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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Carbon source/sink information provided by column CO_2 measurements from the Orbiting Carbon Observatory” *by* D. F. Baker et al.

Anonymous Referee #2

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The paper investigates the errors of flux retrievals to be expected when inverting OCO column measurements of CO2. It uses newly available error characterizations that are more detailed than those available in previous studies of this type. The authors performed inversions on synthetic data and thus quantified the errors by comparison to the known correct answer. They conclude that errors will be sufficiently small that OCO-based CO2 flux estimates will give information on the carbon cycle not yet avaliable at present. In particular, they make the suggestion that the best gain of information would be obtained if OCO would operate in glint mode all the time.

The paper is relevant because a good insight into the information content of the data

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Interactive Discussion

Discussion Paper

is absolutely needed to be prepared for the actual start of measurements. The used synthetic inversions are an appropriate method, and described well. I recommend the study for publication in ACP. The accessibility of the paper will profit, however, from providing much less detail in some places, given that the tests are still rough measures of the real uncertainty; see below some suggestions where the paper could be shortened. Further, it would have been desirable to use an inversion setup identical to one envisaged to be used for the real data (see below). Also, the important question remains not really answered whether other error sources may be equally or more important, and how general the answer is to other inversion approaches, which limits also the conclusion of suffiently small errors.

Detailed comments:

p. 20058 l. 11-12: Shouldn't the a-priori correlations reflect assumptions on the fluxes, rather than properties of the data (at least in theory)?

p. 20062 l. 1-6: I was wondering whether this choice of P_0 is actually appropriate for these tests. Shouldn't you use the same choice that you will use later with the real data? Otherwise, the same errors/biases of the data will lead to different errors in the fluxes between the tests and the real retrievals (same issue for the measurement uncertainly, p. 20068 l. 24ff).

p. 20072 l. 1-8: I'm a bit concerned about how specific the values of this error measure are with respect to the particular choices of prior flux and the flux generating the pseudo-data. The error reduction will be large where the two involved models happen to be particularly different, which is however not a property of the satellite information but rather by chance. Wouldn't absolut error measures (like in Fig. 13) be more relevant throughout, given that biases are the most important issue? In this context: Wouldn't it make sense anyway to use at most a very smooth prior (not a detailed one from models), as the fine-scale flux structure may be hoped to be contained in the data?

ACPD

8, S10590–S10594, 2009

p. 20072-20073: This is a long discussion on the convergence. I'm however not convinced about the value of showing and discussing results that have not yet reached the cost function minimum, and would suggest to omit them (which would shorten the paper, incl. Fig 8). The authors could still make the point that land fluxes alone could be obtained in an shorter minimization, by actually performing the experiment they outline in p. 20074 l. 1ff, and just citing the number of iterations they needed, compared to the standard run.

p. 20083 l 15ff: The results are given for using OCO data only. In the current discussion, often a combination of satellite and in-situ data is envisaged, with the hope to reduce biases. If you do not agree to such a combination, can you comment on why not?

Figs 10 and 11: These figures seem to convey the same message, except for minor details. I suggest to move one of them to the supplement (incl. simplifications in the text).

Smaller comments:

p. 20053 ll. 2-16: A few references would be in order.

p. 20054 l. 14: briefly explain terms like 'inclination'

p. 20055 l. 4: 'We use a tracer transport model...' - also mention the inverse framework, otherwise it sounds like a forward study

p. 20058 l. 27: 'XCO2 measurements' should only be used for real data; use a term like 'pseudo-data' or 'synthetic data' instead.

p. 20059 l. 4-5: mention that the cited studies use in-situ data, not satellite data.

p. 20059 l. 20: Roedenbeck (2005) involved daily (not monthly) fluxes.

p. 20060 l. 1ff: The authors discuss that their numerical method (iterative minimization) was less appropriate than another numerical method (ensemble Kalman smoother).

ACPD

8, S10590–S10594, 2009

Though the Kalman method indeed has the advantage of not requiring an adjoint, it is actually much less efficient, because it involves an effective number of model runs that is not only determined by the number of ensemble members (which is roughly equal to the number of iterations of the iterative method), but is multiplied by the number of Kalman steps within the assimilation window. For a satellite inverison, the assimilation window certainly needs to be long, given by the time it takes to transport surface flux signals into the higher atmosphere which is slower than horizontal transport. A Kalman smoother with an assimilation window as short as 10 weeks (while Law, Atmos. Chem. Phys., 4, 477, 2004 suggests at least half a year even for surface data) would already need 10 times more CPU time (assuming a Kalman step of 1 week). Moreover, it would intrinstically yield an approximation only, while the iterative method can be converged to full accuracy (provided the adjoint is exact). Therefore, the iterative method chosen by the authors seems to me the more advantageous one.

p. 20062 l. 10-12: It seems that rectification between the diurnal cycle of the fluxes and that of the transport leads to another error source that should be mentioned.

p. 20069 l. 20-21: Explicitly define the terms ‘precision’ and ‘accuracy’, because not everyone may share the same concept (or even be aware of common connotations).

p. 20075 par. 2: As a comment, larger a-priori sigmas are generally expected to lead to slowlier convergence, because the effective number of degrees of freedom is larger.

p. 20076 l. 21-26: There does not seem to be much additional information in the second aerosol test. I suggest to only show one.

p. 20080 l. 8ff: How do the a-priori uncertainties compare between this study and Chevallier et al.? Can these differences explain the differences in results?

p. 20083 l. 5ff: I was surprised that you fear the system to become computationally infeasible. Wouldn’t e.g. 200 iterations always suffice but still be fully managable?

p. 20084 l. 5ff: The ongoing TransCom experiment on exactly this aim (lead by S.

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8, S10590–S10594, 2009

Maksyutov) needs to be mentioned here.

Minor corrections:

p. 20059 l. 23: ‘solving’

p. 20061 l. 10: ‘interpolated’

p. 20081 l. 20: ‘expected to be’

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

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8, S10590–S10594, 2009

Interactive Comment