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Interactive comment on “Diagnosing recent CO emissions and springtime O₃ evolution in East Asia using coordinated ground-based observations of O₃ and CO during the East Asian Regional Experiment (EAREX)2005 campaign” by H. Tanimoto et al.

H. Tanimoto et al.

Received and published: 24 June 2008

We thank the reviewer for positive comments and valuable suggestions. Please see our detailed responses below.

Comment 1: It would also be better to add discussion/information of air-masses, which are influencing the observations at these 8 surface sites.

Reply: Detailed discussion of transport paths and mechanisms from Asian continent to individual sites were made in our previous paper (Sawa et al., 2007). Hence, we do not

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think it appropriate to repeat the discussion again in the present paper. We cite Sawa et al. (2007) where it is appropriate.

Comment 2: Very brief information may be provided about the range of correlation values among different ozone analyzers.

Reply: One sentence is now added in section 2.2.

Comment 3: "We examined ... Sawa et al. (2007)." This paragraph can be moved in section 6.1.

Reply: The paragraph is now moved to section 6.1.

Comment 4: It might be more appropriate to add some discussions on observed versus modelled ozone and CO data at "individual" sites. It is appearing to me that CO comparison shows more scattered values at Gosan (GSN), Fukuejima (FKE), and Amami Oshima (AMA) and these three sites might have more/direct influence of East Asian outflow.

Reply: The main purpose of Figure 4 is to look at general agreement between the observed and modeled data. Discussion on individual sites is not made with Figure 4 but with Figure 8 (section 6.2). Brief description on all the sites is now added in section 6.2.

Comment 5: Figure 4 may be drawn using binned average value to add more visibility in this figure.

Reply: We see that this is another good way to express the figure. We intended to show how the model can reproduce the observations on 3-hr basis, and hence keep the figure as it is.

Comment 6: Instead of good correlation, it might be more appropriate to say a positive correlation.

Reply: This is modified.

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Comment 7: "...at several sites...". It is better to provide numbers of sites.

Reply: This phrase is clarified (six sites).

Comment 8: It would be beneficial to reader to provide the information about the approach/method used to estimate the "enhancement in ozone and CO". Additionally, it is better to write "enhancement in O₃" and "enhancement in CO", apart from writing "delta O₃" and "delta CO" in caption of Figure 7.

Reply: Brief description on how to determine enhancements of O₃ and CO (delta O₃ and delta CO) in individual episodes is now added (section 6.1). Caption of Figure 7 is modified.

Comment 9: I feel, transport of ozone rich and CO poor air, probably from higher altitude, could also lead to show increase in ratio of delta O₃ and delta CO.

Reply: CO of stratospheric origin should have very low concentrations (e.g., tens of ppbv). However, CO levels observed here are not that low. Its impact should thus be much smaller than from anthropogenic sources.

Comment 10: Section 6.2 and Figure 8 - I feel that there is a better positive correlation with lesser scatter at Cape Ochi-ishi (COI). Some lines may also be added on this.

Reply: Brief explanation for this better correlation at COI than GSN, AMA, HAT, and YON is added (see Reply to Comment 4).

Comment 11: Figures 3 and 8: It would be more appropriate to compare observed and modelled data both using 3 hourly data.

Reply: Figure 3 and Figure 8 are modified to compare 3-hourly data.

Comment 12: Figure 8: If all eight figures are not on a single page, a label on X-axis (CO ppbv) may be added.

Reply: Figure 8 is now corrected for axis-labels.

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Comment 13: Table 1: Four values in "Inverse model" are mentioned in centre column (between Anthropogenic and Biomass). It is confusing. If they are total emissions, pl mention accordingly.

Reply: Footnote is now added in Table 1.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 8, 3525, 2008.

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