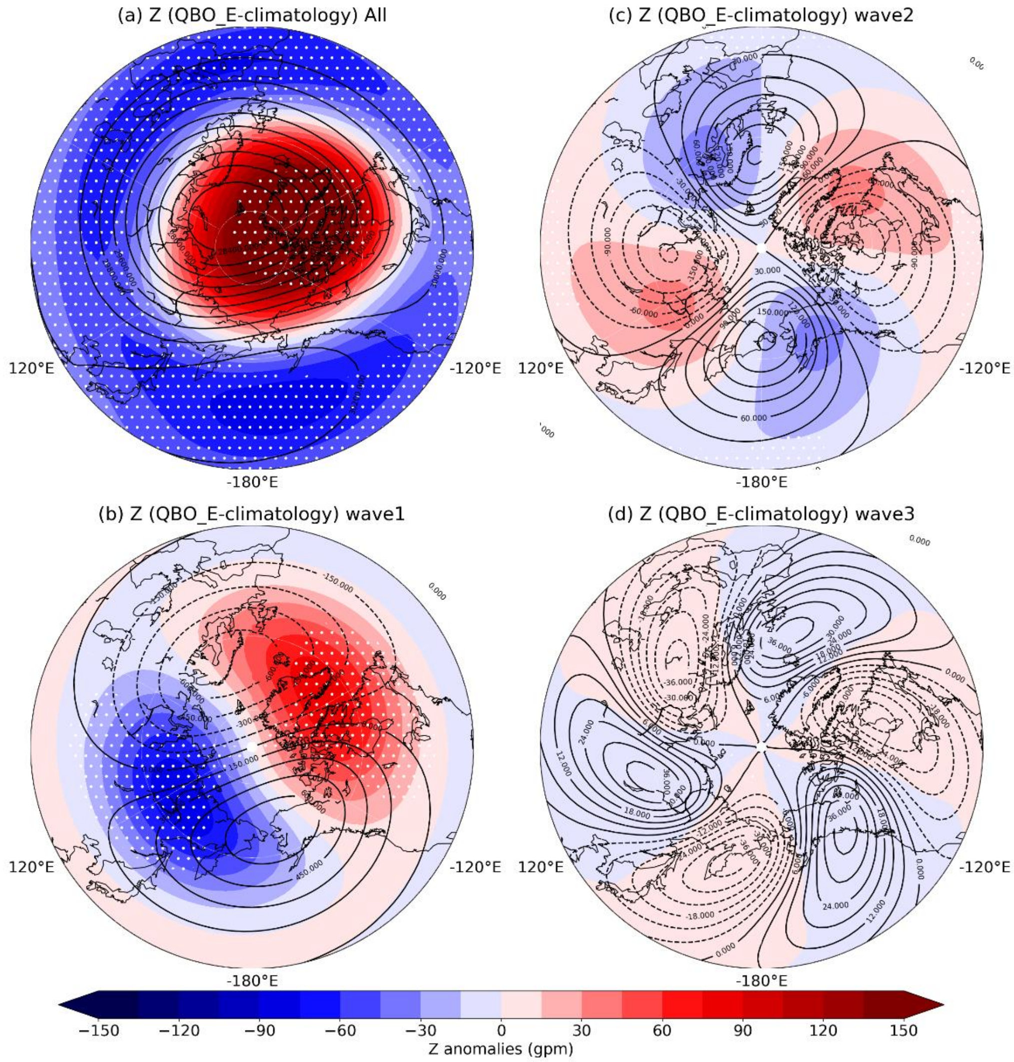

Figure R3. (a) Influences of QBOE (QBOE-climatology, using the 50 hPa U as the QBO index) on geopotential height (Z at 10 hPa) in the northern hemisphere winter (DJF) based on ERA5 data for the period 1979-2020. **(b-d)** The corresponding changes of geopotential height associated with QBO in wave numbers 1-3. The climatological values of geopotential height in winter as well as the climatological patterns of wave numbers 1-3 are also shown (contour lines). Stippled areas indicate results that are statistically significant over the 95% level, using the two-tailed Student's t-test.

An interesting point is the phase shift of the anomaly compared to the climatology in Fig. 12. This seems to be a new finding. I hope the authors represent this for the anomalies in the QBOW and QBOE phases separately with the climatology contours and discuss the reasons.

