Replies to the reviewer-III

The study addresses sudden stratospheric warmings, especially split-downward propagating ones, and their impact on ozone increment over South Asia from the upper troposphere-lower stratosphere to the near-surface. Using the ERA 5 dataset from 1962 to 2018, the authors classified SSW events into three groups: split-dSSW, displace-dSSW, and nSSW. The paper showed a significant increase in ozone over the UTLS and near-surface around the SSW onset. The paper further focuses on the 2018 SSW event among 12 split-dSSWs to understand the dynamics, and concluded that Rossby wave breaking intensification relevant to SSW is the cause of the ozone increment. The authors additionally calculated the radiative forcing of such ozone increment.

Roy et al. covers relevant scientific questions within the scope of ACP, but needs major revision. To be specific, the dynamic linkage with SSW and ozone increment is weak and needs further explanation. Also, the significance test and the year 2018 as a representative case are not convincing enough. Lastly, figures and writing could be improved.

Response: We sincerely thank the reviewer for the meticulous evaluation, constructive comments, and valuable suggestions. In the revised manuscript, we have addressed the motivation for highlighting the 2018 SSW event and clarified the underlying mechanism. Our revised analysis reveals that, phase of the Quasi-Biennial Oscillation (QBO) plays a more critical role in shifting the subtropical jet position and associated Rossby wave breaking over the South Asian region during SSWs. Therefore, in the revised manuscript, we have reclassified the SSWs into two categories based on the prevailing QBO phase: SSWs coinciding with the westerly phase (WQBO-SSW) and those coinciding with the easterly phase (EQBO-SSW). Our results indicate that WQBO-SSW events are associated with a large positive ozone anomaly over the South Asian region compared to EQBO-SSW events. We have also incorporated all recommendations and improved the overall structure. All changes are marked in the track-change version of the manuscript, with line numbers referenced in the responses.

Major comments

1. How can ozone response prior to and close to the SSW event date be caused by SSW? It requires some time for the response to propagate to the UTLS midlatitudes and low latitudes.

Response (1): We agree with the reviewer. However, we would like to emphasize that, unlike high latitudes, where SSWs exert a direct downward influence on the troposphere, our analysis indicates that the low-latitude responses (such as over South Asia) are mediated primarily by Rossby-wave dynamics. Specifically, by RWB and PV streamer intrusions along the subtropical waveguide. The location of these RWBs is modulated by the positioning of the subtropical jet during SSWs. It is well established that major SSWs are preceded by enhanced mid-latitude planetary and synoptic wave driving (e.g., Baldwin et al., 2021). Whether and how that wave activity projects into South Asia depends on the background flow set by the QBO. During the westerly QBO, the associated secondary circulation warms the equatorial lower stratosphere and cools the subtropics, sharpening and shifting the UTLS meridional temperature gradient equatorward (e.g., Hitchman et al., 2021). By thermal-wind balance, this strengthens upper-tropospheric westerlies on the equatorward flank and displaces the subtropical jet equatorward over the South Asian longitudes, favouring subtropical wave guidance, RWB, and PV-streamer intrusions. We now detail this mechanism in Section 3.1.1.

(a) Baldwin, M. P., Domeisen, D. I. V., Hegglin, M. I., Garny, H., Garfinkel, C. I., Langematz, U., Charlton-Perez, A. J., Butchart, N., Gerber, E. P., Birner, T., Butler, A. H., Ayarzagüena, B., and Pedatella, N. M.: Sudden Stratospheric Warmings, Reviews of Geophysics, 59, https://doi.org/10.1029/2020rg000708, 2021.
(b) Hitchman, M. H., Tegtmeier, S., Yoden, S., Haynes, P. H., and Kumar, V.: An Observational History of the Direct Influence of the Stratospheric Quasi-biennial Oscillation on the Tropical and Subtropical Upper Troposphere and Lower Stratosphere, Journal of the Meteorological Society of Japan. Ser. II, 99, 239–267, https://doi.org/10.2151/jmsj.2021-012, 2021.

