Response to Reviewer 01

The authors have considerably improved their manuscript and many of my comments have been taken into account. I am not an expert in dynamics, but the manuscript seems to have a considerably improved flow and structure now. Since there have been very major revisions to the manuscript, I still have some comments:

Response: We sincerely thank the reviewer for the meticulous review, constructive comments, and valuable suggestions, which have significantly enhanced the quality of the manuscript. As suggested by the reviewer, we have carefully addressed all suggestions and incorporated them into the revised manuscript. We appreciate the reviewer's time and effort in improving our work.

As suggested by the second reviewer, we have modified the title to include 'QBO' in the revised manuscript.

General comments:

- The conclusions could put the findings of the radiative forcing calculations more into perspective instead of simply repeating the results from the previous section.

Response: Thank you for this suggestion. In the conclusion, we have added a few sentences on the impact of ozone enhancement and associated RF on the atmosphere (L 457-463).

- Again in the conclusions, health effects of ozone are mentioned. I understand this as a consequence of elevated tropospheric ozone concentrations, even though the authors mention in their answer to my previous points that they focus on the radiative impact. However, I do not see directly from this paper how much ozone is transported to surface altitudes (where humans could inhale it) due to the intrusions followed by the SSWs discussed here. So I would again suggest to formulate more precisely here.

Response: We thank the reviewer for pointing this out. Since our study did not find a significant enhancement in near-surface ozone attributable to SSW-related intrusions, following the reviewer's suggestion, we have removed this sentence from the conclusion in the revised manuscript.

- I appreciate the disclosure of figure generation by python (instead of the previous software). However, I think it is not necessary to mention the python software on every figure caption. From my point of view, this information does not need to be shown at all, since python is one of the standard tools for data analysis these days. The reference to the specific scripts in the end of the manuscript would be sufficient, in my opinion.

Response: As suggested, reference to Python has been removed from the figure captions, and the information is now provided only at the end of the manuscript (L 473-474).

Specific comments (all line numbers according to the ATC document):

L26: The abbreviations QBO and WQBO-SSW should be given on its first occurrence.

Response: As suggested, the abbreviations are now defined at their first occurrence in the manuscript (L-24-25).

L97: "All SSWs were examined during the period from 1962 to 2018": This information was already given in the beginning of the section.

Response: This redundant line has been removed from the revised manuscript.

L101: "... causing anomalous cooling of 18°C": What is meant here? Cooling to temperatures of 18°C or cooling to temperatures 18°C cooler than before? In the latter case Kelvin would be the more appropriate unit (since it describes absolute temperatures, even though the delta is the same in Celsius)

Response: Here, "anomalous cooling of 18 °C" refers to a temperature decrease of 18°C relative to the pre-event conditions. The text has been revised to explicitly state this to avoid ambiguity (L 93-94).

L135: "For the Monte Carlo," -> "For the Monte Carlo test,". Further, I don't understand the term "null" in this context. Is a "null hypothesis" in a statistical sense meant?

Response: We apologize for the confusion regarding the term '*null*'. Here, *null* refers to the background (non-SSW) reference ensemble constructed by calendar-matched resampling from non-SSW years. This has been clarified in the revised manuscript (L 125-126).

L385 & Fig. 6: The text says that "the day of maximum intrusion" was chosen to compile figure 6. I think this should be also mentioned in the figure caption. Otherwise one could think of this figure as an average.

Response: Thank you for pointing this out. The caption for Figure 6 (Figure 7 in the revised manuscript) has been updated to explicitly state '*the day of maximum intrusion*'.

L435: I suggest to move this sentence "We emphasise..." to the end of the conclusions to stress this important point and to not end the main part of the manuscript with a citation of other work.

Response: Thank you for this valuable suggestion. The sentence has been moved to the end of the conclusions section (L 468-469).

Response to Reviewer 02

The authors addressed a lot of my comments, and the revised manuscript on the WQBOSSW teleconnection is very interesting. However, due to significant changes in the analysis, a few additional major comments have been added.

Response: We sincerely thank the reviewer for careful reading and constructive comments. We have carefully addressed all suggestions at the line numbers indicated in the replies. We appreciate the reviewer's time and effort in improving our work.

