Replies to the reviewer-I

Roy et al. analyze in their work ERA5 ozone data for different sudden stratospheric warmings in the northern hemisphere and connect these events with increased ozone concentrations at UTLS levels and near-surface. Finally, a brief analysis for the radiative impact is shown.

The manuscript addresses an important and interesting topic well within the scope of ACP, but I think this manuscript needs major revisions before considering publication. In particular, the main focus of this paper is not clear to me and also, it could be better structured. Another very important point to me is that the ERA5 data, which is the heart of this manuscript, is not discussed if it is suitable for this kind of analysis. Finally, the year 2018 is in focus without motivation why, but then its outstanding role in the time series plots is not discussed in detail. I see room for improvement on this manuscript.

Response: We sincerely thank the reviewer for the meticulous evaluation, constructive comments, and valuable suggestions. In the revised manuscript, we have incorporated all recommendations, clarified the main focus of the study, and improved the overall structure. We have also included a detailed discussion on the reliability of ERA5 ozone data for our analysis. Furthermore, we addressed the motivation for highlighting the 2018 SSW event and clarified the underlying mechanism. We greatly appreciate the reviewer's time and effort in helping to strengthen our work. All changes are marked in the track-change version of the manuscript, with line numbers referenced in the responses.

General comments:

1. Some figures are rather small and of poor quality. Please increase the size, in particular the font size of axes, labels, etc. Some axes or color bars are even not labeled at all: please add labels for all axes and color bars!

Response (1): Thank you for the helpful suggestion. As per the reviewer's suggestions, all figures have been modified (font sizes for axes, labels, and legends have been increased, and all missing axis and colour bar labels have been added) for better clarity and readability in the revised manuscript.

2. Many figures were produced using the COLA/GrADS software, but I did not find any information or reference to this tool. However, it is good that the authors mention how their figures are created.

Response (2): Thank you for pointing this out. In the revised manuscript, we regenerated all figures using Python and have explicitly stated this in the figure captions.

3. The analyses of this work are based on ERA5 ozone data. In order to estimate the findings of

this paper, I need some background information about the reliability of this ERA5 ozone data set. What is the base for the ERA5 ozone? Are there measurements assimilated? Are there validation papers? In particular: How is the UTLS represented in ERA5 in comparison to independent measurements? How does that change over the long time range analyzed in this work?

Response (3): We understand the reviewer's concern and thank you for pointing this out. ERA5 ozone is generated by 4D-Var assimilation and ingests multiple satellite and ground-based observations (e.g., TOMS, SBUV/2 v8.6, SCIAMACHY, MIPAS, Aura MLS, OMI) (Hersbach et al., 2020; S-RIP Final Report, 2022). In the revised manuscript, we have added validation/intercomparison studies of ERA5 ozone, particularly in the UTLS, to document its reliability. (L116-123).

To mitigate potential changes over the long-term range, we detrended the ozone time-series for 1962–2018 (as also suggested by Reviewer 3). This is now described in the Methods section of the revised manuscript (L132-134).

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.

4. Does ERA5 only show stratospheric ozone, which is transported to the UTLS, or are contributions from ground sources (anthropogenic, biomass burning,...) also included? If the tropospheric sources are included, how can these be separated from the stratospheric sources?

Response (4): The reviewer may kindly note that the ERA5 ozone is generated by assimilating multiple satellite and ground-based observations and gives ozone vertical profile from the surface up to a height of 80km (S-RIP Final Report, Hersbach et al. 2020).

To attribute the UTLS ozone enhancements in our study to a stratospheric source, we used potential vorticity (PV) as the primary dynamical tracer (Fig. 2f-j). PV values near or above ~2 PVU are widely used to delineate stratospheric air in the troposphere (Holton et al., 1995; Kunz et al., 2015) (Sec. 2.1). Further, we noted that negative geopotential height (GPH) anomalies in the UTLS coincide with 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).

(a) Holton, J. R., Pfister, L., Haynes, P. H., Douglass, A. R., Rood, R. B., and Mcintyre, M. E.: Stratosphere-troposphere exchange, Reviews of Geophysics, 33, 403–439, https://doi.org/10.1029/95rg02097, 1995.
(b) Kunz, A., Wernli, H., and Sprenger, M.: Climatology of potential vorticity streamers and associated isentropic transport pathways across PV gradient barriers, Journal of Geophysical Research: Atmospheres, 120, 3802–3821, https://doi.org/10.1002/2014jd022615, 2015.
(c) Steinbrecht, W., Hoinka, K. P., Köhler, U., and Claude, H.: Correlations between tropopause height and total ozone: Implications for long-term changes, Journal of Geophysical Research: Atmospheres, 103, 19183–19192, https://doi.org/10.1029/98jd01929, 1998.
(d) Albers, J. R., Elsbury, D., Butler, A. H., Langford, A. O., and Breeden, M. L.: Dynamics of ENSO-driven stratosphere-to-troposphere transport of ozone over North America, Atmospheric Chemistry and Physics, 22, 13035–13048, https://doi.org/10.5194/acp-22-13035-2022, 2022.
(e) Chen, D., Ma, L.-Y., Zhou, T.-J., Guo, D., Shi, C.-H., and Chen, L.: Statistical Analysis of the Spatiotemporal Distribution of Ozone Induced by Cut-Off Lows in the Upper Troposphere and Lower Stratosphere over Northeast Asia, Atmosphere, 10, 696, https://doi.org/10.3390/atmos10110696, 2019.

