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Comment

## ***Interactive comment on “Greenhouse gas relationships in the Indian summer monsoon plume measured by the CARIBIC passenger aircraft” by T. J. Schuck et al.***

### **Anonymous Referee #2**

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This paper presents an analysis of greenhouse gas measurements in the upper troposphere over the region of the Asian monsoon anticyclone, obtained from CARIBIC observations during summer 2008. This region is of interest because recent satellite observations show persistent enhanced levels of tracers during summer, linked to upwards transport in deep convection and confinement by the strong anticyclonic circulation. However, there have been few direct, in situ observations of tracers in this region (from either balloons or aircraft), and the CARIBIC measurements make a novel contribution to documenting and understanding the behavior of this region. This paper highlights the observations of greenhouse gases, and a companion paper will focus on shorter-lived hydrocarbons and other species. Overall these CARIBIC observations

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are a valuable new contribution, and the paper is interesting and appropriate for ACP. However, I also think that the paper is much too long (37 pages of text) and some sections are largely qualitative and hand-wavy. Detailed comments follow:

1) The key results here show an enhancement in CH<sub>4</sub>, CO, SF<sub>6</sub> and H<sub>2</sub>O associated with the anticyclone (often termed a 'plume' in the paper), consistent with previous satellite observations (SF<sub>6</sub> is new). The latitudinal and temporal behavior of the relative maxima are reasonably consistent among these different tracers. There is also a relative minimum in ozone, anticorrelated with H<sub>2</sub>O (again, consistent with satellite observations). Variations of CO<sub>2</sub> are somewhat more complicated to interpret, because of strong seasonal variations in surface CO<sub>2</sub> amounts. All of these results are straightforward and interesting. The shape and position of the plume (Section 4.4) is interesting, but the analysis should be interpreted in light of the limited CARIBIC aircraft sampling (effectively several cross sectional snap-shots across the anticyclone).

2) I find the detailed discussions regarding enhancements in the plume (Section 5) and estimates of emissions (Section 6) to be much longer than necessary, and I suggest these sections be reduced. I like Figure 8, and much of the information on enhancements can be directly derived from there. However, the lengthy discussions on small details (such as the few April measurements discussed on p. 21) are less important, and take away from what could be a very concise summary. Also, Section 5.3 (Structure of the Plume) attempts to derive much detail, but ends up with speculation regarding mixing, etc. While the trajectory analysis in Fig. 9 is interesting, these results for one flight may be difficult to generalize to the entire anticyclone structure (and the discussion comes across as speculative). Perhaps such analysis synthesizing results for many flights could be the subject of a future paper. Likewise, I find Section 6 to be extremely qualitative and based on speculative assumptions. I think a more rigorous error analysis would result in very large uncertainties for emission estimates, given the uncertainties in the observed correlations (Fig. 10) and lack of knowledge of pathways and origins of air in the anticyclone, plus emissions for SF<sub>6</sub>. These uncertainties are

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acknowledged several times in the text, but not quantified. This section tends to ramble with far too many details (aspects of crop emissions from different countries, etc.), and this takes away from the quantitative focus of the first part of the paper. Overall I think the paper would be much improved by shortening or eliminating Sections 5-6, so that the resulting paper would be much more focused and quantitative.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2031, 2010.

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