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

by

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Major Comments

I agree with the first reviewer that this manuscript is not suitable for publication in the state that it in at the moment. It needs to be significantly reorganised and redundant information to be removed. This constitute a major revision of the manuscript and below I present some comments for the authors to consider.

In addition to the re-organisation of the manuscript, in my opinion as reviewer, this paper does not clearly illustrate what the contribution is to the STE body of knowledge thus far. It merely describes the event but how does this event advance our understand of STE processes? This is not clear to me.

Minor comments

Lines 28-29: The authors should clarify exactly how stratospheric air ends up in the tropopause. The occurrence of a RWB (or tropopause fold) event does not necessarily mean that STE occurs, what it guarantees is that the dynamical tropopause has been lowered. If no cut-off occurs so that a blob of stratospheric air is isolated in the troposphere below the tropopause, then how we would conclude that STE has occurred. If a tropopause lowers without a cut-off then stratospheric air remains above the dynamical tropopause. Please consider this issue as I find it to be consistently misunderstood right through the manuscript (as the comment on Fig 6 will illustrate this point).

General comment on the literature review in the introduction is that whilst it is well written, it does not go far in enough in reviewing STE issues and what this work attempts to close. Therefore, review of current STE knowledge needs to be significantly improved.

Lines 69 – 96: Section 1.1 does not belong to the introduction, where we normally present current knowledge, define hypothesis etc. It should be integrated into 3.1

Lines 97 – 159: The data and methods section should only discuss the diagnostics and defer discussion about the case to the results section. For instance the discussion of Figs 2 and 3 is treated here and it should be in Section 3.

Line 136 – 137: The authors are discussing breaking baroclinic waves and therefore should refer to potential vorticity conservation instead of absolute vorticity conservation, as the latter applied to barotropic Rossby waves. Also please consider the conditions under which PV is conserved and these should be mentioned explicitly here.

Lines 151-158: Should be moved to the results where the case is described. For this plot (Fig 3), please specify the range of longitudes through which the zonal averages are calculated for the two panels.

Sections 3.1 and 3.2. Some of the issues raised in 3.1 are repeated in 3.2, so these two sections should be combined and discussed using either Fig 4 or 5 but not both. My preference is the former and the streamers diagnostics could be implemented in Fig. 2. The geopotential heights and the appearance of the COL in the traditional sense has already been covered in Figure 1. Hoskins et al (1985) should also be invoked here as the PV anomaly induces the closed COL circulation

Lines 215 – 233: Fig 6 should show the evolution of the fold that corresponds to Fig 4, so that the time lag between the PV intrusion and the steep rise in ozone concentrations may be better explained. Also this discussion should show what was raised earlier in this review and that is, for there to be stratospheric exchange, a COL should have happened. The lat/pressure plots should clearly show this.

Another advantage of presenting the evolution of these zonal profiles in that we will have a 3-D view of the event, as readers.

Lines 275 – 278: Even though intuition is forcing me to agree with this statement. Fig 8 is not showing that the air that is associated with increased ozone concentrations and the tip of the curved arrow. Reason for this is that the PV values located near the surface are less than 2PVU, meaning that they cannot originate from the stratosphere. If anything, the air there originates from below the dynamical tropopause. The authors will have to explain.

Lines 352 – 354: The authors raise the issue of the role of topography without providing an analysis on these issues. In other words there is no analysis that suggest what the role of topography might be in this paper.

Conclusions: I am afraid I find that this study does not add to the body of literature to help us understand STE better in a convincing manner. It is highly descriptive in many areas and lacks a demonstration of how this case study helps us understand these issues. In addition to this, the paper requires very major restructuring. I am inclined towards rejection of the manuscript.