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# ***Interactive comment on “Airborne observations and modeling of springtime stratosphere-to-troposphere transport over California” by E. L. Yates et al.***

## **Anonymous Referee #1**

Received and published: 15 May 2013

This study presents in situ measurements and modeling analysis of a stratospheric cut-off and a tropopause fold over California, USA. A research aircraft sampled vertical profiles by descending in a spiral motion over the San Joaquin Valley (SJV) and an offshore location. The airborne instruments measured water vapour (H<sub>2</sub>O), ozone (O<sub>3</sub>) and carbon dioxide (CO<sub>2</sub>) and additionally, radiosonde data from Oakland is used to identify stable layers. Stratospheric air is identified by high O<sub>3</sub>, low CO<sub>2</sub> and low H<sub>2</sub>O. The in situ ozone measurements are compared to ozone modeled by the Real-time Air Quality Modeling System (RAQMS) and the RAQMS combined with a Reverse Domain Filling (RDF) technique. The performance of RAQMS as well as RAQMS+RDF in terms

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of reproducing the observed ozone mixing ratios is compared. Finally, the impact on surface ozone levels and policy-making is discussed.

The most valuable aspect of this study is the data obtained from in situ measurements in complex meteorological situations. The measurements were performed at the right time in (nearly) the right place to clearly identify both the stratospheric cut-off and the tropopause fold. What remains unclear to me is the main goal of this study: is it the identification of the measured air as stratospheric, to show the relevance of CO<sub>2</sub> for identifying stratospheric air, the analysis of the meteorological situation, the comparison between the performance of RAQMS vs. RAQMS+RDF or the quantification of the impact on surface ozone and air quality? All these points are discussed but I miss convincing and novel conclusions. Furthermore, a clear understanding of the difference between central concepts such as a stratospheric cut-off, a tropopause fold and Stratosphere-to-Troposphere transport (STT) is missing. The terms are used synonymously which at the very least is confusing if not erroneous. Nevertheless, I recommend the paper to be reconsidered subject to major revisions.

———— Major comments: —————

A) STT occurs when stratospheric air crosses the tropopause and enters the troposphere. In order to talk about STT events, one thus first has to define an appropriate boundary surface, i.e., the tropopause. This can be done in many ways (Thermal lapse-rate (WMO), Dynamical tropopause (using PV), Chemical tropopause (using O<sub>3</sub> and gradients of O<sub>3</sub>)...) but without specifying what boundary surface is used, the concept of exchange is meaningless. In this study the tropopause, although never clearly defined, is regarded as some fixed height (365 hPa, p.10171, l.13, why this value?) and all stratospheric air which descends below this level is regarded as 'STT event'. A constant height (or pressure) is not a good tropopause definition because clearly, it neglects nearly all important temporal and spatial variations including the interplay between the tropopause and meteorological structures such as the jet stream and cyclones. Even though stratospheric air in a tropopause fold is often irreversibly mixed

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into the troposphere (STT) by turbulence and small-scale mixing, this is not always the case. The same applies for the air within a stratospheric cut-off - it might merge again with the stratosphere, resulting in no or very little STT occurring. It is important to investigate the transition from stratosphere to troposphere and not to take it for granted as soon as the air reaches some height level.

B) Concerning the Lagrangian analysis using back-trajectories calculated from RAQMS output, the use of the term 'origin of the STT event' for the location of the back-trajectories 6 days before initialisation is very misleading. What makes this time step so special? Why is it not 5 or 7 days? A meaningful 'origin' would be the location where the trajectories fulfill some objective and relevant criterion, e.g. cross the tropopause. Although the back-trajectories (shown in Figs. 6 and 11) provide useful information about the history of the air sampled by the aircraft, the analysis of the 'Origin of STE Encounter' is thus arbitrary in the sense that its results ('origins' mostly located over the east coast of Asia etc.) highly depend on the choice of 6 days as 'time since origin' and the sensitivity to this choice is not discussed.

C) In line with comment A), the problem of an ill-defined tropopause (as a constant pressure level) also applies to the conclusions reached in Section 3.3 (Stratosphere-to-troposphere implications). I find the choice of 'total column ozone below 365 hPa' an inappropriate measure of tropospheric ozone since any (reversible) displacement of the tropopause below this pressure level is interpreted as a transition from stratosphere to troposphere (STT) and thus as having an influence on tropospheric ozone.

D) The results from the 'Clean Continental PBL Exposure' analysis (Figs. 4 and 9, lower right) seem spurious to me. How can it be that air at 10 km height spent more than 75% of the last 6 days in a clean PBL (p.10169,l.24)? How did it get up there so quickly? What is the percentage of exposure to 'polluted' PBL? Even 40% (mentioned on p.10166,l.9) seem unrealistically high to me. And how do you explain the very sharp vertical gradients in PBL exposure between 6km and 10 km in Fig. 9 (bottom right)? What is the vertical resolution of your model in this region?

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————— Minor comments: —————

1) Page 10159, line 16: 'STT events are episodic in nature...winter-spring period': The correctness of this statement depends on what you mean by STT events and on the geographical location. The seasonal cycle of tropopause folds is shown in Sprenger et al. (2003) (doi:10.1029/2002JD002587) and the seasonal cycle of STT mass flux is presented in the STE climatology of Sprenger and Wernli (2003). A very recent STE climatology also shows the STT ozone flux (Skerlak et al. (2013) (ACPD doi:10.5194/acpd-13-11537-2013)).

