

Response to Reviewer 01

I thank the authors for addressing all remaining requests. I have no further scientific comments, only technical comments which the authors can address for final publication. The line numbers are taken from the ATC2 file:

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.

L.187/212: The text and figure caption of Figure 1 state +/- 30 days, but the figure also shows +/- 60 days.

Response: The text and figure caption are corrected in the revised manuscript (L191 & L193).

L. 230: Figures 2a show → Figure 2a shows

Response: The sentence is corrected in the revised manuscript (L208).

L. 247: Holtan → Holton

Response: This is corrected in the revised manuscript (L224).

L. 327: Do you mean “vertically resolved zonal wind”?

Response: We meant to say the temporal evolution of vertically resolved zonal wind. The sentence has been revised to “time–pressure plot of zonal wind” to avoid confusion (L294).

L. 488/489: The last sentence of Sect. 3.4 would benefit from some more details, since it is relatively vague and not very meaningful.

Response: We thank the reviewer for this suggestion. We have revised the sentence to state more clearly the impacts of UTLS ozone-induced RF (L429-433).

“This radiative forcing is due to changes in ozone in the UTLS, where the ozone radiative forcing efficiency is greatest (e.g., Forster and Shine 1997; Ming et al., 2017). These changes in RF can alter local heating rate, temperature, stability, and influence high clouds (e.g., Xia et al., 2018; Nowack et al., 2014).”

L. 512: Do you refer to positive or negative ozone changes as a consequence?

Response: The sentence is revised as “Williams et al. (2024) reported a surface forcing of -0.36 W.m^{-2} associated with an increase in ozone in the UTLS” to avoid confusion (L454).

L. 534: Should read as: <https://doi.org/10.24381/cds.bd0915c6>

Response: The sentence is corrected in the revised manuscript. (L468)

L. 536: A note on the zenodo repository: The repository should indicate that the code is related to this publication (should be updated once the final doi is available), there is a dedicated section for this. Also the README could be more extensive highlighting the source of the ERA5 data, the python and library versions necessary to run the code.

Response: As suggested, this has been updated in the Zenodo repository.

Response to Reviewer 02

The authors have done an amazing job improving the paper, and it is now mostly clear. However, important gaps remain in the proposed mechanism section. Addressing this major comment and a few minor ones before publication would substantially improve the paper.

Response: We sincerely thank the reviewer for the careful 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.

Major comment:

1) The authors provided additional information indicating that the WQBO-SSW phase is unique compared to the other phases. The suggested influence of RWB seems to explain why WQBO-SSW has the strongest signal. However, the discussion of the mechanism is still lacking. First, the paper does not explain how the interaction between the QBO and the SSW leads to the change in the jet and the corresponding RWB frequency. While this discussion may be outside the scope of the study, the authors should at least offer a hypothesis or acknowledge the open question.

Response: We agree that the detailed dynamical interaction between the QBO and the SSW that produces the jet shift and corresponding RWB frequency is beyond the scope of the present study. In the revised manuscript, we acknowledge that the exact QBO-SSW coupling and RWB interaction remains an open question (L441-443).

“However, it should be noted that the interaction between the QBO and the SSW leading to the change in the jet and the corresponding RWB breaking is complex. The involved dynamics is beyond the scope of this study.”

Furthermore, if the RWB is important, one would expect to see an ozone enhancement comparable to that of the WQBO-SSW during the EQBO-SSW (Fig. 7b). However, Fig. 7d shows the opposite. The ozone concentration is lower, even compared to EQBO-nonSSW. Therefore, RWB may not be sufficient to explain why WQBO-SSW has the most ozone. I suggest examining how ozone concentrations change in the stratosphere during different phases, as transport relies on both chemical concentrations and dynamics.

Response: We thank the reviewer for this helpful suggestion. As recommended, we now analyse ozone in the upper troposphere (400–250 hPa) and lower stratosphere (150–80 hPa) separately (Figure 1 attached below). This shows that the negative ozone anomaly in the UTLS-mean field (Fig.7d in the previous manuscript) was primarily driven by negative ozone anomalies in the lower stratosphere. The upper troposphere exhibits positive ozone anomalies in both QBO phases, with the largest enhancement during WQBO-SSW over South Asia. Also, RWB leads to ozone enhancement in the upper troposphere, we have revised Fig. 7c–d to focus only on the upper-tropospheric response.

