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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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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.

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

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

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

*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.

*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

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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).

*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.

*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?

*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.

*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?

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