

Review for

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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Summary: In their study, Lopez-Bravo et al. consider a stratosphere-troposphere exchange (STE) event over Mexico that is triggered by Rossby-Wave Breaking (RWB), tropopause folding and quasi-isentropic downward ozone transport. This STE event contributes to the background ozone concentrations in Mexico city, and the surrounding region, and contributes to the exceedance of ozone concentrations above regional air-quality thresholds.

The study does present the meteorological conditions leading to the STE event, making use of trajectory analysis, Eulerian diagnostics of tropopause folding and RWB and isentropic transport. The basic ingredients leading to the events are not novel, hence the study does **not** substantially contribute to our process-understanding of STE events. Still, I think there are also several good reasons why the study is of interest to the STE community:

1. The study clearly describes the temporal evolution leading to the downward and southward ozone transport, where it reaches tropical regions far south and thus leads to enhanced ozone concentrations in Mexico city. It is an untypical example of STE reaching the tropical region and thus is a good example of extratropical-tropical interaction. There are also some aspects untypical in this event, e.g., the persistence of the folding event. In summary, the STE evolution might not particularly novel in the processes/weather systems involved, but it clearly illustrates how they can influence tropical regions.

2. To me, the main conclusions and novel insights of the study are, therefore, not in the physical understanding of the processes leading to STE, but more on the societal relevance of events like the one discussed. This is nicely highlighted in the discussion and conclusion sections of the paper, where it is discussed how STE can contribute to enhanced background ozone concentrations in a city like Mexico city, thereby – in addition to other (industrial, combustion,...) sources of ozone – leading to concentrations above air-pollution thresholds. I am not sure whether the ‘community’ is already fully aware of this impact of large-scale-induced STE on local ozone concentrations and exceedances. In short, I enjoyed the discussion where exactly these aspects are addressed.

3. Elaborating on 2), the study also makes clear that this kind of large-scale STE events can contribute to ozone (air pollution) exceedances that are not only localized, but cover a larger region. I think it is important to make the

community (metropolitan administrations) aware of the fact that not all ozone-enhancing processes can be controlled on a local scale, but that there are larger-scale drivers that can substantially contribute to it and this on a regional scale. In this sense, the study is important that it exactly addresses this aspect.

4. In the discussion and conclusion, the authors also address the predictability aspect of ozone exceedances. The synoptic-scale weather systems involved (RWB, tropopause folds,...) can be predicted with some lead time, and thus they might contribute to warning times. In essence, if the synoptic situations favors substantial STE, the local communal administration might be warned about a higher risk that air-pollution threshold might be surpassed. It is a weakness of the study that this aspect is mentioned in the discussion/conclusion, but not further elaborated. I would have appreciated if some more concrete discussion on this predictability aspects would have been addressed. So, maybe the authors could be somewhat more explicit in their discussion on how predictable the driving weather systems are, and how this might transfer to the predictability (and warning times) of ozone exceedances.

In summary, I think the study is interesting, however possibly somewhat less because of detailed process discussions but because of the impact of STE events on regional ozone exceedances and their predictability.

I think the presentation of the processes involved is clear and their temporal evolution is well described, but at several places the discussion could be improved, sometimes also become more concise. This aspects will be discussed in greater detail in my major concerns listed below. Whereas I see a few weaknesses in the process discussion, I really enjoyed the discussion and conclusion sections, which go beyond the physical processes and address the societal relevance of such STE events.

Given the aspect mentioned before, I can recommend the publication of the study if the following (major and some minor) concerns are addressed. I also see that the authors have already revised an initial version of their manuscript, and it seems to me that they have already improved this initial submission substantially.

Major concerns:

1) The introduction (section 1) already includes everything that is needed for the paper, but while reading I sometimes missed a clear storyline. Hence, the structure of the introduction could be improved. A few specific points:

- Is L40-43 really needed in introduction? Possibly remove. But, instead what is written in L63-68 would more nicely fit into the text here than at its current later place in the text?*
- Is the anticipation of the results needed in L78-81? I would remove them, as it somewhat disturbs the reading flow.*

- The topic 'STE reaching a subtropical city' could be highlighted somewhat earlier in the introduction,
- Sometimes, some keywords are misleading (at least to me): Why 'However' in L23; why 'Although' in L56? These are minor points, but sometimes wrongly set key words can distract the reader. If a sentence starts with 'However', as a reader I am looking for the contrasting aspect, and in L23, for example, it is not immediately clear to me what the contrasting aspect is.