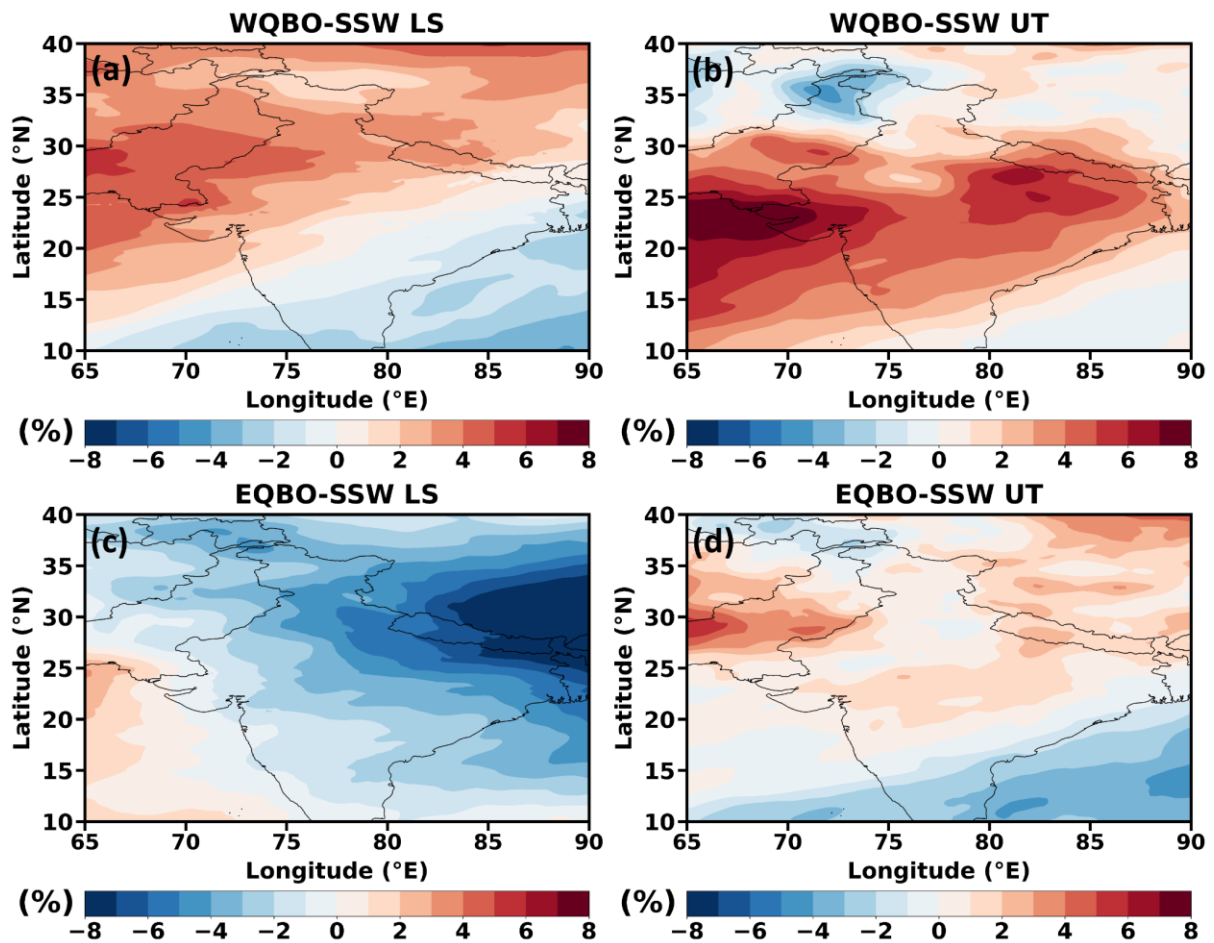


Figure 1. Ozone anomaly composites (in %) for ± 30 days around onset in the (a) lower stratosphere, and (b) upper troposphere for WQBO-SSW relative to the corresponding phase-matched non-SSW climatology (WQBO-nonSSW). (c-d) same as that of (a-b) but for EQBO-SSW relative to the corresponding phase-matched non-SSW climatology (EQBO-nonSSW).

