

Stratosphere–Troposphere Exchange and Surface Ozone Pollution over Tropical Regions: A Case Study of Rossby Wave Breaking and Tropopause Folding

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General comments

The manuscript describes the dynamic processes that led to a well-known ozone exchange process, i.e., STE, which primarily influences mid-tropospheric ozone and occasionally surface ozone, thereby impacting air quality. The STE event was thoroughly characterized using reanalysis data and Lagrangian trajectories. This case study is of great interest to the atmospheric science community; however, the manuscript requires major revisions in terms of its current structure and concise presentation of its results form before it can be accepted for publication in WCD.

I think sections 1.1 and 2 should be better organized by distinguishing clearly between subsections for data and method. Additionally, Section 1.1 presents some results before describing the methodology for obtaining those results, which could be remedied by reordering these sections.

The manuscript contains repetitive descriptions of the same event in multiple sections and subsections (3 and 4) of the manuscript. Some of the information described in these sections could be easily included in the introduction and discussed only when needed.

Although section 3.6 attempts to provide context, it feels out of place. In my opinion, this attempt to contextualize the STE event presents an opportunity to remedy one of the study's fundamental deficiencies, namely, a more thorough elaboration of its relevance (introduction), and to extend the study's period in a more systematic way (method), to later focus specifically on the 6-14 March 2016 event.

Minor comment

Lines 38-40: This line is unclear, linking geography and local meteorology with an independent variable: emissions. I suggest rewriting this sentence.

Line 53: Could you state the mixing ratio (by volume) value of this event?

Line 57: Satellite products are mentioned in the introduction as part of the analysis; however, the satellites used are not described in the methodology.

Line 59: Clarify the term “precursor” here and throughout the manuscript.

Line 89-91: For a general scientific community, I would not recommend using local indexes such as IMECA. The message would be the same and clearer if it just utilized nmol/mol (ppbv).

Line 91: The term “anomalies” is not clear. Do you mean high values, or was the ozone anomaly calculated?

Figure 2: Note that you are using mixing ratios ppb (I assume by volume), which is the same as nmol per mol, instead of concentration. Please correct throughout the manuscript.

Line 203-204: This is just an example of many sentences that can be moved to the introduction: *“Cut-off cyclones of this type have been argued to play an important role in*

STE processes (Holton et al., 1995), as these structures can promote the irreversible descent of stratospheric air into the troposphere”.

Line 242: mass mixing ratios. Between what level pressures?

Line 281-289: If the authors want to include this text, it is first necessary to describe the methodology to estimate the tropospheric column ozone as well as to discuss the limitations of vertical resolution of satellite products, particularly in the UTLS. Consider that high elevation terrain also contributes to uncertainty. A full description of the tropopause estimation is also necessary to include when satellite products are utilized.

Line 326: Instead of “Phase I Mexican environmental contingency,” can the author just state the mixing ratio values reached at the surface level?

Line 328: Temporal precursor? In another part of the manuscript, the terms “synoptic precursor,” “synoptic dynamical precursor,” and “precursor” are used interchangeably. Can the author define the term?

Line 330: Indicate the pressure levels where those ozone mixing ratios were identified.