2) Page 10164, line 1: I would have appreciated a short explanation/definition of the 'Lagrangian average'. Same page, line 13: As already mentioned by the editor, I would replace 'isentropic PV' by 'PV' only.

3) Page 10165, around line 18 and Figure 3: If you expect lower CO<sub>2</sub> mixing ratios in the stratosphere, how do you explain the strong increase above 350 hPa in Fig 3b)? And why does the measurement of CO<sub>2</sub> in Fig 3c) stop at roughly 400 hPa? Furthermore, I don't see a clearly reduced CO<sub>2</sub> 'between 400 and 600 hPa' in the offshore profile but only between approximately 520 and 580 hPa. Also, the H<sub>2</sub>O seems to be generally very low above roughly 560 hPa - differences above this level are not visible and the region between 400 and 600 hPa is not drier than the air above it (or it is at least not visible from the plots). In the SJV profile (Fig 3c), it is very hard to see the minimum in CO<sub>2</sub> (The curve looks quite constant, except for some very small variations and maybe a small local minimum around 630 hPa?). The (local) minimum of H<sub>2</sub>O and maximum of O<sub>3</sub> is visible at 550 hPa, as described.

4) Page 10166, line 23: What is the conclusion from the 'large-scale mixing' analysis?

5) Page 10167, line 25: What you call a 'significant amount of dispersion' looks like a quite coherent flow along the westerly jet to me. Nevertheless, the large zonal spread in 'origins' in a region of strong horizontal ozone gradients is likely the main reason for the very large standard deviation mentioned in the same paragraph. I am not absolutely

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sure I understand what you mean by 'high RDF O3 within the STT event': do you only consider the back-trajectories which have a Lagrangian average O3 greater than 120 ppbv and are sampled by the aircraft (nearest model level to flight altitude) or all the trajectories (from all vertical levels) along the flight curtain (with RDF O3 > 120 ppbv)? Also, it is unclear to me how the values of 97±7.5ppbv in the 'analysed STT' (p.10168,l.12) can then be connected to the data presented in Fig. 5. If I understood this correctly, you are referring to the episodes where RDF O3 > 120 ppbv (one episode between approximately 18.3-18.4 h and one around 19.2 h) but the average measured O3 in these two episodes seems to be less than 97 ppbv?

6) Page 10168, line 20: I would refer to this structure as a tropopause fold, rather than a stratospheric intrusion since the latter is a Lagrangian concept describing stratospheric air which entered the troposphere and the former is 'only' a deformation of the tropopause. Same page, line 27: The enhanced PV and O3 also extend below the jet core and even to the south of it.

7) Page 10171, line 7: What do you mean by 'narrow STT event'? Line 8: Please elaborate how inertial gravity waves could have contributed to the very high ozone values. Line 9: I think you wanted to refer to Fig. 7 (not Fig. 11).

8) Page 10173, line 10: The observed peaks in hourly ozone from several measurement stations might indeed be influenced by the stratosphere (best candidate is the peak around 00:00 on 6th of June in South Pass, WY, Fig. 12 b) but closer analysis of the transport of the air from the stratosphere to this station would be needed. Since the South Pass station is not visible in Fig. 7 it is not possible to assess the meteorological situation over Wyoming. Also, it might be useful to investigate why the Zion National Park station (red in Fig. 12 b) shows two subsequent peaks whereas the Great Basin NP (black) and South Pass (cyan) only show one.

9) Page 10174, line 25: I agree that 'total tropospheric ozone' (as defined in this paper) is not a very good indicator for tropopause folds, but I think the RAQMS analysis does

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a reasonable job in capturing both the cut-off and the tropopause fold (vertical cross-sections in Figs. 2 and 7).

10) Page 10175, line 6: It seems quite obvious to me that a global model with comparatively coarse resolution ( $1^\circ \times 1^\circ \times 35$  levels) cannot capture all the small-scale variations of ozone mixing ratio near tropopause folds and thus also underestimates the peak values. Why this is also the case for a nested meso-scale model with higher resolution, where for example the tropopause fold develops fully within the nested model domain, is not a priori clear to me and I am looking forward to seeing your results from the RAQMS/WTF-CHEM simulations.

————— Additional comments concerning the figures: —————

Fig. 2: Why do you put the coordinate labels in the middle of the plots (top row) and not outside? The white wind vectors are barely visible. White contours not mentioned in the caption.

Fig. 3: Image quality is not very good. In contrast to the caption, there is no change in the ozone horizontal scale.

Fig. 6: Does 'STE Encounter' accurately describe the origin of back-trajectories with RDF O<sub>3</sub> > 120 ppbv? It would be very useful to indicate the 135°E meridian in the plot above. Although clear, mention that the wind is represented by white contours. I find it questionable to project all 'origins' onto the vertical cross-section at 135°E. The 'origins' are spread from South Korea to the middle of the North Pacific and thus I would only show the 'origins' located within a reasonable distance from the chosen cross-section.

Fig. 7: As Fig. 2.

Fig. 8: Image quality is not very good.

Fig. 9: Very strange patterns (see comment D).

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Fig. 11. As Fig. 6.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10157, 2013.

**ACPD**

13, C2362–C2368, 2013

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