Minor comments:

1) L92-94: The flow before and after this sentence is not smooth. All the other paragraph is about ozone over South Asia, while this sentence emphasizes the SSW impact on the East Asia on the weather perspective. It is hard to follow how it motivates study on ozone variability.

Response: As suggested, this paragraph is revised in the manuscript to clearly show the motivation of the study (L92-102).

“The February 2018 SSW was a major SSW characterized by pronounced downward propagation of stratospheric signals into the troposphere. It prominently impacted weather across the Northern Hemisphere, including cold extremes over Asia, North America, and Europe (Shi et al., 2023; Lu et al., 2020; Kautz et al., 2020; Xie et al., 2020). Signatures of this event were also seen in ST-Radar observations over India (Ramya et al 2021). ST-Radar observations showed downward propagation of horizontal wind and a pronounced increase of westerly wind amplitude in the UTLS. These findings suggest that the SSW influences the South Asian UTLS. Transport of ozone and ozone radiative forcing due to SSW over South

Asia are not investigated yet. Since 2018-SSW is a major SSW, and there is evidence of its influence over India (ST-Radar observations), we therefore choose 2018-SSW as a case study.

2) L111-116: I suggest rephrasing this section. It would be clearer if it were structured as follows: Although ERA5 is biased, it is better than the others, which is why we use it.

Response: As suggested, this sentence is rephrased in the revised manuscript as "Although ERA5 ozone is biased, it performs better compared to other reanalyses (Fadnavis et al., 2023)" (L119-120).

3) L129-131: It's unclear what the authors are comparing here, as it says "same" non-SSW background ensemble. Are you saying that you took 27 random non-SSW days and repeated that 20,000 times, or did you compare composites with the same distribution that you used for the 2018 SSW case?

Response: We apologize for the lack of clarity in the methodology section. This paragraph has been revised in the manuscript to describe the methodology more clearly (L127-140).

“The non-SSW years were first separated into easterly QBO (EQBO-nonSSW) and westerly QBO (WQBO-nonSSW) years. For each non-SSW phase, we applied a Monte Carlo resampling method. For this, calendar-matched non-SSW background samples were constructed by randomly resampling days within the same day-of-year window for the EQBO-nonSSW and WQBO-nonSSW years separately (Dai et al., 2022). In each case, 20,000 random samples of non-SSW episodes were generated. The mean of 20,000 samples for WQBO-nonSSW is referred to as the ‘WQBO-nonSSW climatology’. The same procedure is repeated for the easterly non-SSW phase and is referred to as ‘EQBO-nonSSW climatology’. This climatology was used to calculate composite anomalies for the SSW years (EQBO-ssw - EQBO-nonssw climatology, WQBO-ssw - WQBO-nonssw climatology). For the 2018 SSW, which occurred during the westerly QBO phase, we used the ‘WQBO-nonSSW climatology’ for calculating its anomaly. The spread of the 20,000 resampled non-SSW sets for each phase was used to evaluate the statistical significance of the changes caused by the SSWs within that phase (Dai et al., 2022).”

4) L145-146: troposphere-stratosphere boundary -> tropopause; If we want a tropopause consistent with dynamical changes, why aren't we using a dynamical tropopause (2PVU), which you already used for intrusion analysis?

Response: The 2-PVU surface was not used as the tropopause itself because, in the tropical–subtropical transition region, PV-based tropopause definitions become ambiguous and a fixed 2-PVU threshold does not always represent the true tropopause accurately (Kunz et al., 2011).

In the manuscript, the 2-PVU contour is therefore used only as a dynamical indicator of Rossby wave breaking and stratospheric intrusion. While LRT tropopause is used to show demarcation between the troposphere and stratosphere.

5) L147-148: The reference for the dataset is missing.

