Response to the reviewer-II

Large Ozone Intrusions during Sudden Stratospheric Warmings Enhance Ozone Radiative Forcing over South Asia by S. Roy et al. The study of Roy et al. presents an analysis on how sudden stratospheric warmings (SSWs) related to a split vortex potentially affect ozone levels in the troposphere over South Asia. The use ERA5 data and categorize all SSWs between 1963 and 2018 into split vortex and displaced vortex events, as well as in downward and non-downward propagating SSWs. Furthermore, they discuss an event from 2018 in more detail and also present a composite analysis on all downward propagating, split vortex SSWs. From their point of view, they conclude that ozone is enhanced due to Rossby wave dynamics in relation to the SSWs which in turn affects the ozone levels in the troposphere over South Asia and enhances the tropospheric radiative forcing from ozone.
The impact of SSWs on the troposphere has been documented in recent literature. The local effect of changes in trace gas concentrations, here particularly, on additional ozone from the stratosphere in the troposphere is still an open question. For this the topic is of relevance and also in scope of Atmospheric Chemistry and Physics. However, the conclusions in the present study look not very convincing to me so far. I have issues following the discussion in the present form. In particular, I feel that the connection between the SSW and the tropospheric effects needs more in depth analysis, or at least more convincing arguments in written form. Currently, I often do not see the connection between the SSW and tropospheric effects beyond vague lines of argumentation. I therefore recommend major revision before publication. I will lay out my concerns in more detail below and I hope that the authors can resolve my concerns in a revised manuscript.

Response: We sincerely thank the reviewer for the meticulous evaluation, constructive comments, and valuable suggestions. We acknowledge the concern regarding the clarity of the connection between SSWs and tropospheric impacts in the current version. 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 show that WQBO-SSW events are associated with more ozone intrusions compared to EQBO-SSW events. Accordingly, we have revised sections 3.1 and 3.2.

Major comments

1) Presentation of case study in 2018 In Section 2.3 the case study is introduced. This is an important point because here the idea of the study is presented and the basis is laid for the composite analysis. I would therefore recommend to discuss this case in more detail in an individual section. The figures should also be part of the main manuscript and not the

supplement. More so, a reason should be given why this case is presented in more detail and not one of the other 11 dSSW cases. I would also recommend to put all figures related to this case into this discussion first. And then have a separate discussion of the composite, i.e., Figures 1a, 2a, 3a,c, 4, 5a,b.

Response (1): As suggested by the reviewer, a separate section is added for the 2018 SSW event (Sec. 3.1), followed by the discussion of composite (Sec. 3.2) in the revised manuscript. Also, we have now included the motivation for taking the 2018 SSW as a case study in the introduction (L97-100).

The supplementary figures (wind reversal (Fig. S1) and PV evolution (Fig. S2)) are provided to show the change in stratospheric dynamics around the onset of the 2018 SSW event. However, the revised analysis now emphasizes the role of the QBO in modulating the equatorward shift of the subtropical jet and associated ozone intrusion. Therefore, including those supplementary figures in the main text will not add substantial new information to the ongoing discussion. Hence, we have not moved those figures from the supplement.

2) Composites

The composites are made from a very small number of events which is of course related to the fact that the discussed feature is a rare event. But of course this makes the composites also susceptible to outliers. In this case the 2018 case looks like an outlier (in particular, this seems to be the case in the Fig. 3c). So I wonder how much does the 2018 case contribute to the shape of the composite? Or vice versa, how does the composite look like without the 2018 case? In particular, Figures 3a and 3b look very much alike and made me wonder about this.

Response (2): We understand the reviewer's concern. We have now shown the composite excluding the 2018 event (Fig. 4 in the revised manuscript). The figure below shows the composite excluding the 2018 event for your reference. Although the values have marginal differences, the pattern remains largely the same.

