

Interactive comment on “Interpreting the variability of CO₂ columns over North America using a chemistry transport model: application to SCIAMACHY data” by P. I. Palmer et al.

Anonymous Referee #2

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This paper compares simulations of column abundances and column-integrated volume mixing ratios of CO₂ over North America with similar quantities derived from SCIAMACHY measurements. Notwithstanding the imminent launch of purpose-built CO₂ satellites such comparisons are still highly interesting since, if we can learn something from them, we get several years extra information on the carbon cycle. By using tagged tracers, the paper also decomposes the contribution of various sources to column-averaged mixing ratios in North America and makes some comment on the detectability of various source components from these measurements.

I believe the paper contains the material to make a useful contribution to the analysis

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of modelled and measured CO₂ distributions but I do not believe the authors have brought the threads together yet. I see three significant problems in the paper which I will address in decreasing order of seriousness although not in chronological order through the paper.

The authors make a comparison of measured and modelled CO₂ distributions, focused mainly on column integrals or averages. They show that these differ between the SCIAMACHY and GEOS-5-CHEM cases. This, however, is where they stop. As readers what are we supposed to make of such differences? Much has already been written about whether the SCIAMACHY CO₂ data can provide much biogeochemical information. Does this comparison help? One gets a sense from the paper that the authors are uneasy about the measurements but the reader is left to second guess the authors. If the authors feel they cannot make any meaningful comment on the comparison they should explain why but the paper would certainly be much enhanced if this were possible.

My second concern is the choice of study period and location. Firstly this is made without comment in the paper. From a distance it seems a strange choice. The immediate benefit from even difficult satellite data is its coverage. In principle at least the comparison could have been made in a region without the density of surface measurements. The detailed comparison is made at few sites even within the well-sampled North American region so the existence of validation data does not seem a strong motivation. Furthermore the study period, 2003 predates the advent of really useful validation data in the form of the Park Falls FTS. These disadvantages were surely as clear to the authors as they are to me and so I'm sure there were good reasons for the choice. The paper will look more convincing if these reasons are explained.

Finally I doubt the value of the comments on the observability of fluxes. As the authors note, this requires an inversion study and the estimate of some kind of posterior uncertainty. Quite a few studies have attempted this already. I doubt this study can contribute much new with observations from a limited domain and without an estimate of posterior

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covariance. One potentially novel point might be the influence of the detailed structure of the SCIAMACHY averaging kernel compared to the simple structures used in earlier studies but my overall suggestion would be to suspend this analysis until the inversion framework is in place.

Minor technical Corrections:

In the following "->" means "should be changed to".

7341 Observating -> Observing

7349 ecwmf -> ecmwf

7350 "We also note that during mid-summer when the biospheric uptake of CO₂ peaks the model CVMR and surface CO₂ concentrations converge, reflecting the increasing influence of land biosphere on the lower tropospheric column." But at peak draw-down I would also expect the largest vertical gradients as noted in studies using vertical profiles.

7349 I wouldn't show column abundances for the comparison

7352 Parks Falls -> Park Falls

7354 Note that the Jacobian is a function of the state space, I presume here one calculates the sensitivity of CVMR to scaling factors for the various sources, not the detailed structure of these sources.

7356 "Prescribed error covariance matrices describe only the random ..." This comment is correct but often misleading. Too many people interpret "random component" to mean the uncorrelated component. The prescribed error covariance is quite capable of capturing correlations at any space and time scale arising from, for example, spectroscopic uncertainties. The authors are right to contrast these with biased residuals (either in fluxes or data). the sentence would be clearer if the word "random" was replaced with "unbiased".

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7356: "source and sinks." -> sources and sinks.

7356: "owing to spatial nature" -> "owing to the spatial nature"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7339, 2008.

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