As suggested by the reviewer, we have modified the title to include 'QBO' in the revised manuscript.

Major comments:

1) A clear definition of stratospheric and PV intrusions is needed. In many contexts, the authors seem to consider the lowering of the tropopause as a stratospheric intrusion. However, if the tropopause lowers smoothly over the course of more than a month, should we still consider this a stratospheric intrusion? Additionally, not every lowering of the tropopause is accompanied by stratosphere-to-troposphere transport (STT). Since the UTLS region is a boundary region, discussions of stratospheric intrusions or STT are confusing. Please clarify this.

Response: Thank you for this thoughtful comment. We have clarified the definition of stratospheric intrusions in the Methods (L 138-140). We have revised the discussion to replace "tropopause lowering" with "tropopause folds/PV streamers," which more accurately reflects the dynamical intrusion signatures analyzed in this study (L 214-215).

2) The authors provided a good explanation of how WQBO leads to a shift in jet and tropopause height. However, the link between SSW and the signals is unclear. The given mechanism occurs during the whole WQBO period, which is much longer than the time window under consideration. How does the SSW lead to such a focused signal near its onset day? Are the SSW and QBO signals linearly added, leading to a constructive ozone signal during WQBO and a destructive one during EQBO? Or is the relationship between them nonlinear and unique to the WQBO-SSW?

Response: Thank you for this thoughtful comment. To clarify the SSW-specific contribution within each QBO phase, we stratified data as per QBO phase at the time of onset. we analyze the RWB and ozone for each case (discussed in Sect. 3.3 in the revised manuscript (Fig. 7b-d) (L 421-426)). These results indicate that the ozone anomalies in the UTLS over South Asia are influenced by the combined WQBO-SSW rather than WQBO-nonSSW.

3) Similar to the first comment, 2PVU cannot be used directly for RWB diagnostics. Since 2PVU is often used as a dynamical tropopause, it can be related, but explanations are needed, especially when the 13-day average is considered. This time window may exceed the life cycle of the RWB. Please include further explanations.

Response: In the revised manuscript, we have clarified that PV is used as a dynamical tracer of stratospheric air and that the 2-PVU contour is adopted as a proxy for the dynamical tropopause. Stratospheric intrusions are identified based on the downward/equatorward extension of high-PV air as PV streamers/tropopause folds, consistent with prior studies (Sec 2.1, L 138-140).

We have also revised the Fig. 7 caption to clarify that the composites are not a 13-day average. We select the event-wise day of maximum intrusion within the ±6-day window and composite those selected days, which is more consistent with the synoptic life cycle of RWB.

4) In the discussion, the authors emphasize the threat to the community posed by warming and air quality issues from the ozone increment during WQBO-SSW. However, the authors mention that they could not find a significant ozone signal near the surface in the response. Considering the short duration of increased ozone and the corresponding radiative forcing, the threat to the community seems minimal. It would be helpful to provide more details on the impact of increased UTLS ozone on air quality and its effect on warming through radiative forcing.

Response: The above-mentioned text has been removed from the revised manuscript because it was causing confusion. We have also expanded the discussion on the impact of ozone in the UTLS on RF (Sect. 3.4: L 436-437, and L457-463).

Minor comments:

1) L1-2: To provide direct information about the paper's findings, I would suggest adding QBO to the title.

Response: As suggested by the reviewer, we have modified the title to include 'QBO' in the revised manuscript.

2) L27: "Deepening" of the tropopause is a confusing phrase. Perhaps "lowering" is a better term.

Response: Thank you for the suggestion. The term "deepening" has been replaced with "lowering" of the tropopause to improve clarity. (L 26)

3) L27-29: The term "Rossby wave penetration" is confusing. What does it mean for a Rossby wave to penetrate? If it means propagation, then Rossby-wave propagation occurs regardless of the shift. Please clarify this sentence.

Response: We thank the reviewer for bringing this to our attention. In the revised manuscript, we have modified the sentence to avoid confusion (L 27).