Response: Reference “Naujokat, 1986” is added in the revised manuscript (L157).

Naujokat, B. (1986). An update of the observed quasi-biennial oscillation of the stratospheric winds over the Tropics. *Journal of the Atmospheric Sciences*, 43(17), 1873–1877. [https://doi.org/10.1175/1520-0469\(1986\)043<1873:auotoq>2.0.co;2](https://doi.org/10.1175/1520-0469(1986)043<1873:auotoq>2.0.co;2)

6) L149: The latitude is missing from the QBO definition.

Response: Latitude is added in the revised manuscript (L157-L159).

“The classification of westerly and easterly QBO phases is based on winds at 70 hPa, over the equatorial latitude band (2°S–2°N).”

7) L213-214: It would be better to clearly state the existence of the tropopause folds, even without showing a figure, since this can easily be verified.

Response: As suggested, this sentence is rephrased in the revised manuscript (L221).

“In addition, the reduced tropopause height near the onset in Fig.2a shows the occurrence of tropopause folds.”

8) Figure 3: The caption says 3-day intervals, but the titles say from -2 to +2 days. Please clarify this discrepancy.

Response: The figure caption is corrected in the revised manuscript with “1-day intervals” (L271).

9) L280: Is there a reason to mention the specific latitude (23 N)?

Response: 23°N is mentioned only to indicate the equatorward shift of the jet. Since this equatorward shift is evident in the figure 4a, we have removed this specific latitude reference from the revised manuscript.

10) Figure 6 & 7: For reference, could you show the climatology and the anomaly from the climatology? For example, show the climatological jet location versus the jet location in different phases (Figure 6b), the climatological 2-PVU height (Fig. 7a), and the ozone anomalies from the climatology (Figs. 7c and 4). This would help to consistently compare the WQBO-SSW with the others. Also, what is the climatological RWB occurrence frequency over the region (Fig. 7b)?

Response: In the current version of the manuscript, we have considered phase separated non SSW climatology. Non SSW climatology is generated instead of total climatology (SSW+nonSSW). However, as suggested by the reviewer, we have examined the anomalies with respect to the total climatology. These additional diagnostics are shown below for comparison. We find that the features remain similar, though the magnitude is smaller when the total climatology is used.

However, in the manuscript, we want to retain the anomalies obtained from the corresponding phase-matched non-SSW climatology. This is more appropriate baseline for isolating the SSW-

related signal within each QBO phase. This choice minimizes contamination from the background differences between WQBO and EQBO conditions and SSW and nonSSW. We would also like to note that Reviewer 1 agreed with this methodology.

