Atmos. Chem. Phys. Discuss., 7, S7907–S7911, 2007
www.atmos-chem-phys-discuss.net/7/S7907/2007/

Atmospheric Chemistry and Physics Discussions

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7, S7907–S7911, 2007

Interactive Comment

Interactive comment on "Chemical Isolation in the Asian monsoon anticyclone observed in AtmosphericChemistry Experiment (ACE-FTS) data" *by* M. Park et al.

M. Park et al.

Received and published: 21 December 2007

Anonymous Referee #2 Received and published: 13 November 2007

We thank the reviewers for their helpful suggestions. Our response to the questions and specific suggestions are as follows:

General comments: The authors analyzed ACE-FTS retrievals of several trace gases to examine the chemical isolation inside the Asian Summer Monsoon anticyclone in the upper troposphere and lower stratosphere. They showed the spatial distributions of CO and HCN, both tracers of combustion emissions, with which (CO) they defined the boundary of the upper-level anticyclone. They then examined the vertical profiles in and outside of the upper-level anticyclone of CO, HCN, C2H6, C2H2, among other trace gases to illustrate the chemical isolation (manifested in enhanced mixing ratios inside the anticyclone). They also looked at the difference (inside minus outside) profiles of these trace gases and their correlations with CO to drive home the central point of the study. This is a very well written manuscript. It demonstrates once again that space-borne observations of atmospheric composition can provide unique insights into the underlying dynamic process and vice versa; in this case, the isolation effect of the upper-level anticyclone on deep convectively lofted trace constituents.

Specific comments: *Section 2, Data Description: The authors stated that the data used in their study is based on version 2.0 for which validation results have yet to be published. It seems to me that they need to provide a summary of the version 2.0 data, for instance, their uncertainties/precisions and any issues relevant to the present study.

The information about the version 2.2. data is included in the revised text.

*Page 5, less than 1% of profiles depends on the species. -> depending.

This is Fixed.

*Page 5, The initial comparisons of version 1.0 comparisons with what (presumably other independent data)? Consider replacing the word comparisons with validations.

This part is replaced by the version 2.2 validation information.

*Page 5, ’The estimated fitting error’ -> errors. Also, the total retrieval errors in addition to the fitting errors should be provided here

This is fixed.

*Pages 5-6, Section 3, discussions on Figure 1: How good are the ACE-FTS CO retrievals at 16.5 km altitude? In comparison with other CO measurements (say, MLS, TES), the CO values (up to 60-70 ppbv) seem on the low end. Or, did the authors saturate the colors at 70 ppbv? This may affect their definition of the monsoon anticyclone boundary.

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The actual ACE-FTS CO value goes up to 76 ppbv at this altitude, which does not affect the definition of the anticyclone boundary. The comparison between TES and ACE-FTS CO shows ACE CO is lower than TES over 10-20 km. But TES has limited sensitivity at this altitude range (Clerbaux et al., 2007). The ACE-FTS and MLS CO at 100 hPa are comparable (figures not included) though MLS CO is on the higher end (MLS CO has a known high bias of up to 200% at 215 hPa).

*Pages 6-7, Section 3, discussions on Figure 2: the authors refer to the monsoon anticyclone throughout the manuscript. I wonder if they could define the vertical extent (or, depth) of the anticyclone, either chemically (e.g., constituent concentrations) or dynamically. Also, identifying altitude ranges of high values and the vertical gradients can be tricky given the vertical resolution(s) of the ACE retrievals (3 km vertical field of view, see Section 2). The authors discussed the peak concentrations altitudes throughout Section 3 (e.g., Figures 2, 3, 5, 6, and 7) only to acknowledge (first paragraph, page 9) the relatively coarse vertical resolution(s) of the retrieval(s).

The vertical extent of the monsoon anticyclone in the upper troposphere/lower stratosphere can only be estimated from meteorological data as a ‘region of closed circulation’ over ˜250-70 hPa (10-18 km), see Randel and Park (2006). Trajectory calculations in that paper also suggest that constituents are most strongly confined over this altitude range. The observations here suggest confinement of chemical species is most significant ˜150-100 hPa where the horizontal circulations are strong. We have added additional discussion on this in the revised version.

*Pages 8-9, Section 3, discussions on Figure 5 (the same goes for Figures 2 and 3): The authors didn’t discuss as to why CO, HCN, C2H6, C2H2 concentrations peak at different altitudes other than a hint to the different lifetimes. I wonder if they would comment a bit more on that.

Within the vertical resolution of these measurements, the relative enhancements of the tropospheric tracers have a similar vertical structure (peaking near 15 km). Given the

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resolution and uncertainties in retrieval details, we choose not to focus on the relatively small differences among these tracers for this paper.

*Page 7, the 2nd paragraph, Section 3, discussions on Figure 3: I’d like to see the authors comment on the decrease of O3, HNO3, and HCl concentrations with altitude up to 13-14 km. It is one thing to see the relative minimum in ozone within the anticyclone (here, a definition, at least the one the authors have in mind, of the vertical extent of the anticyclone would be helpful), as the authors noted. Such a minimum in ozone (in the horizontal) may largely reflect the contrast between tropical and stratospheric air masses. It is yet another thing to see this kind of decrease since there is no apparent decrease in CO concentrations for the same altitude range. In convective regions (often accompanied by lightning) one would expect typically increasing ozone concentrations with altitude in the upper troposphere. Could it be heterogeneous chemistry?

The decrease in the stratospheric tracers (O3, HNO3, and HCl) seem to be related to the level of convective outflow known as around 13 km. Together with this, the maximum (minimum) tracer differences around ˜15 km suggests that the level of maximum convective outflow in the monsoon region might be higher, which is still an open question in this paper. When the stratospheric tracers decrease, we would expect an increase in the tropospheric tracers (such as CO). Various measurements of ozone vertical profiles do show a minimum in the upper troposphere. And we think that there is little possibility of the ozone heterogeneous chemistry at this altitude.

*Pages 6-7, Section 3, discussions on Figures 2 & 3: it would be helpful if the authors can indicate the (average) tropopause height, at least for the ’inside’ profiles.

The thermal tropopause height derived from the ACE-FTS temperature is added in Figs. 3a and 4a with a discussion in the revised text.

*Pages 9-10, Section 3, discussions on Figure 7: I wonder if the authors can discuss the double peaks in the difference profiles. Or maybe the double peaks need not be emphasized because of the vertical resolution of retrievals?

We think that these double peak structures need not to be emphasized given that ˜ 3 km vertical resolution.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13839, 2007.