4) L32-33: This last sentence was unclear in the discussion. How does elevated ozone in the UTLS pose a threat to humans and vegetation? Please refer to the major comments related to the discussion.

Response: Since our study did not find a significant enhancement in near-surface ozone attributable to SSW-related intrusions. Following the reviewer's suggestion, we have removed this sentence from the revised manuscript.

5) L79: I suggest adding Lee et al. (2025) to the reference. They addressed the global impact of STT near the surface under SSW, which is not limited to high latitudes and covers the South Asia region.

Lee, J., Butler, A. H., Albers, J. R., Wu, Y., & Lee, S. H. (2025). Impact of sudden stratospheric warmings on the stratosphere-to-troposphere transport of ozone. Geophysical Research Letters, 52(2), e2024GL112588.

Response: We thank the reviewer for the suggestion. The reference Lee et al. (2025) has been added in the revised manuscript. (L 78).

6) L85-87: The air quality issue in Lu et al. does not seem related to STE, nor does it seem to align with the rest of the paragraph.

Response: Thanks for pointing this out. The reference has been removed from the revised manuscript.

7) L97: "All SSWs were examined..." This sentence repeats the first sentence of the paragraph.

Response: Thanks for pointing this out. The repeating sentence has been removed from the revised manuscript.

8) L97-99: The current sentence structure is confusing because it does not clearly show what revealed the finding.

Response: We acknowledge the reviewer's concern. The sentence has been removed from the revised manuscript to improve clarity and avoid confusion.

9) L122-123: "Overall, ERA5..." This sentence doesn't seem to flow smoothly from the previous sentences, which pointed out the negative aspects of ERA5.

Response: Thank you for pointing this out. We have revised the text to improve the flow by moving the 'Overall ERA5..' sentence to the preceding discussion from Fadnavis et al. (L 113-114)

10) L123-124: "We analyzed..." This sentence is repeated from the first sentence of the paragraph.

Response: Thanks for pointing this out. The repeated sentence has been removed from the revised manuscript.

11) L127-129: Charlton and Polvani consider separating events from the final warming and events that occur close together. Is this addressed in the paper?

Response: All the SSW events in the present study are identified according to the criteria of Charlton and Polvani.

12) L139: The explanation of how the composites are tested is too brief. How did you determine whether the anomaly differed from zero? Did you create a distribution with 20,000 samples of the mean anomaly?

Response: Thank you for pointing this out. We have revised the manuscript to state clearly how the significance of the composite mean is tested (L 128-131).

13) L176-178: Please rephrase this sentence.

Response: Thank you for the suggestion. The sentence has been rephrased as suggested. (L 181-183)

14) L178-179: What does it mean to emphasize here?

Response: Thanks for pointing this out. This sentence has been removed from the revised manuscript to avoid confusion.

15) Figure 1: If the 380 K isoline is used for the tropopause in (a), is it the same in (b) and (c)? Also, was the tropopause in (b) and (c) calculated for the given days, or is it climatological? Please define the tropopause and explain why this definition was chosen.

Response: Thank you for pointing this out. To avoid ambiguity and ensure consistency across Figures (Fig. 2 in the revised manuscript), we now use the lapse-rate tropopause (LRT) to demarcate the troposphere and stratosphere in all panels. Further, LRT is computed for the corresponding days, not for a climatology. We also added a definition of the LRT and a brief justification for this choice in the methods (Sec. 2.1, L 142-146).

16) Figure 2: A polar projection seems unnecessary when focusing on South Asia. Also, since the sub-figures are small, it is difficult to identify the propagation and breaking of waves. Would it be possible to zoom in on the South Asia region if we are not discussing other regions?

Response: As suggested by the reviewer, Figure 2 (Figure 3 in the revised manuscript) is replotted with the South Asian region in focus, and the original polar projection plots for GPH anomaly (Fig. S2 in the revised manuscript) are moved to the supplementary.

17) L257-261: Stronger winds do not facilitate Rossby wave breaking (RWB). I assume the authors want to mention the jet stream and its role as a wave guide, but RWB itself is not directly related to strong winds. RWB is more related to the PV gradient and wave activity. Additionally, it is too much of a logical leap to connect stronger winds directly to higher ozone intrusions. Please address this mechanism more carefully.