5. In the description of the materials and methods, already results (e.g. from Fig. 1) are discussed, but the Figure is reintroduced again in Section 3.1. This makes the structure inconsistent and should be reconsidered.

Response (5): Thanks for pointing this out. Fig. S1 in the supplementary and its discussion in the methods section have now been removed in the revised manuscript to avoid redundancy and ensure a clearer structure.

6. The event in 2018 is discussed in detail in this manuscript. For that reason, I am missing some background information about this SSW. How was the Arctic winter 2017/18 in general prior to the SSW? Are there articles published dealing with this specific winter, or even this specific SSW? Further, Fig. 2c looks like that this 2018 event was an extreme outlier. Are there explanations for that outstanding behavior in the UTLS? Also, some motivation, why 2018 was chosen would be helpful.

Response (6): We have examined all major SSWs during the study period from 1962 to 2018. Our analysis reveals a relatively more equatorward shift of the subtropical jet over South Asia and a corresponding large ozone intrusion in this region 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 question "How was the Arctic winter 2017/18 in general prior to the SSW? and Are there articles published dealing with this specific winter, or even this specific SSW?", The Arctic winter preceding the February 2018 major SSW was anomalously warm and characterised by reduced sea-ice extent, with widespread positive surface-air temperature anomalies of ~4–7 °C (National Snow and Ice Data Centre report). The 2018 SSW and its mid-latitude impacts have been well-documented (e.g., King et al., 2019; Butler et al., 2020; Karpechko et al., 2018). A recent study by Shi et al. (2023) reported local temperature drops of 14-18°C over Eurasia and East Asia associated with the 2018 SSW. However, to our knowledge, UTLS ozone responses over South Asia during this event have received limited attention, which motivates our emphasis on the 2018 case.

(a) King, A. D., Rudeva, I., Jucker, M., Butler, A. H., and Earl, N. O.: Observed Relationships Between Sudden Stratospheric Warmings and European Climate Extremes, Journal of Geophysical Research: Atmospheres, 124, 13943–13961, https://doi.org/10.1029/2019jd030480, 2019.
(b) Butler, A. H., Lillo, S. P., Long, C. S., Lee, S. H., and Lawrence, Z. D.: Differences between the 2018 and 2019 stratospheric polar vortex split events, Quarterly Journal of the Royal Meteorological Society, 146, 3503–3521, https://doi.org/10.1002/qj.3858, 2020.
(c) Karpechko, A. Y., Tyrrell, N., Balmaseda, M., Charlton-Perez, A., and Vitart, F.: Predicting Sudden Stratospheric Warming 2018 and Its Climate Impacts With a Multimodel Ensemble, Geophysical Research Letters, 45, https://doi.org/10.1029/2018gl081091, 2018.
(d) Shi, Y., Evtushevsky, O., Milinevsky, G., Wang, X., Klekociuk, A., Han, W., Grytsai, A., Wang, Y., Wang, L., Novosyadlyj, B., and Andrienko, Y.: Impact of the 2018 major sudden stratospheric warming on weather over the midlatitude regions of Eastern Europe and East Asia, Atmospheric Research, 297, 107112, https://doi.org/10.1016/j.atmosres.2023.107112, 2023.

7. What is the focus of this paper with respect to ozone? Showing the contribution of stratospheric ozone to UTLS ozone and highlight its role for the radiative impact, or showing the impact on near-surface ozone, which is relevant for human health. Both aspects are mentioned here and there (near-surface more in the introduction, UTLS more in the conclusions), but I am missing the focus.

Response (7): We appreciate the reviewer's concern and are sorry for the confusion. The revised manuscript focuses exclusively on the contribution of stratospheric ozone to UTLS ozone and highlights its role in radiative impact. 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 ozone impacts.

8. In the big picture, I am missing a comparison to different regions of similar latitudes than India: Are the stratosphere-troposphere exchange processes after SSWs similar here, or is the Indian region outstanding? If so, why?

Response (8): We thank the reviewer for this valuable suggestion. The present study is focused on ozone intrusion during SSWs over South Asia and its radiative forcing. While comparison with other regions at similar latitudes could yield useful insights, it is beyond the scope of the current analysis. We will, however, consider this aspect in future work.

Specific points:

9. L72: I would not call "wildfires" or "sea ice melt" tropospheric weather phenomena, but rather consequences of these.

Response (9): Thank you for this valuable suggestion. In the revised manuscript, we have removed the wording "tropospheric weather phenomena"(L63-66).

10. L83: The word "displaced" is used to explain the "displaced case", which is a circular explanation.

Response (10): Thanks for pointing this out. The term has been removed from the revised manuscript.