As shown in Figures R1-R2, the phase shift of the anomaly compared to the climatology is evident for both QBOW and QBOE phases. The reason for the phase shift may be explained by the wave activity changes as shown in Figure R4 (Figure 13 in the main text). Note that there are some differences between the current version of Figure 13 and the last version. This is because we used the 2-D (horizontal) divergence of the wave fluxes in the former version, but use the 3-D (both horizontal and vertical) divergence of the wave fluxes in the current version, which is more reasonable since the vertical propagation of waves and its divergence is very important. Compared to QBOE phases, more waves propagate upward from the troposphere to the stratosphere over the eastern Eurasia and North Pacific sector (60° E to 120° W, red contour lines in Fig. 13a) of the Arctic (north of 70° N), but less waves propagate upward in other sectors of the Arctic (blue contour lines in Fig. R4a) during QBOW phases. The favorable upward propagation of planetary waves over eastern Eurasia and the North Pacific may be due to the relatively large climatological wave flux from the troposphere to the stratosphere in these regions (Elsbury et al., 2021). This leads to a weakening of the zonal wind in the eastern Eurasia and North Pacific sector but an enhancement of zonal wind in other sectors of the Arctic due to the wave-mean flow interactions. To conserve angular momentum and maintain mass continuity (Kidston et al., 2015), the weakening (strengthening) of the zonal wind near the pole leads to stronger (weaker) westerlies in the subpolar regions (50-70° N) over eastern Eurasia and the North Pacific (other sectors). On the other hand, 3-D waves diverge in the eastern Eurasia and North Pacific sector but converge in other sectors (shading in Figure R4a), which also contribute to the stronger (weaker) westerlies in the subpolar regions (50-70° N) over eastern Eurasia and the North Pacific (other sectors) due to the wave-mean flow interactions. This indicates a shift of the polar vortex in the subpolar regions from North America and the North Atlantic to eastern Eurasia and the North Pacific, which is consistent with the geopotential anomalies as shown in Figure 12 in the main text and also Figure R1. This shift of the polar vortex is also consistent with previous studies (e.g., Elsbury et al., 2021). We hope this can help to explain the phase shift of the anomaly compared to the climatology. However, why the changes of planetary waves associated with QBO show varying characteristics over different regions awaits further studies.

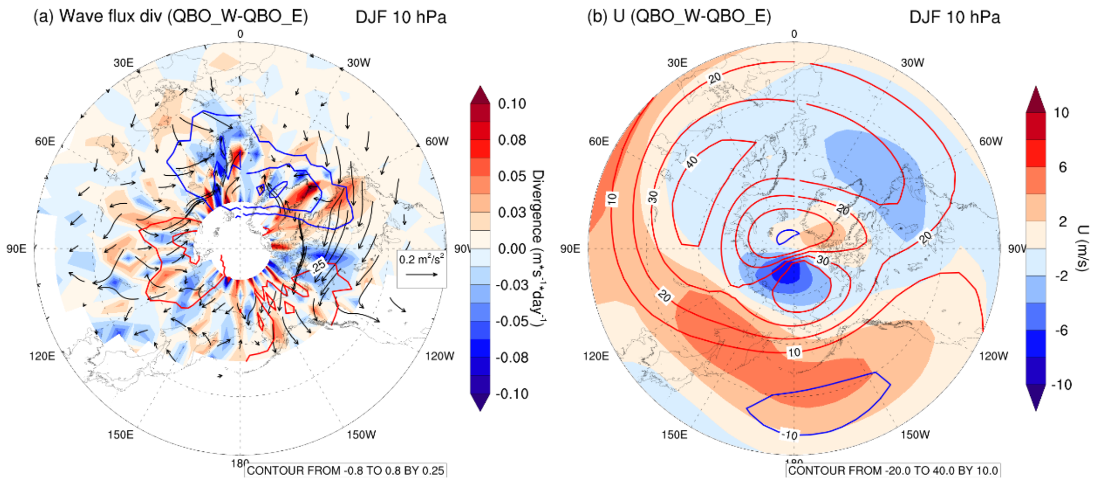

Figure R4. Influences of QBO (QBOW-QBOE) on T-N wave flux **(a)** and zonal winds

(b) at 10 hPa north of 30° N during winter (DJF) based on ERA5 data for the period 1979-2020. In **(a)**, the meridional and zonal components of the wave flux are shown as vectors, the vertical component is shown as contour lines (positive in red) and the divergence of the wave flux is shaded. In **(b)**, the climatological values of zonal wind during QBOE are shown in contour lines (solid lines for westerly) and the anomalies (QBOW-QBOE) are shaded. Stippled areas indicate results that are statistically significant over the 95% level, using the two-tailed Student's t-test.

Another issue is that there are several sentences in which it is difficult to understand the logic or physical meaning. For example, the authors should carefully explain that the data period of TCO (1979–2020) and that of ozone at 10 hPa (2002–2020) is different when they compare between Figs.8 and 11.

We apologize for the confusing information. The different period of analysis is due to the data availability. The TCO data is available since 1979, while the satellite observed ozone with global distribution at different levels is only available since 2002. We have added more information in the Data and Method Section to make it clearer.