In brief, the authors could 'streamline' the introduction somewhat.

2) Section 2) [Data and Methods] is 'jumpy', i.e., I would reorganize it to enhance the reading flow. Some aspects are also unclear and ask for further details. Some concrete suggestions:

- In section 2.2, the tropopause is defined; section 2.7 also deals with tropopause definition.
- Would it make sense to combine 2.2 and 2.3, and thus to have all observations in one section? Possibly, 2.1 (ERA5) and the tropopause definition could be handled in one section?
- Transport modelling now comes in 2.4 (trajectories) and in 2.6 (isentropic analysis of tropopause fold transport); how about combining all transport aspects in one section?
- There is some repetition in 2.5, 2.6 and 2.7., for example, in discussion the 2-PVU contour and the identification of PV streamers (as proxy for RWB).
- L110-111: Here, it is described how tropopause folds are identified by low PV (<1 PVU) and 'enhanced O3 mixing rates'. How much enhanced? A concrete threshold is missing.
- L117-119: I couldn't follow how the 'tropospheric ozone columns are calculated'. More details are needed
- L142: How is the *intensity* of a RWB event defined?
- L142: 'identifying streamers' – really streamers or cutoffs? I would also always refer to it as a PV streamer or a PV cutoff, as streamer/cutoff might be not completely clear.
- L166-167: "Tropopause folds occur ... cross tropopause exchange". Is this statement from literature (which reference?)
- L154-156: Incomplete sentence.

3) Section 3.1) [Results] starts with a case overview, then makes a link to ozone observations and tropopause folding in section 3.1.1, and continues in section 3.1.2 (very short section!) with a diagnostic perspective on the intrusion. The next section

3.2 then starts again with a synoptic-scale evolution... Here, I think there is a lot of repetition, and these first two parts of section 3 could become much more concise. As concrete suggestions:

- *I am not sure whether Figure 1 (relative vorticity) is actually needed because later the PV charts (at several time instances) are shown to discuss the evolution of the RWB event.*
- *Possibly, the whole discussion in L177-190 could, concisely, be combined with section 3.2. to present the synoptic-scale evolution of the RWB event? For me, it would even work starting with the observations (section 3.1.1 and Figure 2), before then proceeding to the detailed discussion of the RWB event (now section 3.2 and Figure 3-5).*
- *Figure 3: In the figure caption, the left panel refers to the mean over *North America*, the right one to the mean *over Mexico*. I assume that in both panels it is the same, but once as a monthly mean and once as a specific time instance. This is misleading for the reader.*
- *It is also not completely clear what can be learned from Figure 5 compared to Figure 4. Both figures show PV at isentropic surfaces (330 and 320 K).*

The points listed above point also to some repetitive parts. In summary,, I think that the manuscript would benefit if such repetitive parts are removed. Particularly, in L305-327 the text could become much more concise, removing repetitions.

4) At several place, the arguments are not completely clear to me or remain somewhat vaguely supported by the analysis. For instance,

- In L L61-262 it is stated that the combined use of PV, N2, ozone and humidity diagnostics establishes a comprehensive framework for identifying STE. Why is it completely clear that this is STE? In L274-275 it is further stated that the amplification of the 1-PVU contour reflects STE. I assume that this is indeed the case, however, the authors should also be careful in directly linking tropopause 'distortion' to STE. STE means that air parcels actually cross the tropopause, but if the tropopause becomes distorted, it does not necessarily mean that the air parcels cross the tropopause. They might remain stratospheric, and later – when the tropopause 'relaxes' to an undistorted state – just move with the 'tropopause' while always staying in the stratosphere. In brief, some careful discussion (a few sentences) would be appropriate to clearly distinguish between STE and tropopause distortions/intrusions.

- *Where do the back trajectories shown in Figure 7 cross the tropopause? It would be interesting to see these locations marked in a map, thereby highlighting whether the tropopause crossing happens locally over Mexico City or further upstream.*

- To me, Figure 9 and its discussion in L346-354 is difficult to understand. Either rephrase in clearer way, or consider removing Figure 9. Is it really need to make the key points of the manuscript? If not, I would remove it.