2. The novelty and motivation of the study should be emphasized more. Although not focused on South Asia, previous studies like William et al. (2024) and Lee et al. (2025) have already calculated the radiative impact and global ozone anomaly due to SSW. I think the authors showed an interesting result that only split-dSSW has a substantial impact, while other types of SSW do not. I recommend emphasizing this more and providing additional explanations for why.

Response (2): Thank you for the helpful suggestion. Our revised analysis highlights that SSWs occurring during the westerly phase of the QBO (WQBO-SSW) are associated with a more pronounced equatorward shift of the subtropical jet, enhanced Rossby wave breaking, and deeper ozone intrusions over South Asia compared to SSWs coinciding with the easterly phase (EQBO-SSW). The detailed mechanisms underlying these features are discussed in the revised manuscript (section 3.2).

3. When introducing the previous studies and comparing, please match the units and consider the time scale. For example, % and ppb units are hard to compare and confuse their importance. Also, stratospheric intrusion (hourly to daily) should be carefully compared to the stratosphere-troposphere exchange (often in seasonal, interannual, and even climatology) or trend due to anthropogenic emissions.

Response (3): We appreciate the reviewer's suggestion. In the revised manuscript, we harmonise units and timescales when introducing prior work and making comparisons. We also have modified the sentences where stratospheric intrusions are compared with STE and the trend due to anthropogenic emissions.

4. Is the Student t-test appropriate in this sample size? I do not understand how a significance test is being conducted on a single case (year 2018). Also, 12 cases of split-dSSWs are pretty small. I would recommend adding more detail on how the significance test is done and improving the statistical robustness of the result.

Response (4): We apologise for not describing our statistical testing in sufficient detail. We have now added the details on how the statistical tests are done in the Methods section of the revised manuscript (L134-141).

Given the small sample size for and the likelihood of non-normal distributions, we replaced the initial Student's *t*-test with the Monte Carlo bootstrap and the Wilcoxon signed-rank test in the revised manuscript. "For the Monte Carlo, we built a calendar-matched null by resampling days from non-SSW years within the same day-of-year window. We then use a bias-corrected and accelerated (BCa) bootstrap with 20,000 resamples to form 95% confidence intervals. For 2018, we checked whether the observed value lay outside the BCa interval of the background ensemble. For the composite, we tested whether the mean anomaly differed from zero. Next, we applied an exact Wilcoxon signed-rank test to the same data. A grid point is called significant only when both tests agree at 95% significance." This approach offers three advantages: (a) distribution-free inference suitable for small samples, (b) improved coverage from BCa intervals that correct bias and skewness, and (c) robustness of the Wilcoxon test to outliers and non-Gaussianity (Efron, 1987; Efron & Tibshirani, 1993; Davison & Hinkley, 1997; Wilcoxon, 1945)."

(a) Efron, B.: Better Bootstrap Confidence Intervals, Journal of the American Statistical Association, 82, 171–185, https://doi.org/10.1080/01621459.1987.10478410, 1987.
(b) Efron, B. and Tibshirani, R. J.: An Introduction to the Bootstrap, chapman hall crc, https://doi.org/10.1201/9780429246593, 1994..
(c) Davison, A. C. and Hinkley, D. V.: Bootstrap Methods and their Application, cambridge university, https://doi.org/10.1017/cbo9780511802843, 1997.
(d) Wilcoxon, F.: Individual Comparisons by Ranking Methods, Biometrics Bulletin, 1, 80, https://doi.org/10.2307/3001968, 1945.

5. Can the year 2018 represent the split-dSSWs? Figures 2 and 3 compare the year 2018 with the other split-dSSWs. However, the year 2018 seems to be anomalously strong, and the patterns look quite different. If there is a reason to choose the year 2018, please explain it. Also, as stated in the paper, due to anthropogenic emissions and global warming, tropospheric ozone has a positive tendency. If this is not detrended, could there be a bias in the impact of SSW in the year 2018?