11. L84: Is the term "baby vortices" really the scientific term, or rather slang?

Response (11): Thanks for pointing this out. The term "baby vortices" has been removed from the revised manuscript.

12. L124: I think this is a typo: Typically ERA5 has 137 pressure levels, not just 37.

Response (12): We appreciate the reviewer's concern. The 137 levels refer to model/hybrid sigma-pressure levels from the surface up to ∼0.01 hPa, while 37 levels refer to standard pressure levels (from the surface to 1 hPa), which are used in our current analyses. We have corrected the manuscript to explicitly state "37 standard pressure levels" (L125).

13. L143: It seems like the software has also an acronym (at least some characters in the name are capitalized). Please also give the common acronym.

Response (13): In the revised manuscript, we have included the common acronym, DISORT (Discrete Ordinate Radiative Transfer) code (L156).

14. L250: "Figure 2c-d shows clear evidence of a substantial ozone increase...": I do not agree with this statement. I think the authors cannot write about "clear evidence of a substantial ozone increase" in general. Actually, I think that only at 850 hPa one can see an increase over time. At

150 hPa, I would argue that one can see a decrease over time with an extreme outlier for 2018. Further, I think that this outlier should be discussed in detail. Further, the language should be more specific here and it should be mentioned that the discussed ozone increase is an increasFe over time over several years.

Response (14): 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, Fig. 2d (850 hPa) has been removed.

The reviewer may kindly note that Fig. 2c (in the old manuscript) does not represent a long-term increase over several years, but rather event-specific ozone enhancements linked to stratospheric intrusions during particular SSW years. However, this figure has been removed in the revised manuscript. Additionally, as suggested by the reviewer, the outlier behaviour of the 2018 event is discussed in detail in the revised manuscript (Section 3.1).

15. Figure 4: Why is 200 hPa chosen as a UTLS level, while in Fig.2, 150 hPa have been shown? Are there good reasons for that, or would it be possible to have the selection of pressure layers shown here to be consistent?

Response (15): Thanks for the valuable suggestion. To maintain consistency across the figures, we have chosen 200 hPa as a representative level for the upper troposphere in the revised manuscript.

16. L322: "Our analysis shows strong vertical coherence between 10 and 200 hPa levels": How can one observe this "strong vertical coherence" by comparing these plots? Please elaborate.

Response (16): We appreciate the reviewer's concern. 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, this sentence is removed and the section has been modified as per our revised methodology.

17. L324: Please explain "wave-1" and "wave-2".

Response (17): 'Wave-1' and 'wave-2' refer to the zonal wavenumber-1 and wavenumber-2 components of planetary-scale disturbances in the stratosphere. In geopotential-height (GPH) anomaly maps (e.g., at 10 hPa; Fig. 4a-e in the old manuscript), wave-1 appears as a single ridge–trough dipole encircling the pole. Whereas wave-2 appears as two alternating ridge–trough pairs (e.g., Severe et al., 2013). However, in the revised manuscript, we have removed this term.

18. Figure 4 nicely showed the region of interest with a box. I suggest to repeat that box for the panels of Figure 5. Further, the orientation of the maps are different between these figures. Also

in panels a-b, it would be good to mark the boundaries of this box, since the text is mentioning that region.

Response (18): Figure 5 has been modified in the revised manuscript as per new methodology. However, as suggested by the reviwer, we showed the region of interest with a box and changed the orientation of the spatial maps (Fig 5a-b).

19. Section 3.3: These radiative forcings highlight again the outstanding role of the 2018 event, which is in focus of this paper, but this outstanding role is not sufficiently commented here.

Response (19): We thank the referee for pointing this out. In the revised manuscript, we have expanded the discussion of the 2018 event in detail (see section 3.1).

20. Figure 6: I am not sure about the relevance of this figure, since it basically shows 6 numbers, which are already given in the text.

Response (20): Thank you for pointing this out. Since Fig. 6 essentially repeated the values already presented in the text, we have removed this figure from the revised manuscript.

21. Line 414: I think the 290% given here are linked to the outlying event in 2018 and should not be given here as a general trend.

Response (21): As suggested by the reviewer, this sentence in the conclusion section has been modified in the revised manuscript (L420-422).

22. L439: I do not understand the final sentence: please rephrase.

Response (22): Since our focus is on the UTLS ozone in the revised manuscript, this sentence is removed.

23. L444: The given link is not accessible to me.

Response (23): The link has been updated in the revised manuscript.

24. Figure S1: Why is this figure given? It shows very, very similar results as Figure 1, just that the climatology is not subtracted from the values. Why do the authors want to show this figure in addition?

Response (24): Thank you for pointing this out. Since our present analysis is based on anomalies derived from the composites of all non-SSW years, we have removed Fig. S1 in the revised manuscript.

25. Caption Figure S1: Panel (b) is specifically mentioned, while panel (a) is not. I guess this was just missed to mention.

Response (25): In the revised manuscript, Figure S1 is removed.

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