Finally, I found an inconsistency in the figure format between Figs. 2, 3, 7, and 8 and Figs. 9–11

Sorry for the inconsistency. We have changed the format of the Figs. 9-11 to the same format as Figs. 2, 3, 7, and 8.

Minor comments

The line numbers refer to the ATC (article tracked changes) version.

1. Abstract: It would be better to include the period for analysis (1979–2020) somewhere. The ERA5 data have recently been updated for the period before 1979 due to some problems affecting the performance in the tropics in the original version. However, this would not affect the case under consideration, since the target period of this article is 1979–2020.

As described in the Data and Methods Section, the periods for analysis are different for TCO (1979-2020), zonal mean (1985-2020) and 3-dimensional ozone due to the data availability. Therefore, we are sorry but cannot simply add a period for analysis here.

2. Abstract: An explanation of the QBO definition is needed, otherwise readers may be confused. See minor comment No.10.

Thanks for the good suggestion. We have added the QBO definition in the Abstract in the revised manuscript.

3. Lines 17–18, “influenced by the corresponding temperature changes and subsequent chemical reactions”: It seems better to say, “influenced by chemical reactions associated with the corresponding temperature changes.”

Thanks. We have updated this sentence.

4. Lines 29–30: Since the description associated with the references is a general description, add "e.g.," before "Son et al., 2008….", or add more appropriate references (review papers, for example).
Thanks. We have added "e.g.," before "Son et al., 2008…." as the reviewer suggested.

5. Line 30, "WMO et al., 2018": Should be "WMO 2018."
Corrected.

6. Line 58: So, what is the level used to define the QBO?
We use a QBO index around 20 hPa in this study. Such information has been described in the Data and Methods Section and also added in the Abstract and Introduction in the revised manuscript.

7. Lines 64–65: I do not understand why the global pattern of ozone changes is important to the regional UV radiation. It is certain that regional ozone change is a cause of regional UV change.
Sorry for the confusing description. It should be "While the global pattern of ozone changes is important to regional variations of the UV radiation".

8. Lines 65–66, "it is therefore interesting to look through the zonal differences of QBO signals in ozone": I do not understand how this flows logically from the previous sentence.
We try to highlight the importance of the zonal differences of QBO signals in ozone, however, it seems not successful. Therefore, we decide to remove the whole sentence in the revised manuscript.

9. Line 144, "deviation of the zonal mean": Change to "deviation from the zonal mean"
Corrected.

10. Lines 148–149, "monthly anomalies of TCO near the equator (10S-10N) are anomalously high during QBOW phases compared with QBOE.": This expression is understandable because the authors define QBO by PC1 (around 20 hPa). However, for me, and possibly some others, who are used to the definition of QBO at 50 hPa, the relationship between the QBO phase and TCO anomaly sounds reversed and may be confusing. Would you add some descriptions explaining this? For example, "…during QBOW phases (easterly around 50 hPa), as compared with QBOE (westerly around 50 hPa). I hope this kind of explanation may be repeated in other places where "QBOW" and "QBOE" appear in the text.
Thanks for the good suggestion. We have added such information in the corresponding places in the whole manuscript as suggested.

11. Line 156: Would you state here or somewhere that the anomaly distribution is the difference between QBOW and QBOE? The authors often mention a "zonally asymmetric feature" or "positive/negative anomaly is seen in …" in the text, but it is

not compared to a climatological mean field but to the field in the QBOE.
Thanks. We have defined the QBO-related anomalies/signals clearly as the differences between QBOW and QBOE (QBOW-QBOE) in the Data and Methods Section as well in the title and figure caption in every figure. We have added such information here in the revised manuscript as the reviewer suggested.

12. Line 157, “to our understanding”: Change to “to the best of our knowledge.”
Updated.

13. Line 164: Add “(Fig. 2d)” after “the QBO signals disappear in the NOQBO run”
Added.

14. Lines 164–165: I do not understand this sentence. My understanding is “Because the only difference between the two model simulations is the QBO nudging and because the difference in the two composites is similar between the simulation and the observation, this result indicates that the differences in the two composites of the observed TCO are mostly due to QBO.”
Thanks for the suggestion. Yes, the description as the reviewer suggested is clearer. We have updated this sentence.

15. Line 181, “disappear”: The phrase “not evident” would be better.
Updated.

16. Line 195, “in the stratosphere Fig. 4d”: Do you mean “in the stratosphere in Fig. 4d”?
Yes, we have corrected this sentence.

