

Interactive comment on “Atmospheric column-averaged mole fractions of carbon dioxide at 53 aircraft measurement sites” by Y. Miyamoto et al.

Anonymous Referee #2

Received and published: 26 December 2012

General comment:

This is a well-written paper presenting a valuable new data set which would be of general interest and to both the remote sensing community (for validating satellite measurements of XCO₂) and scientists working with tracer transport in atmospheric modelling. The authors present these data clearly, and discuss their methodology for extending the measured partial column to the full column in order to have XCO₂. It would be very useful if this dataset were made available for the use within the scientific community.

I recommend publishing the manuscript after taking into account the following com-

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ments.

Specific comments:

1. How does uncertainty in the height of the PBL from NCEP GFS affect the overall error estimation? There are some studies that address this, and comparisons could be made at sites where radiosondes are also available. The importance of the tropopause height is addressed (or at least was in comparisons with rawinsondes in Araki et al. (2010)), but the importance of PBL height isn't, although you state that the uncertainty associated with the lowest part of the column dominates the total uncertainty.

2. Is it possible that the reference atmosphere (CIRA) used for the dry air number density doesn't perform as well in other regions as it does at Tsukuba (where Araki et al. compared it to rawinsondes)? Is there another dry air number density that could be tested to estimate the size of this uncertainty? This might be of more relevance for other gases (see comment four).

3. GOSAT is discussed at great length in the article. Why not show comparison with GOSAT data for 2009? Or at least give an idea of how many GOSAT measurements coincide with the 53 sites in a given year (using the matching criteria of Keppel-Aleks, as used in Wunch et al. 2010 for instance). This would help give an idea of the utility of the dataset for GOSAT (and other satellite) validation. Or is this beyond the scope of the article? Nonetheless, it would be relevant to introduce what needs to be done to compare the aircraft-derived XCO₂ with the GOSAT measurement, taking into account the averaging kernel of the spaceborne measurement?

4. Finally, the very last sentence suggests that the approach could be easily extended to methane using the tracer-tracer correlation method. However the stratospheric contribution of methane to the total column is much more uncertain than it is for CO₂, given the structure of the stratospheric sink (see Geibel et al., ACP, 2012). Here the CIRA might be insufficient.

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Technical corrections:

page 28495, line 4: remove "of" page 28495, line 5: remove "equipments" and "the"
page 28497, line 2: change "closed" to "close"

Figure 4: There are too many plots here. Could a few representative sites be chosen (as for Figure 5) and the rest placed in supplemental material? Furthermore, the black symbols showing the uncertainty of the measurements makes it difficult to see the values in blue, and make it look at first glance as if the data are much noisier than they acutally are. Could these values not be plotted as error bars instead? Or is this just too messy?

When the values for the different error regimes are introduced, the rationale needs to be introduced as well, or at least the reader needs to be directed to section four where they're discussed in greater detail. Otherwise the numbers seem too arbitrary, "based on data comparisons" until the careful reader gets to the relevant section. Unfortunately not all readers are so careful.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28493, 2012.

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