

## ***Interactive comment on “Carbon source/sink information provided by column CO<sub>2</sub> measurements from the Orbiting Carbon Observatory” by D. F. Baker et al.***

**D. F. Baker et al.**

Received and published: 20 March 2009

We appreciate the referee’s comments, particularly regarding the length of the document being a deterrent to potential readers, and we have made a good-faith effort to reduce the content and discussion to a bare minimum in the revision.

Replies to Specific comments:

*1)The weekly state vector seems strange given the proposed 16-16-day duty cycle of OCO. The authors have neglected this duty cycle, instead opting to consider either nadir or glint measurements (Table 1).*

Yes, we have disregarded the nominal 16-day nadir / 16-day glint duty cycle to make

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a point: in terms of spatial coverage, it makes sense to use a shorter flux span. We have used a 7-day period, and defended it with the argument on page 20057, based on coverage of the ground tracks. Flux uncertainties for this span should be comparable or slightly higher than those obtained for the 8-day span used in Chevallier, et al (JGR, 2007), given the fewer data per flux interval. If the OCO team were to go with a glint-only mission, for example, there would be no particular reason to base the flux spans on the 16-day orbit repeat cycle. Even if the nominal alternating nadir/glint operations plan, based on the 16-day cycle, were to be used, using either an 8-day or 4-day flux span would get more detail out of the measurements than using the longer 16-day span: the greater coverage for the longer span is likely worth less than the added value of examining the variability in the shorter spans.

*2)The introduction is too long and needs to be more focused on introducing the study.*

We have shortened the Introduction in the revised version, and focused it more closely on our study.

*3)Page 20054, line 16. Clouds will only decrease the accuracy of the  $X_{CO_2}$  retrievals if they are not properly characterised.*

It is true that one could attempt to model the radiative transfer effects of the clouds properly, and use cloudy scenes. All attempts to do so using real data have failed, as far as we know, at the sub-1 ppm accuracy level. Because of that, the strategy of identifying cloudy scenes, and avoiding them, was chosen for OCO. To reflect this, we have modified the sentence on lines 14-17 to: "OCO's field of view (FOV), 2 km on a side, was chosen that small on purpose, to increase the chances of seeing through holes in the clouds (Crisp et al., 2004). Radiative transfer modeling errors for cloudy scenes generally result in large  $X_{CO_2}$  errors; nominally, these scenes are discarded."

*4)Page 20054, line 22. GOSAT measures at thermal IR wavelength so it will measure at night.*

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That is true, but the thermal IR band will provide little information on CO<sub>2</sub> sources and sinks then. To be more precise, we have reworded the sentence on lines 22-24 to: "Neither mission provides much information on the diurnal cycle of the CO<sub>2</sub> sources and sinks, since their near-IR measurements are sampled only in the early afternoon.

*5)Page 20056, line 10-14. Do the authors mean they average the measurements over the grid box?*

The measurements themselves would be averaged, yes, with a weighting proportional to the inverse of their individual uncertainties. Since we are dealing with measurement uncertainties only in this simulation study, an equivalent uncertainty is calculated in a similar manner, with correlations accounted for by computing the equivalent number of independent measurements inside each grid box.

*6)Page 20056, line 15. Mie scattering?*

To include all possible cloud-related errors, we will change "radiative transfer errors due to scattering" to "associated radiative transfer modeling errors".

*7)Section 2. Can the authors clarify the local time of OCO? 1.30 is very close to Aqua.*

The key requirements for OCO's ascending node time were, first, that it be at least 3 minutes earlier than Aqua, to prevent interference with that A-train satellite, and, second, that it be later than 1:25pm to prevent interference with Terra (in a morning sun-synchronous orbit, but crossing the afternoon sun-synchronous A-train orbit near the poles). The ascending nodes of the A-train satellites drift due to sun-moon interactions and are kept within a certain range with periodic maneuvers. Aqua's ascending node is currently around 1:40 pm but is on the late edge of its control box and will soon be maneuvered to go towards an earlier crossing. The 1:30 ascending node time for OCO safely satisfies these two considerations.

*8)Page 20058. Why downgrade the met fields?; this will increase model error.*

The met fields are reduced in resolution mainly to allow the simulations to be run faster.

While it is true that this increases model error, this is more of an issue when doing an inversion with actual data rather than when doing a simulation study. Inversions done with real data should be run at the finest resolution possible, computationally.