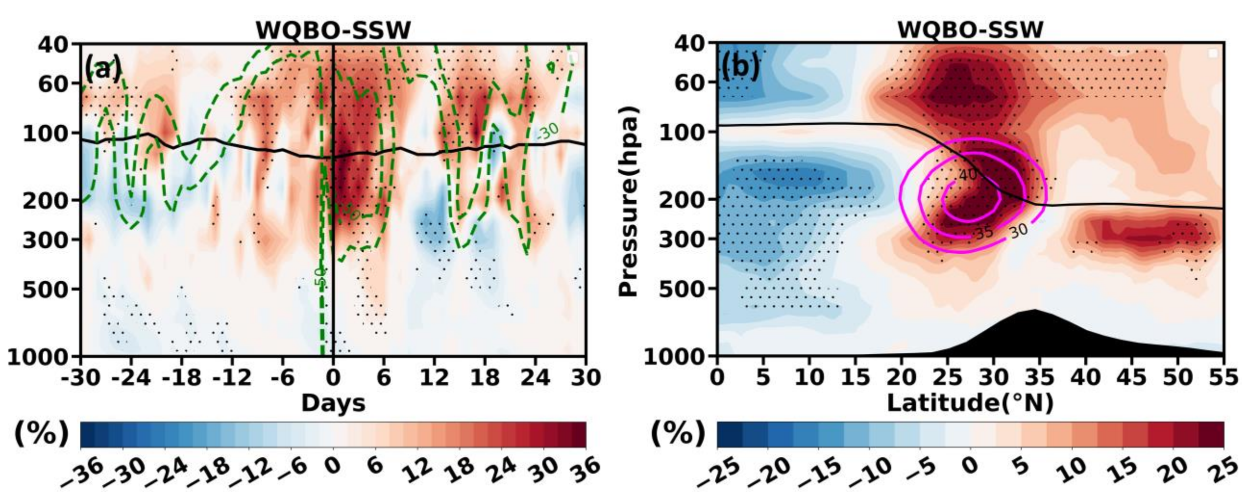

Figure 1: (a) Temporal evolution of vertical ozone anomalies averaged over South Asia (65-90°E, 20-35°N) from ±30 days around the onset for the composite of all WQBO-SSWs without 2018. (b) Latitude-pressure section of ozone anomalies averaged over South Asia (65-90°E) for ±6 days around the onset for the composite of WQBO-SSWs without 2018.

3) Connection between stratospheric and upper tropospheric dynamics
I have issues with the chronological sequence of the processes. In Figure 2a,b the ozone maxima in the UTLS occur right at the time of the SSW event. But should there not be a time lag between the vortex split at around 10 hPa and the effects evident at 200 hPa? In line 322 it is stated: "Our analysis shows strong vertical coherence between 10 and 200 hPa levels (see Fig. 4a–e and Fig. 4f–j)" But actually, I do not see a vertical coherence in these figures. I also would not expect it due to the time lag between the processes at 10 and 200 hPa. What I also wonder is why the maximum ozone anomaly is evident even before the onset of the split dSSW. The pattern in Fig 2a looks for me more like a positive anomaly caused by RW dynamics. But from this figure I do not directly see the connection to the split dSSW. I think the authors should work out this point much clearer.
Also the discussion centered around Fig 4 f)-o) and Fig. 5 looks to my like a discussion which is centered around Rossby wave dynamics. Anomalies in GPH in Fig. 4 f)-j) show positive and negative anomalies related to a Rossby wave train and Fig. 4 k)-o) show the associated ozone. In Fig. 5a) a strong jet is evident and in Fig 5 b) it becomes evident that the jetstream maximum is located over South Asia during the time of the onset of the dSSW. But in all this discussion I do not see the connection to what is happening in the mid- to upper stratosphere. I would ask the authors to better show the connection between the Rossby wave dynamics in the UTLS with the vortex in the stratosphere in a clearer way.

Response (3): We acknowledge the reviewer's concern. However, we would like to emphasise 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 are 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 Asain longitudes, favouring subtropical wave guidance, RWB, and PV-streamer intrusions. Accordingly, because the ozone enhancement is mediated by these RWB/PV-streamer intrusions, features that are often established prior to SSW onset, we do not expect a time lag. We now detail this mechanism in Section 3.1.1.

To the reviewer's comment on "vertical coherence", the reviewer may kindly note that in Figure 4, 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, we have removed the term 'strong vertical coherence' and Figure 4(a-e) (in the old manuscript).

(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.

4) Impact on troposphere

The first point I would like to make here is that is not once shown that the ozone anomalies at 200 hPa are undergoing stratosphere-troposphere exchange. From Fig. 4 k)-o) I would rather argue that the anomalies are all on the stratospheric side of the tropopause and thus have not really a significant impact on the troposphere. At least for the 2018 case study the authors should try to assess the related ozone flux from the stratosphere into the troposphere (e.g., using trajectories to calculate a mass flux, see Skerlak et al., 2014: https://doi.org/10.5194/acp-14-913-2014). The ozone impact which is discussed in the paper is more built on the ozone related transport within Rossby waves which simply advect stratospheric air masses over South Asia which then at 200 hPa produces a positive ozone anomaly. However, without an assessment of the ozone flux into the troposphere this does not significantly affect the tropospheric ozone concentration.

I also find Fig. 2d) not very convincing. The maximum ozone at 850 hPa shows to me rather the near surface increase in pollution levels over South Asia over the years. Again I am missing the connection to the stratospheric dynamics here.

I want to make clear here, I do not say that there is no ozone flux from the stratosphere into the troposphere but I do not see any proof yet that ozone transport takes place in relation to the dSSW in the presented analysis.

In turn, this puts the entire discussion centered around the radiative impact into question since I can not say whether the authors really determine the effect from ozone transported into the troposphere.