Response (5): We appreciate the reviewer's concern. We have examined all major SSWs from 1962 to 2018. Our analysis revealed a relatively more equatorward shift of the subtropical jet over South Asia and associated large ozone intrusion during the 2018 SSW compared to other

SSW years. This motivated us to report the detailed mechanism of the 2018 SSW as a case study. We have included this motivation in the introduction section of the revised manuscript (L97-100). This equatorward shift facilitates the eastward-propagating synoptic-scale Rossby waves to move further equatorward, favouring RWB and PV-stremer activity along with more ozone intrusion (Homeyer & Bowman, 2013; Albers et al., 2016). This unique dynamical setup explains why the 2018 event stands out as an outlier in the UTLS ozone response. In the revised manuscript, we have discussed this aspect in detail (see section 3.1.1).

To the reviewer's comment on "trend", to mitigate potential bias from long-term tropospheric ozone trends due to anthropogenic emissions and global warming, we detrended the ozone data over 1962–2018 in the revised methodology. The enhancement in 2018 is therefore interpreted as SSW-related dynamical transport rather than an artefact of the trend.

6. Authors emphasized the impact on the near-surface ozone level from SSW, but detailed explanations are missing. How can the signal in the UTLS propagate toward the near-surface? The propagation of geopotential height in the figure is difficult to recognize. Also, tropospheric ozone has multiple sources, such as local emissions and tropospheric long-range transport. What is the contribution of each driver to the ozone increment, and is the stratospheric intrusion truly the dominant driver?

Response (6): We appreciate the reviewer's concern. In our revised methodology, after detrending the ozone data over the study period (1962-2018), we found that the near-surface signal is weak and not significant. Accordingly, we have removed the discussion of surface impacts. The revised manuscript now focuses exclusively on the UTLS region, where the stratospheric intrusion signal is robust and more directly attributable to SSW-related dynamics.

7. It seems the Rossby wave breaking is the suggested mechanism for the ozone intrusion. However, it is hard to see that RWB has been amplified or more frequent due to the split-dSSWs. It would be helpful to explicitly show that the frequency or amplitude of the RWB, whichever matters, differs by type of SSW. The current qualitative discussion with Fig. 5 is not convincing enough.

Response (7): We appreciate the reviewer's concern. However, the reviewer may kindly note that the categorization of SSW has been changed in the revised manuscript. We now consider all the major SSWs during the study period and categorize them based on the QBO phase. This has been mentioned in the methodology of the revised manuscript. Further, in the revised manuscript, we present the longitude-pressure composite of PV (2 PVU contour) averaged over the South Asian region for both the western and easterly phases of the QBO (Fig. 6). During the westerly phase of the QBO, the 2 PVU composite extend below 300 hPa into the upper troposphere. This behaviour is consistent with stronger and more frequent RWB in WQBO compared to EQBO, facilitating deeper PV-streamer penetration over the region.

8. Is the radiative forcing due to SSW significant over this region? If so, further information to compare and show the importance could be helpful. Also, what is the temperature change due to this radiative forcing?

Response (8): The radiative forcing due to SSW is statistically significant ($p < 0.05$) and has a warming effect over South Asia. All RFs are positive and exceed the uncertainty range (±1 standard deviation). This kernel-diagnosed RF reflects the event-scale radiative response to SSW-related ozone/temperature anomalies and, by design, is not directly comparable to published effective radiative forcing from long-term changes in ozone or anthropogenic influence. Hence, we opt to refrain from a comparison and instead report the magnitude ($W.m^{-2}$)

To the reviewer's comment on "temperature change due to this radiative forcing…", in this study, our focus has been on examining ozone intrusions into the UTLS during SSWs and their associated radiative impacts. Hence, we did not explicitly quantify the resulting temperature changes due to radiative forcing. We acknowledge that establishing a link between ozone-induced radiative forcing and UTLS temperature response is an important avenue for future investigation.

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.

Minor comments

9. L47: What does this 'increase' mean? A trend or variability?

Response (9): Thanks for pointing this out. We have removed this line from the revised manuscript.

10. L48-50: Add reference to show the contribution of the stratosphere to the South Asian ozone

Response: Thanks for pointing this out. In the revised manuscript, references are added (L45-46).

11. L98: Isn't Lu et al. (2023) only focused on the stratospheric ozone variance?

Response (11): We acknowledge the reviewer's concern. After revisiting Lu et al. (2023), we recognize that their work is primarily focused on stratospheric ozone variability. Accordingly, we have removed the reference.