Response: Thank you for this important suggestion. We agree that Rossby wave breaking (RWB) is not directly related to stronger winds. Fig. 4b was included to show that westerlies become more prominent toward the equatorward sector (65–90°E, 10–20°N) around the onset

day. Following the reviewer's suggestion, we have revised the text and removed that sentence (L 281-286).

18) Figure 3: The color range for Figure 3a seems inappropriate. Most of the values are above the range, making it difficult to identify the intensification of zonal winds.

Response: We acknowledge the reviewer's concern. The color range for Figure 3a (Figure 4a in the revised manuscript) has been adjusted to better capture the intensification of zonal winds.

19) L293: Are we excluding 2018 SSW?

Response: Yes. The 2018 SSW has been excluded from the composite, following the reviewer's suggestion in Revision 1.

20) L295-296: Could you parametrize and demonstrate this in a scatter plot? Mentioning the detailed examination without providing supporting details is less convincing.

Response: Thank you for pointing this out. We have removed this statement from the beginning of the paragraph in the revised manuscript. Following the reviewer's suggestion, we have now added a new figure in the Section. 3.3 (Fig. 7b) showing the area-averaged occurrence frequency of Rossby wave breaking (RWB) over our study region for WQBO–SSW, EQBO–SSW, WQBO-nonSSW, and EQBO-nonSSW conditions. This analysis clearly indicates higher RWB occurrence for the WQBO–SSW cases.

21) L296-300: Since nothing has been shown yet, starting with "our analysis" is confusing.

Response: As suggested, this sentence has been modified in the revised manuscript (L 318-323).

22) L326-328: If needed, you can compare the significance level not ozone concentration

Response: As suggested, this sentence has been modified in the revised manuscript (L 347-350).

23) Figure 4c-d: Does this dipole signal exist throughout the events, or does it indicate two different groups with different peaks?

Response: The dipole pattern is observed throughout the events.

24) L354-356: Need a reference on the relationship between the deepening of a trough and stratospheric intrusions.

Response: Relevant references have been added in the revised manuscript (L 374-375).

25) Figure 5: Again, most of the values are outside the range of the color bar.

Response: The reviewer may kindly note that the color range in Fig. 5 (Fig. 6 in the revised manuscript) was intentionally chosen to highlight the features over South Asia. As a result, values over the polar region appear saturated. However, this does not affect the interpretation of the South Asia patterns discussed in the manuscript.

26) L400-401: Please indicate the direction of the positive radiative forcing.

Response: Positive TOA radiative forcing denotes an increase in net downward radiative flux, $\Delta (F_{in} - F_{out}) > 0$. This sign convention is now stated in the revised manuscript (L 169).

27) L430-433: The SSW trend may not be sufficient to cause a significant WQBO-SSW trend, particularly since the EQBO experiences SSW more frequently (Garfinkel et al., 2012).

Garfinkel, C. I., Shaw, T. A., Hartmann, D. L., & Waugh, D. W. (2012). Does the Holton–Tan mechanism explain how the quasi-biennial oscillation modulates the Arctic polar vortex?. Journal of the Atmospheric Sciences, 69(5), 1713-1733.

Response: Thank you for pointing this out. This sentence has been removed from the revised manuscript.

Response to Reviewer 03

Review of revised manuscript of Roy et al.: Large Ozone Intrusions during Sudden Stratospheric Warmings Enhance Ozone Radiative Forcing Over South Asia

I want to thank the authors for their extensive work to improve their manuscript. For me the revised version has a much clearer message and I think the authors are on track to provide a valuable manuscript. However, I still have two more general and some minor technical concerns which I hope the authors can resolve before final publication.

Response: We sincerely thank the reviewer for the thorough review of our manuscript. We have carefully considered all the comments and revised the manuscript accordingly. We appreciate the reviewer's time and effort in helping us to improve our work.

As suggested by the second reviewer, we have modified the title to include 'QBO' in the revised manuscript.