Response (4): We acknowledge the reviewer's concern. As shown in Figure 3a of the previous manuscript (now Fig. 1c), the maximum ozone enhancement during the 2018 event occurs below the tropopause, particularly in the upper troposphere near 200 hPa. To highlight this, Figure 4(k–o) (now Fig. 2f–j) focuses on ozone anomalies at 200 hPa, illustrating that the enhancements are primarily in the upper troposphere rather than confined to the stratosphere.

To the reviewer's comment on "ozone flux from the stratosphere.", the reviewer may kindly note that a full flux attribution using trajectory analysis is beyond the scope of the present study. However, to attribute the upper tropospheric ozone enhancement over South Asia to a stratospheric source, we use potential vorticity (PV) as a dynamical tracer (Fig. 2f-j and Fig. 6). PV values ≥ 2 PVU are widely used to delineate intrusions of stratospheric air into the upper troposphere (Holton et al., 1995; Kunz et al., 2015). Furthermore, we observed that negative geopotential height (GPH) anomalies in the UTLS correspond to positive ozone anomalies over the study region (Fig. 1a and 4a). This indicates troughing over the region and is commonly associated with enhanced stratospheric influence and higher ozone in the UTLS (e.g., Steinbrecht et al., 1998; Albers et al., 2022; Chen et al., 2019). We have clarified these in the revised manuscript (Sec 3.1.1).

To the reviewer's comment on "maximum ozone at 850 hPa..", in the revised manuscript, after detrending the record (1962–2018), near-surface signals were weak and not statistically significant; accordingly, Fig. 2d and all surface ozone discussion have been removed from the revised manuscript.

Minor comments/technical comments: (in order of appearance)

5. Line 25: ERA-5 → ERA5

Response (5): Corrected in the revised manuscript (L21).

6. Line 129: here you state +/- 61 days but later it is always stated +/- 60 days for the Composites.

Response (6): Thank you for bringing this to our attention. We have harmonized the values to +/-30 days in the revised manuscript (L126-127).

7. Line 141: Why are temperature, water vapor and clouds taken from the ECMWF forecast and not from ERA5?

Response (7): The reviewer may kindly note that, in our study, we did not directly use ECMWF forecast data. Instead, we applied the radiative kernel developed by Skeie et al. (2020), which was constructed using monthly mean meteorological fields (temperature, water vapour, and clouds) from ECMWF. This kernel provides a consistent framework for estimating ozone radiative forcing. In our analysis, we only used this kernel to compute the radiative forcing of ozone based on ERA5 ozone anomalies.

8. Line 146: The agreement between the radiative kernel technique and the radiative transfer model is given globally. But this study looks at local effects, so what are the maximum and minimum differences between these methods. What does a radiative transfer model take into account what the radiative kernel technique does not?

Response (8): We acknowledge the reviewer's concern. The radiative kernel (Skeie et al., 2020) has sufficient resolution (~5.6° × 5.6° horizontal, with 60 vertical levels) to calculate the RF over the study region (65-90°E, 20-35°N).

The radiative kernel method is computationally efficient for quantifying the radiative forcing than the radiative transfer model. The kernel is the partial derivative of the radiative flux calculated by a radiative transfer model with respect to small perturbations in ozone concentration at different altitudes. It is multiplied by the change in ozone concentration to compute the radiative forcing. While the radiative transfer model calculates the direct radiative transfer through the atmosphere, it considers the absorption, emission, and scattering by ozone.

9. Line 147/148: The ERA5 data is interpolated onto the kernel resolution for which it is specified to have 60 levels. How are this levels distributed in the atmosphere and what is the vertical resolution?

Response (9): The ERA5 data were linearly interpolated in pressure to 60 vertical levels, ranging from the surface up to 0.1 hPa, to match the vertical resolution of the radiative kernel. The vertical resolution of the kernel is unevenly distributed vertically. In the UTLS, it has 14 layers with level spacing ranging from 12 hPa to 34.5 hPa.

10. Line 152: UTLS has not yet been defined (except in the abstract)

Response (10): Thanks for pointing this out. We have defined UTLS in the revised manuscript

(L93).

11. Line 155 ff: Is it possible to include a satellite ozone product in the 2018 case study? This might help in the "validation" of the technique. Even more because the analysis is heavily based on ERA5 ozone.

