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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Observations of PAN and its confinement in the Asian Summer Monsoon Anticyclone in high spatial resolution" *by* J. Ungermann et al.

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This discussion paper presents the data and analysis of peroxyacetyl nitrate (PAN), retrieved from the CRISTA-2 mission on the Space Shuttle during August 1997. This dataset is one-of-a-kind, providing a near instantaneous view of the UTLS chemical structure created by the dynamics of the Asian monsoon. The authors did an excellent job integrating the chemical and dynamical information, and complementing the direct observations with modeling tools. The work makes a significant contribution to the characterization of the ASM chemical impact. Some comments and suggestions are provided to the authors for improving the clarity of the analysis and the presentation.

Major points:

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Discussion paper

1. The most outstanding weakness of the discussion paper is the section on PAN-O3 tracer-tracer space analysis, captured by Figure 9. The idea of examining the tracer-tracer space is a very good one. The specific analysis and description presented need some critical thinking.

1) The vertical range of air masses represented in Figure 9: follow your statement, the air masses you selected for this analysis are within a 10 km layer following the tropopause (±5 km around the tropopause). What is the horizontal range of the selection? Are they all from the region shown in Fig 9b? What is the altitude range represented in Fig 9b? i.e., the vertical distribution of the parcels in the O3>120 ppbv and PAN>80 ppbv quadrant? Does it justify the use of the 380K level tropopause isoline and PV contours?

2) Representation of the tropospheric branch: Based on Fig 5, sampling of the extratropical UT is very limited. That might be the main contributor to the ambiguity in identifying the tropospheric branch. It is important to look at the distribution from the specific dataset when selecting the critical values. Based on the tracer space distribution in Fig. 9a, there is no obvious reason for choosing 120 ppbv of ozone as a cut-off value for the tropospheric air. I am curious: what is the ozone distribution for parcels below the tropopause and equatorward of the subtropical jet core? Intuitively, the ozone cut-off seems to be more appropriate to be near 250-300 ppbv. The spatial resolution of the data here are significantly different from the aircraft in situ measurements. This factor needs to be considered when choosing the cut-off values.

3) Be careful and consider the difference between "the tropospheric (stratospheric) air" and "the air parcels of tropospheric (stratospheric) origin". They mean very different things. The former classifies the air mass and the branch. The latter says which layer the air is coming from but usually implies it has left that original layer. In this case (P7L32-32) I think you meant to say that the tropospheric branch is formed by tropospheric air, not by air parcels of tropospheric origin. Some clarification in wording is necessary.

4) Be aware of the difference between "where mixing occurred/was taking place" and "where mixed air was observed". You don't have enough information to prove the former so it would be more accurate to stay with the latter.

5) A similar idea but a different strategy is shown in Park et al., 2007 (Fig 9), which should be referenced here.

2. Additionally, I have minor concerns with the source region discussion.

1) You used "back trajectory falling to below 5 km" as the criterion to identify the source region. Please clarify how the terrain is treated in the trajectory calculation, since the Tibetan plateau is about 5 km in altitude. The criterion would have excluded the plateau as a source region. It would be better to use a terrain following criterion.

2) It would be useful to include a discussion of the surface wind field (southwesterly flow), which will give a perspective of the larger region that contributes to the entry point of the vertical lifting, not leaving the impression that only the southern slope of the Himalaya where the emission matters (a very small source region).

3. The paragraph starting P6L17 is somewhat troublesome. You intend to use the concept of tracer gradient to define the underlying dynamical barrier. You are struggling because the PAN-PV gradients do not seam to provide a clean result. Later you say it appears that the thermal tropopause works better than the PV contour selected from d(tracer)/d(PV) gradients. The fundamental problem is that PV itself is also a tracer and it goes through strong gradients at the barrier you are seeking, just like the chemical tracer. You will not find a strong gradient of one versus the other because they are co-varying. Please consider again the relevant information in Kunz et al. 2011a,b: 1) The tracer gradient should be calculated with respective to the latitude or equivelant latitude, and 2) at 380 K level, the separation of stratospheric and tropospheric air is better represented by 6pvu, not 2-4 pvu. If you look at Figure 4, a contour of 6 pvu may appear to be reasonably close to the contour of the thermal tropopause.

Specific wording suggestions and comments:

P3L14: “concentrate for practical matters” =>"focus on the information more relevant to this work"?

P3L18: “A simultaneous good signal-to-noise-ratio”?? => "a sufficient signal-to-noise-ratio"??

P3L25: “which could be observed” => “which was observed”

P4L2: “an aerosol underground” => “the background level of the aerosols” ??

P4L19: “the shown pressure surface” => “the 100 hPa pressure surface”

P4L31: “Shown is” => “Shown in Figure 3 is”

P5L1: “rather high in the anticyclone. . . trace gases” => “near the top of the anticyclone. The spatial extent of the anticyclone can be identified using the horizontal gradient in the trace gases at this level .”

P5L13: in this paragraph please be more specific about how many days of measurements you used to fill one day at 12Z using the trajectories. Is the filling only by forward or backward etc. Similarly, the paragraph in L25 could use some specific words like mid-point of every 3 days of measurements.

P6L12: “Figure 6c. . .” . This sentence is very confusing. It needs to be re-written.

P6L16 “tropical jet” => "tropical easterly jet" (check multiple occurrences)

P7L7: “is less good” => “is weaker”

P7L17: “in-mixing”=>”mixing”

P7L15: define “elevated”

P7L11: I think this paragraph is better to be following the Fig 5. The tracer cross sections show nicely that the tracer (PAN) is poleward bound by the westerly jet and equator-ward bound by the easterly jet, although the easterly jet is weaker.

P7L21: the elevated PAN in the 150 °E cross-section looks very similar to the 90° E cross-section in its vertical extent.

P7L16: "majority of mixing does not occur within the anticyclone" – example of word "mixing occur" being used loosely. It is highly likely the mixing among the tropospheric air occurred within the anticyclone, but that kind of mixing is not what you designed to identify.

P8L26: Add "altitude" after "parcels fell below 5 km". As commented in point 3, there needs to be some clarification on how the terrain is treated in the back trajectory calculation.

P9L11: "It represents. . ." => The CRISTA-2 dataset represents the first high-resolution global coverage of PAN at the UTSL level."

P9L31: "where the strongest mixing was taking place" -see comment 1.4)

P10L6: "The majority . . . stems from a rather small source region. . ." – see comments 2.2)

Figures: it will make it easier for the readers if the lat-lon great circles and the continents are draw with darker lines. Alternatively, change to using a different type of map projection. Current projection and the faint gray lines for the latitude circle made it very hard for the readers to compare the locations on vertical cross section (figure 5) with the maps, because the vertical cross sections extend to 60 °N latitude, the maps only show latitude labels up to 30°N.

References:

Kunz, A., P. Konopka, R. Müller, and L. L. Pan (2011a), Dynamical tropopause based on isentropic potential vorticity gradients, J. Geophys. Res., 116, D01110, doi:10.1029/2010JD014343.

Kunz, A., L. L. Pan, P. Konopka, D. E. Kinnison, and S. Tilmes (2011b), Chemical and dynamical discontinuity at the extratropical tropopause based on START08 and WACCM analyses, J. Geophys. Res., 116, D24302, doi:10.1029/2011JD016686.

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