**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on "Error correlation between CO$_2$ and CO as constraint for CO$_2$ flux inversions using satellite data" *by* H. Wang et al.

H. Wang et al.

hwang@cfa.harvard.edu

Response to Interactive comment on "Error correlation between CO2 and CO as constraint for CO2 flux inversions using satellite data" by H. Wang et al.

Anonymous Referee #1: This manuscript describes the use of both CO and CO2 satellite observations in an inversion study to estimate CO2 fluxes. The idea is to have a stronger constraint by exploiting the model transport error correlations between CO2 and CO. Both the NMC method and a comparison between different models are used to estimate these error correlations. This is in principle a nice idea that could give indeed a stronger handle on the important transport model errors. The manuscript is well written, easy to read, and the figures are clear. The paper provides a nice first look at the use of the proposed method, but will need further research to really show the

full potential. However, I recommend publication of the paper in its current stage (after some minor adjustments outlined below). At the same time, I would like to encourage the authors to fully exploit the potential benefits of this method by performing a more extensive study. Specific comments

Page 11785, lines 2 - 6: The sentence about OCO is not really relevant for the paper. I would recommend to remove it.

We mention OCO here since we use OCO averaging kernels later in the paper.

Page 11785, first paragraph: Please, acknowledge some other work as well here: Chahine et al., GRL, 2005, doi:10.1029/2005GL024165 and Engelen et al., JGR, 2009, doi:10.1029/2008JD010739 for AIRS; Frankenberg et al., JGR, 2006, doi:10.1029/2005JD006235 for SCIAMACHY.

We will add these references.

Page 11786, line 3: Please, also cite Clerbaux et al., 2009, ACPD, 9, 8307 for the use of IASI to estimate CO.

We will cite this paper.

Page 11787, line 22: The references to Engelen et al. seem to be referring to the same paper.

Engelen et al. [2006] contains correction to Engelen et al. [2002].We will cite Engelen et al. [2006].

Page 11787, equation 3: Should the errors not be added quadratically?

This equation sums the errors, not the error variances. We will add a sentence that the error variances add quadratically (if the errors are independent).

Page 11789, line 1: Can you really assume that the representation errors are independent for CO2 and CO? I would think that these errors depend on things like model grid

size and horizontal concentration gradients, which are not that different for CO2 and CO.

This may indeed be the case, though the representation error is in general very small. We will change the text to "Since the instrument errors for CO2 and CO can be assumed independent, and the representation errors can be assumed to be small [Heald et al., 2004].

Page 11790, lines 10 - 11: Please mention that the infrared allows observations twice daily, which is one of the main assets of this type of observation.

We will mention here that infrared instruments can measure on both the day side and the night side of the orbit.

Page 11792, lines 3 - 4: This is not an obvious conclusion. Why would the correlations resulting from the differences between 2 models for 2 sets of models be the same? What do the actual model error variances look like for the 2 sets of models? Are the highest CO2-CO error correlations found for areas where the variances are small or large? Is there any relationship? The authors should elaborate a bit more on this, i.e., why are the correlation patterns so similar.

On Page 11791, line 25, we will add "Regions of strong model error correlations include but are not limited to regions of strong model error variances." We willl add in line 4 on Page 11792 "The robustness of error correlation patterns indicates that the directions of general gradients of column CO and CO2 are similar between the two sets of models.

Section 5: I am a bit concerned that everything is done with basically the same model. The estimated error correlations are based on the GEOS-Chem model (although with different meteorological input) and the flux inversion is also done with GEOS-Chem. This means that both S and K are based on the same model, which might make the results a bit optimistic compared to the 'real' world. It would be good if the authors could comment on this in the text and do some more rigorous inversions in a further

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

study.

On page 11794, line 21, we will change the last sentence to "Our demonstration involves several simplifications such as neglecting instrument and representation errors, neglecting spatial and temporal correlations, neglecting a priori error correlations, and using the same model for both pseudo data and inversion. These simplifications may influence the benefits of the joint CO_2-CO inversion. A more extensive study will be needed to better understand their effects." In addition, on Page 11796, line 8, we will replace "demonstrated" by "illustrated".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11783, 2009.