

Interactive comment on “Global estimates of CO sources with high resolution by adjoint inversion of multiple satellite datasets (MOPITT, AIRS, SCIAMACHY, TES)” by M. Kopacz et al.

M. Kopacz et al.

mkopacz@princeton.edu

Received and published: 12 January 2010

Response to Reviewer 1: We would like to thank the reviewer for the positive and insightful comments. Our responses to the comments are listed below.

Comment 1. Section 2: While the paper gives the equations used to describe the relationship between the retrieved CO and the true profile, the times of retrievals that each instrument uses is not mentioned. It should be pointed out that MOPITT and TES (and maybe SCIAMACHY) use the optimal estimation technique described by Rodgers (2000), while AIRS uses a rather different retrieval (see, e.g., Warner et al., 2007, which you already reference). I think it is important to note this. While the paper concludes

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

MOPITT and AIRS are generally consistent, the different retrieval procedures could explain some differences.

Response: We now state it explicitly that MOPITT and TES retrievals use the Rodgers [2000] optimal estimation technique, whereas AIRS uses a different retrieval.

Comment 2 p.19978: Please clarify whether or not the anthropogenic a priori emissions have seasonal variation.

Response: We now clarify with the following statement: “The a priori anthropogenic sources other than Streets et al. [2003] in China do not include seasonal variability. A recent update of the Asian inventory (not used here) does include seasonality [Zhang et al., 2009].”

Comment 3 p.19979; Fig.5: Seems like it would be better to show a full year of TES data, such as the same time period as used in Fig. 6.

Response: We changed Figure 5 to match the time period used in Figure 6.

Comment 4 p.19983: What is used for the model error? Or do I misunderstand? When you say ‘We estimate the latter ...’ it sounds like you refer just to the representation error, but maybe you mean you use RRE to determine the observational error matrix? Please clarify.

Response: We now clarify the error calculation to read: “We estimate the total observational error with the Relative Residual Error (RRE) method [Heald et al., 2004; Kopacz et al., 2009; Palmer et al., 2003].”

Comment 5 TES is included in the title of this paper, and promoted in the abstract as being used for the evaluation of the a posteriori emissions, however, all that is given is a very brief paragraph on p.19992. A figure showing the results of the comparison (e.g., scatter plot as in Fig. 6) would be useful.

Response: We now elaborate on our TES comparison. We added a scatter plot fig-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

ure (Figure 10), similar to Figure 6, which shows model-TES CO column correlations (for 2004–2005 period) using model a priori and a posteriori concentrations. Our TES comparison paragraph now reads: “Since we did not use TES CO data in the source inversion, we use it as an additional independent set of measurements to verify our a posteriori results. Figure 10 shows a priori and a posteriori global TES correlations against GEOS-Chem for the 2004 - 2005 period. The a posteriori correlation coefficient is $r = 0.91$, same as the a priori, but the slope of the regression line increases from 0.89 to 1.04, indicating a better fit.”

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19967, 2009.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)