17. Line 196, “sown”: Typo.
Corrected.

18. Lines 196–198 and Fig. 5: Can you say this without statistical significance in the positive anomalies in the lower and middle stratosphere? The authors could use the 90% level figure in the previous version and explain the relatively low significance for the seasonal panels.
We are really sorry but there was a bug in our codes of the statistical significance test. Fig. 5 has been updated and the QBO-related signals are more significant at the 95% level.

19. Line 208, “SOLOMON et al.”: Should be “Solomon et al.”
Corrected.

20. Line 217, “to our understanding”: Change to “to the best of our knowledge.”
Updated.

21. Lines 219–221: Fig. 7 indicates the distribution of QBOW–QBOE. The asymmetry during QBOE is not mentioned.
Thanks. We have updated this sentence.

22. Lines 259–261: I think the authors should say, "The results are consistent with preceding studies, considering the different definition of QBO."
We thank the reviewer for the good suggestion. We have updated this sentence in the revised manuscript.

23. Lines 271–272: 10 hPa is a difficult altitude level to use to discuss the dominance of the chemical process or dynamical (transport) process for ozone, because it is a transition altitude from dynamical control at lower altitudes (except for polar lower stratosphere) to chemical control to upper altitudes. For example, in panels (d) in Figs. 9 and 10 (DJF), the ozone and temperature anomalies have opposite signs in the SH mid and high latitudes, and this is considered to result from chemical control in the austral summer. In some regions of the NH high latitudes in DJF, the anomalies have the same sign, and this is considered to result from dynamical control in boreal winter (the negative anomalies indicate less heat and ozone transport from the midlatitudes).
We thank the reviewer for the very useful information. We have added the discussions mentioned above in the discussion about the relationship between temperature and ozone anomalies.

24. Line 275, "anomalies at 10 hPa during QBOW phases": These are anomalies from QBOE, that is, QBOW-QBOE.
Updated.

25. Lines 277–279: Because Fig. 11 shows the Z difference between QBOW and QBOE, zonal asymmetry of Z in the QBOE should be mentioned.
Thanks, we have updated the descriptions here.

26. Lines 287–288, "However, ozone anomalies are all positive over the Antarctic from the merged satellite data": Which figure are you referring to? Fig. 8b? I think the data period is different between Fig. 8 and Fig. 11.
Yes, we are referring to Fig. 8b here and the data period is indeed different between Fig. 8 and Fig. 11 due to the data availability. We have added some discussions here in the revised manuscript.

27. Lines 287–290: The authors should consider and discuss the difference in data periods (2002–2020 for Fig. 8 and 1979–2020 for Figs. 11 and S6).
Thanks, we have added some discussions about this issue in the revised manuscript.

28. Line 292, "during QBOW": The distribution is an anomaly from QBOE.
Updated.

29. Lines 303–304 and 305–306: The anomalies indicated by color are those from QBOE, not from the climatology.
Updated.

30. Lines 321–328: I understand the necessity of showing the 3-D QBO anomaly in the Northern Hemisphere in DJF in Fig. 12, because in winter, planetary wave activity is active. Why did the authors explain the QBO anomalies in the NH and SH in SON? Is there nothing worth to mention in MAM? There is a description in lines 347–348 that seems to be the answer. Please mention it in Section 3.4.
We thank the reviewer for the good suggestion and have added such information as suggested in the revised manuscript.

31. Lines 332–335: Please mention the QBO phase.
Updated.

32. Line 336, "a single-factor controlling simulation": It would be better to say, "a sensitivity simulation."
Updated.

33. Line 340, 120W-30E: I found this longitude range in Conclusions and in the Abstract (line 12) but could not find it in the other sections. Is it inconsistent?
We have also added the longitude range in the main text while describing the corresponding figures.

34. Lines 469–470: Should be "WMO (World Meteorological Organization), Scientific Assessment of Ozone Depletion: 2018, Global Ozone Research and Monitoring Project–Report No. 58, 588 pp., Geneva, Switzerland, 2018."
Corrected.

35. Figs. 9–11: Why is the format for these figures different form that for Figs. 2, 3, 7, and 8?
There is not any special reason for that. We have changed the format of Figs. 9-11 to keep consistent with Figs. 2, 3, 7, and 8.

References:

Elsbury, D., Peings, Y., and Magnusdottir, G.: Variation in the Holton-Tan effect by longitude, Quart. J. Roy. Meteor. Soc., 147, 1767–1787, https://doi.org/10.1002/qj.3993, 2021.

Kidston, J., Scaife, A. A., Hardiman, S. C., Mitchell, D. M., Butchart, N., Baldwin, M. P., and Gray, L. J.: Stratospheric influence on tropospheric jet streams, storm tracks and surface weather, Nat. Geosci., 8, 433–440, https://doi.org/10.1038/NGEO2424, 2015.