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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 #1

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This is an interesting, well written, and informative paper. It gives a broader chemical context for the upper tropospheric Asian Monsoon anticyclone. I certainly think it is publishable. I have, however, a few comments which I would like to see addressed in the final version.

(1) Definition of "in" and "out" of the anticyclone. Many readers of the paper will think it unfortunate that the authors did not use an independent dynamical criteria for being in or out of the anticyclone. The authors should at least give an argument as to why this is difficult to do. The lack of an independent dynamical criteria gives rise to a general uncertainty in the paper about the degree to which the chemical enhance-

ments and correlations are really due to dynamical isolation, or simply the tendency for tropospheric/stratospheric tracers to be correlated.

(2) Figure 1 shows that most of the CO enhancements are in a specific geographic region. However, there also appears to be some CO $>$ 60 ppbv values that may have been considered to be "in" the anticyclone, despite being located, for example, in the Caribean. It is not clear to me that these were excluded. So, I think, the paper should also be a bit more clear on whether or not points outside the defined Asian Monsoon region (0 - 120 E and 10 - 40N), but which had CO $>$ 60 ppbv, were considered to be "in" the anticyclone.

(3) A rather obvious and desirable reference to include, in the context of the HCl/O3 correlation, is the paper, "Quantifying Stratospheric Ozone in the Upper Troposphere with in Situ Measurements of HCl", by Marcy et al., Science, 2004.

(4) comment on page 11: "The maxima (minima) in the tropospheric (stratospheric) constituents over 13 - 15 km may suggest that this is the altitude of strongest outflow from deep convection in the monsoon region". Maybe so, but some provisos may be in order. Imagine there is a region in the tropics with enhanced deep convection. There will be an upper level export of mass in the upper troposphere from that region (measured as a dynamical divergence), arising from this enhanced deep convective outflow. However, some of the convective outflow occurring within the region will be internally compensated within the domain by radiative descent. Due to the weakness of radiative cooling rates above 14 km (due to low water vapor mixing ratios), the fraction of the convective outflow that is compensated by nearby radiative descent will be much smaller at 15 km than at 12-13 km. Therefore, the dynamical divergence will tend to peak at an altitude above the convective divergence. It is the dynamical divergence that contributes to the larger scale export of tropospheric tracers to the background atmosphere from a convective region. In a sense, the stiffness of radiative heating rates in the TTL means that you get more bang for your buck, in terms of export of tropospheric tracers, from a convective outflow at 15 km than you do at 13 km, since radiative ver-

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tical motions at 15 km are so slow. Convective outflow at 13 km is balanced, to a relatively greater degree, by a convergence in the near field clear sky radiative mass flux. Due to the role played by radiative subsidence, diagnosing convective outflow from enhancements of tropospheric tracers is a bit problematic. More details are in the paper, Folkins, I., S. Fueglistaler, G. Lesins, and T. Mitovski, A Low-Level Circulation in the Tropics, accepted for publication in J. Atmos. Sci., May 2007.

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