In lines 212/213 and subsection 3.1.1:

I am still not convinced about the causality here and the role of the SSW. In other words, a Rossby wave feature is described which is (maybe by chance, maybe not) associated with an SSW event and the westerly phase of the QBO. And the Rossby wave dynamics coincide with SSW features as described in 3.1.1. My concern is that a rather general feature of RW dynamics is described here (southward displacement of the higher latitude tropopause). The point of the analysis is that this displacement occurs over South Asia in this setting of SSW and QBO. Otherwise, e.g., in an easterly QBO phase this displacement occurs somewhere else but not over South Asia. The other point is: would you see similar ozone anomalies for non-SSW events which have similar RW behavior, i.e. southward displacement of the tropopause and is the SSW event in any way special here? I want to make clear here that I do not question your analysis, but I am rather curious about the significance of the described feature.

Response: Thank you for this thoughtful comment. The reviewer may kindly note that our analysis is a regional study focused on South Asia, and that our interpretations are limited to this region. To clarify the SSW-specific contribution within each QBO phase, we analyze the RWB and ozone for each case and add a discussion paragraph in Section. 3.3 in the revised manuscript (Fig. 7b-d) (L 421-426). These results indicate that the ozone anomalies in the UTLS over South Asia are influenced by SSW occurring during the westerly QBO phase.

Section 3.3:

I am still wondering whether the radiative forcing over South Asia has larger scale implications? I think it is good to know that the radiative balance over South Asia is disturbed here but actually I do not see yet whether there is a reversed feature somewhere else which leads to a total radiative forcing around zero? So what are the consequences of this imbalance over South Asia on larger scales? Or are there any local changes associated with this instantaneous forcing? The point is that values are presented but only little context is provided here around these values.

Response: Thank you for this important comment. However, the reviewer may kindly note that our radiative forcing calculations were designed as a regional study over South Asia. We therefore do not evaluate the spatial pattern of forcing outside South Asia. Nevertheless, we have added the UTLS implications of ozone-driven forcing, supported by prior literature (Section 3.4, L 436-437), to the revised manuscript.

Technical comments:

• Line 24: " … South Asia region. … " not ","

Response: Thanks for pointing this out. The punctuation has been corrected in the revised manuscript. (L 23).

• Line 27/28: What do you mean with Rossby wave penetration?

Response: By 'Rossby wave penetration', we meant more Rossby wave breaking over the South Asian region. In the revised manuscript, the wording has been corrected to "Rossby wave breaking" for clarity, and the sentence is modified accordingly. (L 27).

• Line 95-100: I think you report your own results here in the introduction without further context. I would suggest to remove these here.

Response: Thanks for pointing this out. As suggested, we have removed these lines from the revised manuscript.

• Line 101/102: in which altitude does the atmosphere cool about 18°C?

Response: We appreciate the reviewer's concern. In Shi et al. (2023), the cooling of up to ~18 °C was reported for surface air temperature anomalies relative to pre-event conditions. The text has been modified in the revised manuscript to state this explicitly**.** (L 93-94).

• Line 142 ff: This method holds for all SSWs and not only for the 2018 event, or?

Response: Thanks for pointing this out. Yes, this method applies to all SSW events. This has been clarified in the revised manuscript. (L 135).

• Method section: How do you distinguish between westerly and easterly phase of the QBO ? Can you put a small section with an explanation in Section 2.

Response: A brief explanation has now been added to Section 2 in the revised manuscript. (L 147-151).

• Section 2.3: I would suggest to make this section part of Section 3 because it belongs to the case study.

Response: As suggested, this section has been moved to Section 3 as Section 3.1

• Section 3.1.1: without a 3.1.2, a subsection 3.1.1 makes little sense and I would recommend to include this text simply into 3.1.

Response: Thank you for the suggestion. Subsection 3.1.1 has been incorporated into Section 3.2 in the revised manuscript.

• Figure 5c: Is there a meaning behind the dashed green line?

Response: The dashed green line in Figure 5c (Figure 6c in revised manuscript) indicates 30°N and was used to illustrate the equatorward shift of the subtropical jet during the westerly phase of the QBO. This line has been removed in the revised manuscript to avoid ambiguity.