12. L99-100: Willims et al. (2024) only focused on the PJO type of SSW when leading to that conclusion. Please clearly state it.

Response (12): Thanks for pointing this out. We have now revised the sentence to accurately reflect the scope of the original study (L79-81).

13. L111: Maybe split-dSSWs? Not all the downward-propagating SSWs?

Response (13): Thanks for pointing this out. In the revised version, we investigated the impact of all the SSW events from 1962 to 2018 on ozone variability in the UTLS over the South Asia region (L92-94).

14. L127-129: What does the corresponding daily mean of all the non-SSW days mean? Is every day except SSW onset days considered?

Response (14): We appreciate the reviewer's concern. The daily climatology is constructed using data from non-SSW years during the extended winter season (November–May). Anomalies were computed by subtracting this daily climatology from the respective calendar days of SSW years. We have clarified this point in the revised manuscript (L130-132).

15. L152: UTLS definition should be included.

Response (15): Thanks for pointing this out. In this study, we consider the upper troposphere and lower stratosphere (UTLS) region to span the pressure levels from 300 hPa to 50 hPa, consistent with previous literature (e.g., Chavan et al., 2021). We have added this in the revised manuscript (L93).

(a) Chavan, P., Fadnavis, S., Griessbach, S., Chakroborty, T., Sioris, C. E., and Müller, R.: The outflow of Asian biomass burning carbonaceous aerosol into the upper troposphere and lower stratosphere in spring: radiative effects seen in a global model, Atmospheric Chemistry and Physics, 21, 14371–14384, https://doi.org/10.5194/acp-21-14371-2021, 2021.

16. L159-163: These two sentences seem to be repeated.

Response (16): Thanks for pointing this out. We have removed the sentence from the revised manuscript to avoid repetition.

17. L193-195: Hall et al. (2021) used ERA-Interim and ERA 40 for the classification. Is ERA5 consistent with these two datasets?

Response (17): In most cases, ERA5 shows higher accuracy than that of ERA-Interim and ERA-40 (Hersbach et al., 2020, S-RIP Final Report, 2024).

18. L210-212: Why is ± 30 days selected when the previous sentence mentioned up to 60 days after SSW onset?

Response (18): We appreciate the reviewer's concern. In the revised manuscript, we have focused on the ±30-day window, as this period best captures the dominant signal over the study region (L126–127). Since the most pronounced ozone enhancement occurs within ±6 days of the SSW onset, all subsequent analyses in this study are conducted for this period (L202-203).

19. L220: I think this is a typo. Maybe section 3.2, not 2.2?

Response (19): We thank the reviewer for bringing this to our attention. Yes, this was a typographical error. The sentence has been removed in the revised manuscript.

20. L237-239: It is hard to see the downward propagation of GPH in the figure

Response (20): Thanks for pointing this out. In the revised manuscript, we have removed this term.

21. L239-241: I can't see the lowering of the 380K. Also, why is the 380K isoline being used here?

Response (21): We agree that the lowering of the 380 K potential temperature isoline is not clearly visible in the figure. Accordingly, we have removed this sentence. We have used the 380 K potential temperature isoline as a proxy for the tropopause in tropical and subtropical regions (Fueglistaler et al., 2009). Examining the 380K contour alongside ozone and GPH anomalies provides a meaningful way to identify stratosphere-to-troposphere exchanges during SSW events.

(a) Fueglistaler, S., Mote, P. W., Folkins, I., Fu, Q., Dessler, A. E., and Dunkerton, T. J.: Tropical tropopause layer, Reviews of Geophysics, 47, https://doi.org/10.1029/2008rg000267, 2009.

22. L252-255: What leads to the two different peaks in 2018?

Response (22): Thanks for pointing this out. In our revised methodology, after detrending the ozone data over the study period (1962–2018), we found that the near-surface signal is weak and not statistically significant. Accordingly, this discussion and the related figure (Fig S6) are removed from the revised manuscript.

23. L282-283: I don't think this has been discussed later in this section.

Response (23): Thank you for bringing this to our attention. This aspect is discussed in Section 3.1.1, not in the current section. We have corrected this in the revised manuscript accordingly (L207-208).

