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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 #3

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1 General comments

This paper describes a series of sensitivity experiments about the inversion of CO_2 surface fluxes from the forthcoming OCO retrievals of X_{CO2}.

The first noticeable feature of the paper is its length. The authors have had little success in synthesizing their thoughts and often got lost in side details. They also could have made better use of their 40 references.

Second, and related, the effort was arbitrarily focused on some parts of the error budget while nearly forgetting about the model error. What would be the point of the six pages

of Section 2.5 if the transport error was to dominate the error budget?

Last, some parts of the data assimilation system were left crude, even though it was created more than two years ago. The first weak point is the adjoint model, the approximations of which make all the iteration-dependent results (Fig 9 and related conclusions) of little interest. The second weak point is the diagonal covariance matrices that prevent the authors from achieving their 'perfect model experiment', despite their claim.

These issues dramatically limit the information content of the paper. A shorter, focussed and more balanced version of this paper is needed.

2 Specific comments

p.20053, l.20: the continental scale can also provide insight into flaws of the carbon models.

p.20053, l.22: the mixing errors affect the continental scales as the regional ones.

p.20055, l.3: 'quantify' is very optimistic.

p.20056, l.5: there are both improvements (those noted indeed) and steps backwards (the 'perfect model' assumption in the reference run, and the loss of accuracy in the computation of the error reduction). Which effect dominates?

p.20056, l.9: is the 'full-physics' system used here the prototype for OCO level-2 data or will another algorithm be used?

p.20057, l.16: strictly speaking, relating the definition of the state vector to the observation system is not correct.

p.20057, l.22: such an assumption is part of the prior information.

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p.20058, l.17: is the CFL criterion satisfied with such a long time-step?

p.20058, l.23: the remark is meaningless since the convergence rate also depends on the optimality system and on the minimization algorithm. The only relevant verification of the accuracy of an adjoint model is against its tangent-linear model. How different is the authors' computation of $(Hx)^T Hx$ from that of $x^T H^T Hx$?

p.20059, l.11: it may be relevant to mention where the computational problem lies. It is not in the linear equations themselves, but in the estimation of the Jacobian matrix of the transport model.

p.20059, l.15-18: the authors seem not to include the initial CO_2 concentrations in the state vector, which would not be correct.

p.20059, l.28: the authors should make it explicit that they are talking about the posterior error covariance matrix.

p.20060, l.1: the variational and the ensemble methods do not necessarily achieve the same level of accuracy for a given computational effort.

p.20061, l.10: interpolate*d*.

p.20061, l.19: the reason given is not appropriate. Physically-based differences may lead to anything resembling or not the prior errors.

p.20062, l.4: the authors should be more explicit. Do they take the grid-point annual statistics of the differences or something cruder?

p.20062, l.11: the argument does not hold: what about aggregation errors?

p.20062, l.18: the flat mass-weighted average may be adequate for the apparent-optical-path-difference OCO product.

p.20062, l.19: do those numbers correspond to measurement error or to observation error (with the transport and the representation errors included)?

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p.20063, l.6: this argument may not hold for the model error.

p.20064, l.18: 'striking' is a strong word for a feature that was expected.

p.20065, l.15: the distinction between along-track and track-to-box errors is rather artificial in this context. In both cases, it is a sampling problem.

p.20066, l.10: the choice of the prior or wrong error statistics may also influence the correlated errors to a large extent.

p.20068, l.8: statistical correlations do not make the errors deviate from these so-called 'extremes'.

p.20068, l.13: missing word.

p.20068, l.25 and following ones: what the authors do is not clear.

p.20069, l.3: Why do the authors assume diagonal covariance matrices? Is this valid in space in the real world? Is this valid in time?

p.20069, l.8: This statement is not correct since the space-time correlations of the prior-truth differences are not taken into account. The so-called 'perfect model' experiment is rather the first 'mistuned experiment'.

p.20069, l.15: Same.

p.20069, l.29: do the true fluxes have errors?

p.20071, l.9: the authors shift day and night, which may not be a good choice.

p.20071, l.12-end: at last, the transport error is considered, but in a crude manner compared to the details of the retrieval uncertainty.

p.20071, l.15: why this factor? The results are about the tenth of the ppm. Is this realistic? Is there any reference that shows that the authors' model is so accurate?

p.20071, l.25: what would an alternative goal be?

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p.20072, l.3: why was the study restricted to one year?

p.20073, l.2: should we have expected another behavior?

p.20073, l.20: the increase of the land error illustrates the fact that the experiment is mistuned.

p.20074, l.24: the authors had to do it but did not.

p.20074, l.26: too many words.

p.20075, l.12-19: the explanation is wrong: the convergence should not depend on the 'true' error statistics, but only on the assigned ones.

p.20076, l.8-10: is this realistic?

Section 3.4: It is difficult to read Figs. 10-11. Further, the results could be condensed.

p.20078, l.16: mistyped word

p.20079, l.19: not really.

p.20083, first paragraph: the conclusion is pointless. The authors should first improve the accuracy of their adjoint model.

p.20083, l.18: 'certainly' can be removed.

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

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