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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increasing synoptic scale variability in atmospheric CO_2 at Hateruma Island associated with increasing East Asian emissions" *by* Y. Tohjima et al.

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Thank you very much for your comments and suggestions on our paper, which are very useful for revising the manuscript.

Reply to General Comments: Comm#1. It is very understandable that the referee would like to see the actual data measured at HAT and compare them with the data measured at other remote locations, like MNM or MLO. However, for the accurate comparison of those growth rates we need to describe more details about our measurements and, in particular, stability of our CO2 standard scale. Actually, another paper about continuous CO2 measurements at NIES monitoring sites, including comparison

of mixing ratios between NIES and NOAA GMD network, is now being prepared (H. Mukai et al.). Since our paper just focuses on the synoptic-scale variability, we would prefer not to add the actual data in the revised manuscript.

Comm#2. The increase in China emission is about 800 Tg-C for the period 1996-2006, while the GLOBAL ship emission is increase by about 40Tg-C ($\sim$150 Tg-CO2 for the same period; see Eyring et al., Fig. 6). Assuming percentage of the ship CO2 emission occurring in East Asian region is about 30%, which roughly corresponds to the percentage of East Asian (China+Japan+Korea) fossil CO2 emission to the global total, the increase in ship emission from East Asia is at most 12Tg-C. In addition, since ship emission allocations are distributed over the Pacific region, the ship CO2 emissions that really influence the observation at HAT are much less. Therefore, taking the uncertainties in our estimation into account, the influence from the ship emission would not be captured by this method (as is the case for biofuel; discussed by Review#2).

Reply to other comments of a substantial nature:

-Strictly speaking, we measure CO2 and CH4 mole fraction or molar mixing ratio, not volumetric mixing ratio (ppmv). In fact, the CO2 and CH4 standard scales are based on the gravimetric gas mixtures (mixtures of weighted gases of CO2 or CH4 and purified air), which give absolute molar mixing ratios not volumetric mixing ratios. In addition, IUPAC recommends use of SI units in atmospheric measurements, in this case u(micro)mol/mol for CO2 and nmol/mol for CH4. (More details are in Pure and Applied Chem., 67, 1377-1406, 1995 or Tellus, 44B, 1-2, 1996). The former and later units are often abbreviated to "ppm" and "ppb", respectively. In the revised manuscript, we will replace "concentration" with "molar mixing ratio" and state that "ppb" is an abbreviation of "nmol/mol" and so on.

-In response to the referee's comment, we will replace "trends" with "increases".

-We will add continuous line numbers in the margins of the revised manuscript.

-Fossil carbon emissions from cement production are not included in the EIA emissions for China, Korea, and Japan. So, we added carbon emissions from cement production in the CDIAC dataset to the EIA dataset. This is stated in 3. Results, 3rd paragraph, 4th last sentence of the original manuscript. To clarify this, we will add this in the figure caption of Fig. 4 in the revised manuscript.

-In the trajectory calculations, the NCEP reanalysis data with time resolution of 6 hours, latitude/longitude grid of 2.5 degree and 17 pressure levels are uses. We will add above sentence in the revised manuscript.

Reply to Minor Comments

We would like to thank the referee again for his comments. Most of the suggested texts will be accepted in the revise manuscript. Following are some replies that we need to explain: (Note that the page and line numbers in the parentheses below refer to the manuscript published on the ACPD website.)

Page 2 (page 15726, line 26): As the referee indicated, the fossil carbon emissions from CDIAC was updated through 2006. We will use the updated emissions in the revised manuscript. Consequently, we will change Table 1 and the relevant discussions (page 15731, line 14-26) in the revised manuscript.

Page 6, first paragraph (page 15729, line 23-26): The sentence "The leading edge of the CO2 SSV ... around the site" will be changed to "The positive _CO2 values are produced mostly when the air mass is directly transported from the source regions, while the negative_CO2 values are associated with the air mass that travels longer over the ocean, circling clockwise around the site."

Page 8, first sentence of Discussion (page 15732, line 6-10): Since we show an example of how El Nino events influence the wind and radiation in Fig. 6 in the original manuscript, we don't consider a reference is needed here.

Page 8, beginning of Discussion (page 15732, line 6-10): The sentences "As for an

example, the differences in the CO2 and CH4 mixing ratios at surface, wind vectors, and outgoing longwave radiation (OLR) between the periods of December 2001 to February 2002 and December 2002 to February 2003, respectively, are depicted in Fig. 6. Note that the distributions of the 3-monthly values in Fig. 6 are simulated by ACTM" will be added in the revised manuscript.

Figure 4, caption: As is described in Response to other comment, carbon emissions from cement production in the CDIAC dataset are added to the EIA dataset.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 15725, 2009.