Atmos. Chem. Phys. Discuss., 7, S7900–S7903, 2007
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Atmospheric Chemistry and Physics Discussions

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7, S7900–S7903, 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 #1 Received and published: 5 October 2007

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

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

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it unfortunate that the authors did not use an independent dynamical criterion 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 enhancements and correlations are really due to dynamical isolation, or simply the tendency for tropospheric/stratospheric tracers to be correlated.

We used CO criterion because of the simultaneous measurement of each of the species available from ACE together with the observation that CO at 100 hPa is high primarily in the region of the anticyclone. To further quantify this relationship, we have added a scatter plot of Ertel’s potential vorticity (EPV) calculated from the NCEP/NCAR reanalysis data with CO as an additional independent dynamical criterion (Figure 2 of revised version). There is an overall correlation between EPV and CO measurement and the high CO (≥ 60 ppbv) is correlated with relatively low EPV, which is characteristic of the anticyclone (except lower EPV at the tropics, which is far from the anticyclone).

(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 Caribbean. 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.

Only the points lying between 0°-120°E/10°-40°N are considered to be inside of the anticyclone, and this is now explicitly mentioned in the revised text.

(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.

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This is included in the revised text.

(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 vertical 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.

We have included this as a possible mechanism for the maximum tracer differences being at higher altitude rather than at the level of convective outflow in the revised text. Overall we think the mechanism(s) of transport above deep convection are still an open question.

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

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