9)Page 20059. *Can the authors explain the major differences between the variational approach used in the paper and the one used by LSCE.*

In the paper, we have said: "It is similar to the "4-D Var" methods used in numerical weather prediction, except that instead of optimizing an initial condition (the atmospheric state) at the start of a relatively short assimilation window, we optimize time-varying boundary values (surface CO<sub>2</sub> fluxes) over a longer measurement span. Baker et al. (2006b) outline the mathematical details... ". Given the space constraints, it is probably not appropriate to go into more details on the differences between our method and other specific approaches.

We do not have a good understanding of the details of the LSCE approach. However, we can outline one main difference in this response. The LSCE approach, as we understand it, takes, for input, time-varying 3-D CO<sub>2</sub> fields estimated across short assimilation windows as part of routine ECMWF analyses, driven by OCO data, data from other CO<sub>2</sub> sources (including other satellites), and other meteorology data. Surface CO<sub>2</sub> fluxes are then computed from these over longer spans, using prior estimates of CO<sub>2</sub> fluxes from fossil fuel, the oceans, and the land biosphere. In contrast to this two-step approach, our method solves for the fluxes across a longer time window directly from the OCO  $X_{CO_2}$  data, albeit without the benefit of the other data used in the ECMWF analyses.

10)Page 20062, line 5. *How was the imprecise estimate constructed? Looks like a smoothed version of the control estimate.*

The imprecise estimate was based solely on the a priori flux estimate (i.e. with no knowledge of the true fluxes, as would be the case in reality), using the magnitude of those fluxes and their variability from month to month.

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11)Page 20065. Can the authors clarify the "track-to-box" representation error.

On lines 18-24, we wrote: "The first two of these error sources have been examined by Corbin et al. (2008). They did detailed simulations of  $X_{CO_2}$  variability inside domains of 1x1 and 4x4 deg using a mesoscale atmospheric transport model, comparing the  $X_{CO_2}$  averages along an OCO-like FOV ground track to the average values across the full domain to obtain estimates of the track-to-box representation errors. They also simulated the effect of clouds on the availability of OCO retrievals, coming up with realistic estimates of the along-track representation errors. "

The "track-to-box" representation error is the standard deviation of the full along-track average minus the average of the full box (either 1x1 or 4x4 deg). The name "track-to-box representation error" is assigned to this error in the sentence before it is described.

12)Page 20070. I am confused why the authors increase the  $X_{CO_2}$  error associated with aerosols, say, and then increase the measurement error variances assumed. Surely they should increase the (unknown) error on the measurements and use an assumed error covariance? Again, page 20076.

For each error type examined, we have assumed that we know such an error is likely to be present, and have increased our measurement uncertainties to reflect that. (We may not have done this in a perfect manner, since some of the error sources are biases rather than random, but increasing the uncertainty in this manner is a reasonable way to attempt to handle an error source – see Chevallier, et al, GRL, 2007).

I think the reviewer is suggesting a type of experiment that we did not test: what is the effect of adding an unknown error, which is then not reflected in the assumed measurement uncertainties. There are probably errors that would affect OCO or other missions that we do not know about now, and that would be well-simulated in this manner. However, for the error sources we have addressed here (aerosol biases, transport model errors), we are well aware that they exist and we are certainly entitled to try to mitigate their effects as best we can. We do this here by increasing the measurement uncer-

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tainties to account for them. This will certainly be done, too, in any operational system processing the real data, if/when it arrives. So that is how they ought to be simulated, as well.

*13)Page 20073. Assuming only glint measurements is fine for a sensitivity experiment but unrealistic for a control experiment.*

We are not sure what the reviewer is trying to say here. Is he/she suggesting that the control experiment must be some combination of both nadir and glint measurements? We have chosen to treat nadir and glint measurements separately, and not include a nadir/glint combination, in order to save space. It is not difficult to imagine that the results of the combined nadir/glint case would fall in between the other two.

*14)Page 20075. The authors appear to be bogged down in irrelevant detail.*

Though we are not sure why the reviewer feels the mistuning effects discussed here are irrelevant, we will take the hint and scale back the discussion.

*15)Page 20084. Transport error can be estimated from tracer transport model spread only if it is calibrated to the truth.*

We are not sure how such a calibration should be done. This calibration step has not been done as part of the TransCom 3 tracer transport intercomparison project, for example, and yet the model spread obtained there is often used as a proxy for model-truth transport errors. Reviewer 2 seems to agree with us that this approach is reasonable, and would like us to add that such an effort is underway as part of the Transcom project, in an effort led by Dr. S. Maksyutov.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20051, 2008.

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