Atmos. Chem. Phys. Discuss., 15, C2365–C2369, 2015
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Atmospheric Chemistry and Physics Discussions

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15, C2365–C2369, 2015

Interactive Comment

Interactive comment on "Sensitivity analysis of the potential impact of discrepancies in stratosphere–troposphere exchange on inferred sources and sinks of CO_2" *by* F. Deng et al.

Anonymous Referee #1

Received and published: 7 May 2015

This paper endeavors to characterize the impacts of atmospheric transport errors on CO2 surface fluxes inferred from GOSAT data using the GEOS-Chem transport model. The work does this by propagating a CO2 'adjustment' imposed in the Arctic tropopause region through the GOSAT inversion and examining the change in inferred surface fluxes relative to a baseline inversion. The magnitude and vertical location of the adjustment is set by comparing GEOS-Chem simulation with HIPPO measured CO2 and O3 in the high latitude UT/LS, where sizable profile discrepancies are noted, presumably as a result of excess model mixing. To isolate the transport error impact on CO2, the model O3 field is constrained by assimilation of stratospheric O3 obser-

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vations, and the resulting CO2/O3 tracer correlations are used to infer a pan-Arctic CO2 error relative to the HIPPO correlation. A second sensitivity run adds a partially compensating adjustment to CO2 in the tropical/subtropical NH upper troposphere, again in the direction of observed HIPPO discrepancies. The results are consistent with what might be expected: if you put a sink in the high latitude UT, then the inferred surface sink is diminished relative to the baseline inversion and the inferred tropical source increases. Adding the second tropical source adjustment brings the inversion back closer to the baseline but perturbs the seasonality somewhat. The bottom line of the paper is that mixing-transport errors in the UT/LS matter for surface flux inference, and that these errors result either from erroneous large-scale dynamical balance or numerical errors.

The paper is interesting. It addresses an important topic with a novel approach and the authors have done a lot of work for it. The difficulty with the paper is that the approach doesn't really test the sensitivity to the problem in question. The basic question is how much do errors in transport affect inferred flux distributions, in particular well known errors in strat-trop exchange in models driven by assimilated winds. Further, do they affect inversions based on column CO2 data differently than those using surface data? Sticking a CO2 sink in the Arctic UT/LS is not really testing the sensitivity to transport error. A few points deserve consideration:

The purpose of the adjustment is to estimate what the inversion would do if the model did not have excess mixing. Since there is no net source/sink in the UT/LS, the simulation including the offsetting tropical source is the more representative surrogate distribution for a model with better mixing characteristics. It should be featured. A better test would be a flux-balanced adjustment (as the text recommends on P. 10830). This is model land: do it.

The magnitude of the Arctic CO2 adjustment is not small (p. 10828, line 13). 0.13 PgC/mon would be 1.56 PgC/y, which is more than half of the global residual land sink or greater than the US fossil fuel emissions for 2010. It is not surprising that the

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perturbation shows up in the CO_2 column a long way from its home after a few months, particularly in a model with excessive isentropic transport. Discussion and figures in Section 3.2 are only loosely related to main point of paper.

Ideally, one would run the same inversion with different transport fields that vary in some known fashion with quantifiable errors. This has proven difficult over the (TransCom) years and that is why this paper retains interest. The one clean test that can be made, and which will answer one of the possible root causes of transport error, is to run the transport at higher spatial resolution. Likely both numerical and dynamical errors contribute. Previous studies have shown that UT/LS tracer gradients can be improved significantly by going to finer resolution than 4 x 5 (Strahan and Polansky, 2006; Considine et al., 2008). Do the transport at higher resolution, answer the question (hopefully), and drop the speculation from the discussion.

One aspect of the analysis where the paper really misses an opportunity is relating the flux sensitivity differences to the baseline posterior error estimates, which are not given at all. A key question is whether the error covariances are adequately scaled to include transport uncertainty in the posterior flux uncertainties. This aspect should be worked into the paper. Similarly, some indication should be given as to how the prior uncertainty estimates (P. 10822-10823) influence the posterior fluxes. It may turn out that the UT/LS flux adjustment does not change the posterior fluxes beyond their error bars, in which case, the basic conclusions would have to be revised, but we might feel more confident of our flux calculations and their uncertainties.

Finally, P. 10819, line 7-8 promises a discussion of the implications of this work, but the Conclusions section mostly just reiterates what has been done and said above. There is very little here, or in the abstract, to say what the implications are for source/sink inference with GEOS-Chem and GOSAT beyond some speculation about the root causes of the transport discrepancies. Addressing the comments above should give the paper more impact.

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Minor Recommendations:

P. 10815, line 5: sub ‘whose representation in models is’ for ‘which are’.

P. 10815, line 12: ‘use’ for ‘used’.

P. 10815, line 13, 15: Reword ‘correction’. This exercise establishes an error magnitude and location, but it’s not really a correction. Maybe ‘adjustment’ or ‘error’.

P. 10817: need references for Lauvaux and Davis, and Parazoo et al.

P. 10819: it would probably be worth upgrading to a more recent version of the ACOS GOSAT product that includes glint and high gain data. Flux sensitivity to UT/LS transport may well depend on data coverage.

P. 10825, line 27: ‘altitudes’ should be ‘latitudes’?

P. 10828, line 23 ff: The paper ‘would expect a negligible change in the flux estimates. . . in the SH.’ This may or may not be, as a 0.2 ppm perturbation might have a significant impact on flux in the region of small variability dominated by relatively small ocean fluxes. The point is that transport errors may impact distant fluxes especially as the run progress beyond a few months. Revise, delete, or run it out for year or so.

P. 10831, line 1-4: This reasoning does not make sense to me. Seems like balancing the high latitude sink is the least arbitrary way to test the impact of transport mixing error.

P. 10833, line 24 ff: Numerical scheme and resolution are separable issues in model formulation but here they are intermingled. Clarify discussion and its point.

P. 10834, line 21 ff: There is a fairly rich literature on the subject including O3 and other tracers including CO2 from the ER-2 and balloons in the UT/LS that could be explored to address some of these questions (before calling for more measurements).

Recommended References: Evaluation of near-tropopause ozone distributions in the

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Global Modeling Initiative combined stratosphere/troposphere model with ozonesonde data, Considine, DB; Logan, JA; Olsen, MA, ATMOSPHERIC CHEMISTRY AND PHYSICS, Volume: 8, Issue: 9 Pages: 2365-2385, 2008.

Meteorological implementation issues in chemistry and transport models, Strahan, S. E.; Polansky, B. C., ATMOSPHERIC CHEMISTRY AND PHYSICS, Volume: 6 Pages: 2895-2910, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10813, 2015.