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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CO emission and export from Asia: an analysis combining complementary satellite measurements (MOPITT, SCIAMACHY and ACE-FTS) with global modeling" *by* S. Turquety et al.

Anonymous Referee #2

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The paper of Turquety et al. reports about an interesting study on atmospheric carbon monoxide (CO), an important air pollutant, focusing on Asia which is a region known for significant CO emissions. The authors have taken advantage of using CO measurements from several satellites and they used a global chemistry/tranport model to assess its quality and to interprete the satellite measurements. The paper gives a comprehensive overview about the different measurements and their characteristics and also points out several features, uncertainties, limitations, and possible inconsistencies, which makes an interpretation quite a challenge. This is partially resolved by

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using a model as a kind of transfer standard but on the other hand this introduces new uncertainties as the model also has its characteristics and limitations, e.g., with respect to its resolution and the assumed emissions. In my opinion the authors made a good job here and discussed and presented the method and the results relatively clear. In summary, I conclude that the paper covers an important topic and is very well written. I therefore recommend to publish it in Atmos. Chem. Phys. Below some mostly minor comments which should be considered for the revised version of the paper.

Abstract:

First sentence: The term "available satellite observations" may be a bit misleading as it can be misunderstood. It may give the wrong impression that the authors have used "all available" satellite data. That the authors have used available data is trivial. I suggest to replace available by, for example, "several complementary satellite observations" or equivalent.

Page 1719, line 10:

It is written that CO over low clouds is included but that this does not result in a bias. I would assume that if part of the CO is missing (the CO below the cloud) that this should result in too low columns, i.e., a low bias. Please clarify.

Figure 1: Unit of the emissions: Is it per year and per 3.75 deg x 2.5 deg grid cell ? Please clarify.

Figure 2: The annotation is quite small (essentially unreadable in the printed version).

Figure 4: Three of the four maps shown in the first two rows show quite high CO over India, except SCIAMACHY (top right). What is the reason for this ?

Figure 7: The correlation between MOPITT and the model looks suprisingly good. It this primarily because of topography combined with sampling ? Please clarify.

Figure 9 and discussion on page 1726: I am not convinced that "good consistency" is

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an appropriate summary of the FTS and model comparison shown in Fig. 9. Apart from biases also the latitudinal variations seem to be quite different. I recommend to clarify this by providing, for example, correlation coefficients.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1709, 2008.

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Interactive Comment