Review of egusphere-2025-1098:

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.

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.

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.

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.

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.

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

- Line 25: ERA-5 → ERA5
- Line 129: here you state +/- 61 days but later it is always stated +/- 60 days for the composites
- Line 141: Why are temperature, water vapor and clouds taken from the ECMWF forecast and not from ERA5?
- 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?
- 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?
- Line 152: UTLS has not yet been defined (except in the abstract)
- 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.
- Line 207: couple → couples
- Line 232: Can you explain more on which data exactly you applied the student's t-test? Some assumption must be made to apply this test where I see potential issues (normality of data, continuous data, ….)
- Line 243: DSSW → dSSW
- 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.
- Line 354ff 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.
- 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?
- Figure 5b: Again how is this related to the vortex, all I see is that there is a maximum around the SSW onset.
- Line 362: What do you mean with Rossby wave intrusion?
- 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?