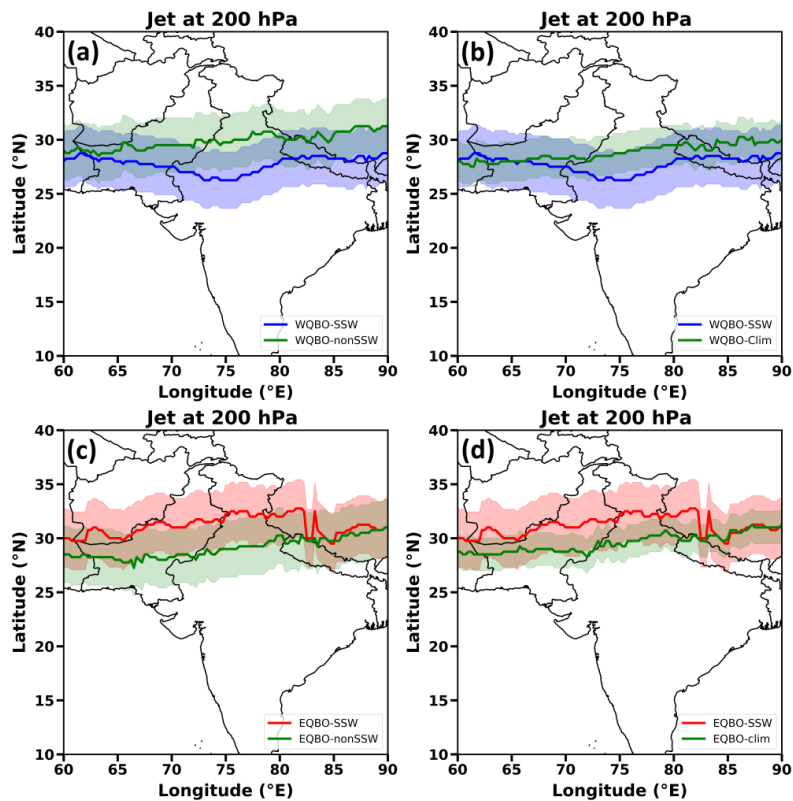


Figure 2: Spatial map of jet core at 200 hPa averaged for ± 6 days around the onset. Blue line in (a-b) and red line in (c-d) represent the jet core for the westerly phase and easterly phase of QBO, during SSW years. The green line represents the climatological jet corresponding to (a) WQBO-nonSSW, (b) WQBO total climatology, (c) EQBO-nonSSW, and (d) EQBO total climatology, respectively. The shading in (a-d) represents standard error.

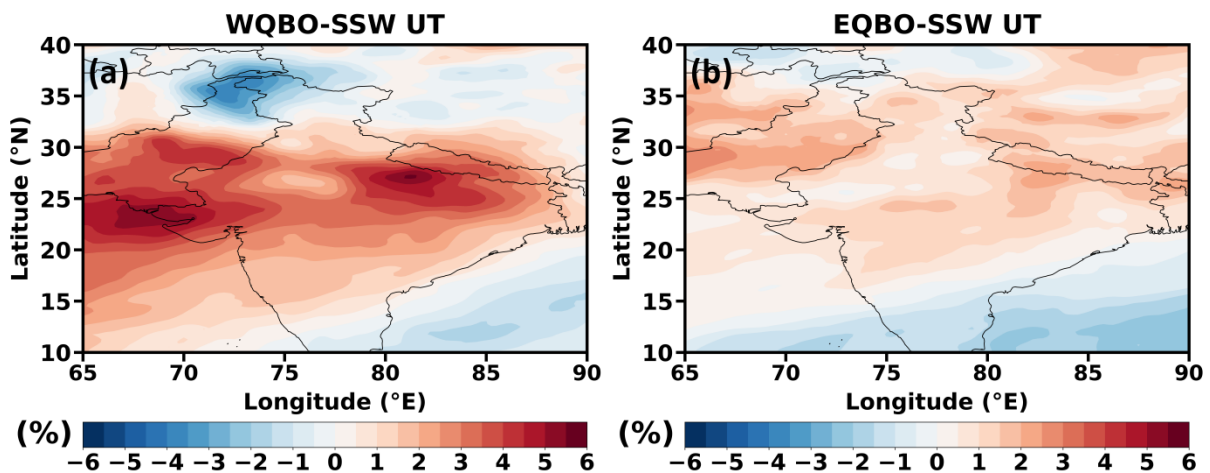


Figure 3: Upper tropospheric ozone anomaly composites (400-250 hPa; in %) for ± 30 days around onset for WQBO-SSW relative to the corresponding WQBO total climatology. (a) same as that of (b) but for EQBO-SSW relative to the corresponding EQBO total climatology.