Response (11): Thank you for the suggestion. Unfortunately, daily UTLS ozone profiles over our study region are limited: limb profilers (e.g., Aura MLS) sample too sparsely for sub-regional, day-to-day maps, and nadir sensors (e.g., OMI/OMPS) provide column ozone rather than UTLS-specific fields. We therefore rely on ERA5 ozone, which is produced by 4D-Var and assimilates multiple satellite and ground-based datasets (TOMS, SBUV/2 v8.6, SCIAMACHY, MIPAS, Aura MLS, OMI), supporting its use here (Hersbach et al., 2020; S-RIP Final Report, 2022).

a) Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., Schepers, D., Simmons, A., Soci, C., Abdalla, S., Abellan, X., Balsamo, G., Bechtold, P., Biavati, G., Bidlot, J., Bonavita, M., De Chiara, G., Dahlgren, P., Dee, D., Diamantakis, M., Dragani, R., Flemming, J., Forbes, R., Fuentes, M., Geer, A., Haimberger, L., Healy, S., Hogan, R. J., Hólm, E., Janisková, M., Keeley, S., Laloyaux, P., Lopez, P., Lupu, C., Radnoti, G., de Rosnay, P., Rozum, I., Vamborg, F., Villaume, S. and Thépaut, J.: The ERA5 global reanalysis, Quarterly Journal of the Royal Meteorological Society, 146(730), 1999–2049, doi:10.1002/qj.3803, 2020.

b) SPARC Reanalysis Intercomparison Project (S-RIP) Final Report. M. Fujiwara, G.L. Manney, L.J. Gray, and J.S. Wright (Eds.), SPARC Report No. 10, WCRP-17/2020, doi: 10.17874/800dee57d13, available at www.sparc-climate.org/publications/sparc-reports, 2022.

•12. Line 207: couple → couples

Response (12): The sentence has been restructured in the revised manuscript.

13. Line 232: Can you explain more on which data exactly you applied the student's ttest? Some assumption must be made to apply this test where I see potential issues (normality of data, continuous data, ….)

Response (13): We apologize 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, 1994; 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.

14. Line 243: DSSW → dSSW

Response (14): This term is replaced in the revised manuscript.

15. Line 333ff: How does a filament extend downward from the LS into the UT? A filament is first of all a quasi-isentropic equatorward excursion of a stratospheric air mass. It is "downward" in a sense that the lower tropopause from high latitudes also moves toward the equator.

Response (15): We thank the reviewer for this clarification. We agree with the interpretation and have revised the manuscript accordingly (L226-227)

16. Line 354 and Figure 4: It is not easy to relate this discussion to the relevant parts in the figures. Maybe it is worth splitting the figure and increase the individual panel sizes to better highlight the features the authors need for the discussion.

Response (16): Thanks for this suggestion. Figure 4 is modified in the revised manuscript (Fig. 2 in the revised manuscript). Figure 2 shows the 200 hPa GPH and PV contour to characterise the upper-tropospheric response to the vortex dynamics, together with ozone anomalies for ±6 days around SSW onset. This discussion has been added to the revised manuscript (Sec 3.1.1).

17. Figure 5a and related discussion: I do not know what the take away message here is. In Figure 5a, we simply see that there is a jet which is slightly weaker after the onset of the SSW. But is this already the effect of the SSW? At which altitude is this Hovmuller diagram taken?

Response (17): We acknowledge the reviewer’s concern. Figure 5a (Fig. 3a in revised manuscript) is plotted at 200 hPa, where the subtropical jet core is most prominent over South Asia. Our revised analysis shows that the equatorward shift of the subtropical jet is more prominent during the westerly phase of QBO than the easterly phase (Fig. 5c in the revised manuscript) during SSW years. This equatorward shift creates favourable conditions for enhanced Rossby wave breaking and PV-streamer activity (Fig. 6 in the revised manuscript) over the study region (e.g., Albers et al., 2016), thereby linking the jet dynamics to the subsequent stratospheric intrusions highlighted in our study. We have added this point in the revised manuscript (sec 3.1.1).

18. Figure 5b: Again how is this related to the vortex, all I see is that there is a maximum around the SSW onset.

Response (18): Figure 5b (in the old manuscript) illustrates the local wind response over the Indian sector around SSW onset, specifically, the transient intensification of the subtropical jet across UTLS levels. Our analysis suggests that the ozone enhancement observed over the South Asian region is more closely associated with Rossby-wave dynamics modulated by the SSW and concurrent QBO phase, rather than a direct downward influence of the polar vortex itself. Please see ***Response (3)*** for the detailed mechanism and citations.

19. Line 362: What do you mean with Rossby wave intrusion?

Response (19): We thank the reviewer for bringing this to our attention. In the revised manuscript, we have modified the wording to Rossby-wave breaking (L259).

20. Sect. 3.3: How do you compute the uncertainties in the radiative forcing which are given in the text? The radiative forcing which is given here, is this relative to the non-SSW climatology?

Response (20): We acknowledge the reviewer's concern. The radiative forcing values (mean ± 1 standard deviation) reported in our study are calculated relative to a baseline of non-SSW climatology conditions. The radiative forcing value was calculated as the spatial mean within the region for each event. The uncertainties are the ± 1 standard deviation within the region for each event.