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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of a new middle-lower tropospheric CO_2 product using data assimilation" *by* A. Tangborn et al.

Anonymous Referee #1

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This paper describes an evaluation of a new middle-lower tropospheric CO2 product with data assimilation method. Since satellite CO2 observations are averaged either over partial column (e.g., AIRS CO2) or over the total column (e.g., GOSAT Xco2), it is challenging to evaluate it with conventional flask observations, which are normally point observations. By ingesting column CO2 into model using data assimilation, it is possible to indirectly evaluate the column CO2 with flask observations. This methodology has been demonstrated by previous studies, such as Engelen et al., (2009) and Liu et al., (2012). The study described in this paper uses different data assimilation methodology and evaluates a different AIRS CO2 product. Overall, the study shows the value of data assimilation in evaluating column CO2 data. However, there is some deficiency in the setup of data assimilation, and confusion in the description of results. Some of the description about carbon cycle is inaccurate. I would suggest major revision before

publication. Below are my detail comments.

Major comments:

1. Data assimilation methodology

a. In this paper, the forecast error variance is set as a constant percentage of the local mixing ratio. The coefficients are function of latitudes. This setup has a big flaw since summer CO2 value is smaller than winter CO2, but this does not mean summer CO2 has smaller error. On the contrary, the variability of summer CO2 is normally larger than winter. As a result of this setup, it is possible that the impact of observations becomes smaller during summer, especially over land. I would like the authors to comment on the impact of seasonally changed CO2 value on the forecast error variance, and its impact on the assimilation results.

b. Lines 145-146 mention that the correlation length varies with latitude and direction. I would like the authors to describe specifically how the correlation length changes, and the rational behind the setup.

c. What is the observation error variance used in data assimilation? And where is it from?

2. Discussion of results

a. The paper validates the results against surface flask observations at 6 sites shown in Figures 4 and 5. Could the author comment on why these 6 sites were chosen? and what the results look like at other surface flaks stations?

b. The time periods chosen to generate different plots are very confusing. In evaluating against the NOAA aircraft observations, the authors use the whole assimilation period (year 2005 and 2006) (Figures 7 and 8), while when evaluating against INTEX-B campaign, the authors use Feb-May 2006 (Figure 9), even though the campaign is from Jan-May 2006. Then in discussing why the comparisons against NOAA aircraft and INTEX-B campaign are different, the authors use March-August (2005) (Figures 11 and 12). I would suggest the authors to state why only Feb-May 2006 is used in evaluating against INTEX-B campaign. It would be clear if the authors could add in one plot that shows the comparison of the results with NOAA aircraft observations for the same time period as INTEX-B campaign. For Figures 11 and 12, I would suggest the authors using the same time period as INTEX-B, then you can state why the performance are different between the these two comparisons.

c. Why figure 13 uses the time period of July 2005- June2006? If there is no specific reason, I would suggest the authors to use the whole year of 2006. If there is specific reason, please state in the main text. Also, I suggest the authors comparing the column CO2 over North America to the TCCON CO2 observations (http://www.tccon.caltech.edu/). d. Figure 4 shows that the assimilation in general has improved the agreement with surface flask observations. But at SPO site, after the first 6 months, the CO2 from assimilation run is much lower than the flask observations. I would suggest the authors to comment on this, since this change is not minor.

3. Description about carbon cycle

a.The statement in the introduction between lines 32-33 is not well supported. It states that : ". . . can be used to produce maps of the global CO2 concentrations, which may eventually be used in "inverse" model applications to infer surface fluxes". There is no study so far that uses CO2 maps generated from data assimilation in inversion.

b.In lines 157-159, the authors briefly describe the surface flux forcing. I would suggest the authors to describe the surface flux forcing more in details, such as the magnitude of annual fossil fuel emission, ocean flux and the total net flux for these two years, since this will impact the explanation of the results. The results clearly show that the model simulation has a low bias compared to observations, which may be due to the magnitude of fossil fuel emissions used being low.

c. In lines 176-178, the authors state that : "In the SH, where there is little seasonal cycle in CO2, the improvements due to the assimilation are particularly important be-

cause the initial difference between the model and observation is only about 1ppm". The connection between the importance of the improvement and the amplitude of seasonal cycle is not clear.

d. In lines 328-329, the authors state that assimilation is beneficial "even when using a model that uses incorrect source-sink distributions." It is easier to have positive impact especially when the surface flux forcing is not that correct. I do not think it is appropriate to have this statement here since the surface flux forcing may be one of the reasons for the positive impact.

4. Figures

a. I would suggest the authors plotting Figure 1 in log-scale. b. Figure 6a is too vague.

Minor comments

1.I would suggest the authors to drop the reference to Imbiriba et al. (2012), since it is in preparation.

2. In lines 126-127, it states: "This results in a state dependent error covariance because the error standard deviation satisfies the constituent advection equation." The error covariance is state dependent simply because it is proportional to time-changing CO2 values.

3. In line 188, it says "8 CCGG sites". It should be 6 CCGG sites.

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