24. L283-286: Where is the anomalous lowering of the tropopause? Figure 3 doesn't show the anomalous tropopause.

Response (24): We thank the reviewer for pointing this out. The term "anomalous lowering of the tropopause" has been removed, and the sentence is modified in the revised manuscript (L208-210).

25. L323-326: It is hard to see the wave-1 and wave-2 pattern in Fig. 4. Also, please explain the connection with vertically coherent waves at 10 hPa and 200 hPa to the subtropical jet perturbation. Currently, there is no explanation why the first sentence leads to the conclusion in the next one.

Response (25): We appreciate the reviewer's concern. In Figure 4 (in the old manuscript), GPH anomaly maps allow identification of planetary wave structures: wave-1 is characterised by a single ridge–trough dipole encircling the pole, while wave-2 manifests as two alternating ridge–trough pairs (e.g., Seviour et al., 2013). However, in the revised manuscript, we have removed this term.

In Figure 4 (in the old manuscript), we observed that regions with low GPH at 10 hPa (Fig. 4a-e in the old manuscript) correspond to similar low GPH anomalies at 200 hPa (Fig. 4f-j in the old manuscript). To describe this feature, we used the term 'strong vertical coherence'. However, in the revised manuscript, this sentence is removed, and the section has been modified as per our revised methodology.

26. L338-340: Which pattern is the persistent GPH anomaly over South Asia? A more detailed description would be helpful.

Response (26): We acknowledge the reviewer's concern. The persistent pattern corresponds to low GPH over South Asia. A more detailed description of this feature has been added in the revised manuscript (L231-233).

27. Figure 4: It is hard to follow the explanation without the latitude grid line

Response (27): Thanks for this suggestion. The grid line is added in Figure 4 of the old manuscript (Fig. 2 in the revised manuscript).

28. L368-369: Please explain in more detail about the synoptic wave pattern that only exists in Fig. 5c. It is hard to see in this figure. Also, the figure is rotated from Fig. 4, which is confusing.

Response (28): We acknowledge the reviewer's concern and apologize for the confusion. We ensured that in the revised manuscript, the orientation in Figure 5 (Fig. 5b-d in the revised manuscript) is consistent with Figure 4, and a detailed explanation is added (L350-351).

29. L370-371: What does it mean to enhance RWB? Is it amplitude-wise or frequency-wise?

Response (29): In our analysis, "enhanced RWB" refers to both its frequency and amplitude. In the revised manuscript, we present the longitude-pressure composite of PV (2 PVU contour) averaged over the South Asian region for both the western and easterly phases of the QBO (Fig. 6). During the westerly phase of the QBO, the 2 PVU composite extend below 300 hPa into the upper troposphere. This behaviour is consistent with stronger and more frequent RWB in WQBO compared to EQBO, facilitating deeper PV-streamer penetration over the region.

30. L432-435: A reference is needed for the future projection.

Response (30): Thanks for pointing this out. Reference is added in the revised manuscript (L431).

(a) Kim, J., Park, H.-S., Son, S.-W., and Gerber, E. P.: Defining Sudden Stratospheric Warming in Climate Models: Accounting for Biases in Model Climatologies, Journal of Climate, 30, 5529–5546, https://doi.org/10.1175/jcli-d-16-0465.1, 2017.

31. L435-436: A reference is needed for the high-top model performance expectation.

Response (31): Thanks for pointing this out. Reference is added in the revised manuscript (L436-438).

(a) Scaife, A. A., Charlton-Perez, A. J., Son, S.-W., Hardiman, S. C., Polvani, L., Lim, E.-P., Haynes, P., Baldwin, M. P., Shepherd, T. G., Perlwitz, J., Richter, J. H., Noguchi, S., Thompson, D. W. J., Karpechko, A. Y., Butler, A. H., Scinocca, J., Sigmond, M., Domeisen, D. ShiI. V., and Garfinkel, C. I.: Long-range prediction and the stratosphere, Atmospheric Chemistry and Physics, 22, 2601–2623, https://doi.org/10.5194/acp-22-2601-2022, 2022.