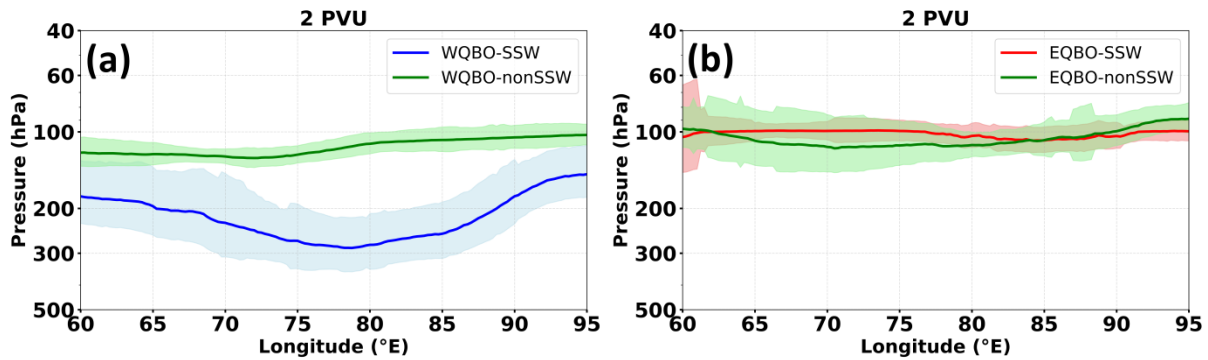


Figure 4: Longitude-pressure cross section of 2PVU line averaged over South Asia (20-35°N) within ± 6 days around the onset. Blue line in (a) and red line (b) represents the 2PVU line for WQBO-SSW and EQBO-SSW composites, respectively. The green line in panels (a-b) denotes the climatological 2PVU line corresponding to (a) WQBO-nonSSW and (b) EQBO-nonSSW climatology, respectively. The shading in (a-b) represents standard error.

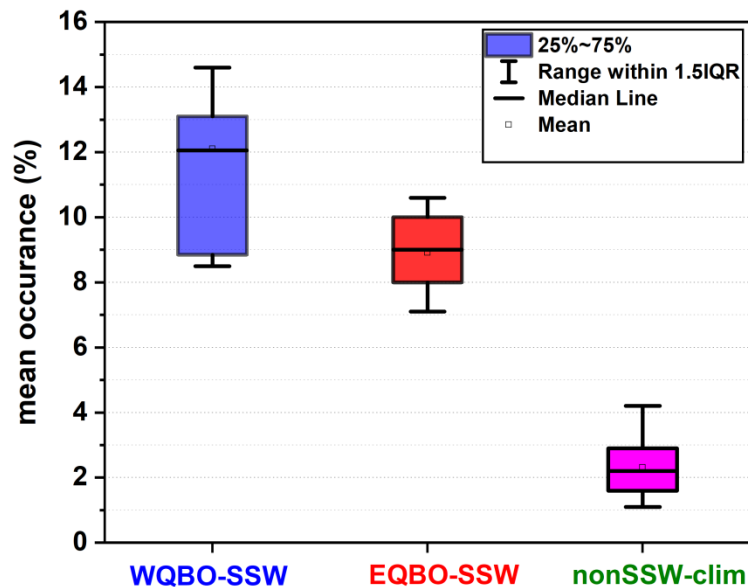


Figure 5: Area-averaged occurrence frequency (%) of ozone-rich high-PV intrusion signatures consistent with Rossby wave breaking events during January-March over South Asia identified from $PV > 2$ and $Ozone > 80$ ppbv at 300 hPa for WQBO-SSW, EQBO-SSW, and nonSSW climatology.

11) L405: Does maximum intrusion mean the deepest intrusion based on the pressure level?

Response: Yes, here “maximum intrusion” refers to the “deepest intrusion”. We have changed the term “maximum intrusion” to “deepest intrusion” in the revised manuscript.

12) L406-409: Could you provide a more detailed explanation of the RWB definition? Using ozone for RWB identification is novel and would benefit from additional justification.

Response: Since RWB events produce ozone-rich, high-PV intrusion signatures in the upper troposphere, we used $PV > 2$ PVU and ozone > 80 ppbv at 300 hPa to identify these events. The reviewer may kindly note that ozone > 80 ppbv is chosen because it exceeds the climatological mean value of ozone at 300 hPa over the study region. This is now mentioned in the methodology (L149-152).

13) L424-425: UTLS ozone is only compared between WQBO-nonSSW and WQBO-SSW (Fig. 7c). This sentence needs the anomaly compared to climatology as suggested earlier.

Response: Reviewer may kindly note that the ozone anomaly relative to total climatology is shown in Figure 3a–b (Response 10). The overall pattern remains similar when compared to either the phase-separated non-SSW climatology or the total climatology. The magnitude is smaller for total climatology because it includes SSW years. However, in the manuscript, we used phase-matched non-SSW climatology, which we consider the more appropriate baseline for isolating the SSW-related signal within each QBO phase.