Atmos. Chem. Phys. Discuss., 3, S1040–S1043, 2003
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Atmospheric Chemistry and Physics Discussions

ACPD

3, S1040–S1043, 2003

Interactive Comment

Interactive comment on "CO_2 flux history 1982-2001 inferred from atmospheric data using a global inversion of atmospheric transport" *by* C. Rödenbeck et al.

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Received and published: 13 July 2003

This paper presents inverse model estimates of global CO2 sources/sinks for the 1982-2001 period using in situ atmospheric CO2 measurements form the NOAA/CMDL measurement network. The study builds on a series of inversion studies that have made use of the CMDL dataset to derive information on CO2 source/sink patterns.

This is a very good study. The main strength of this study is that it is a very systematic. There is a careful selection of measurements to insure against potentially problematic biases in results due to the changing nature of the measurement network. In addition the chemical transport model used in the inversion is driven with interannually-varying meteorology, and the model is sampled in a temporally-consistent manner with the

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measurements. Finally, the inversions are carried out a high spatial resolution, thereby minimizing the effects of aggregation error in the inverse results.

I do however have some concerns with the study as presented. These are detailed below:

1) The difference between the results of this study with similar previous studies is not well presented. It was difficult for me to assess to what extent the results of this study differed from previous inversions, both qualitatively and quantitatively. A table summarizing the results and perhaps plots showing differences with similar previous studies would be extremely helpful. The authors have close connections to the other groups who have performed similar studies, so it should not be difficult to obtain these results. It is also important that at key points in the text (in Section 4.10, where only a note saying compare to Bousquet et al. 2000 is included; and in Section 7 where major findings are discussed) references be provided to previous work along with a discussion as to how exactly the results of this study agree/differ with previous studies. This will clearly identify what is a new finding, and what is a confirmation or disagreement with a previous finding.

2) In a similar vein to point 1 above, a clear demarcation should be drawn between this study and the study by the same authors in Tellus (55B, 488-497, 2003). It should be clearly stated as to the extent to which this paper presents new work and new findings as opposed to what has already been published in the Tellus paper. To the extent that there is duplication, material that has already been published should be culled from this paper so long as the readability of the paper is maintained.

3) From a methodological perspective, when all is said and done the specification of prior errors is ad hoc. This is particularly true in terms of the methodology used to specify the model representation error (section 2.2.3, footnote 6). For example, the justification given for dividing by n instead of the square root of n (footnote 6) is ad hoc and not based on statistical theory. Similarly, there is no clear justification given

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as to why one should believe that the structure of the model error is as specified in the study (i.e. as the standard deviations calculated from surrounding grid boxes every two consecutive model time-steps). Clearly, this approach gives results that seem to be on the whole physically realistic. But is that sufficient to accept the results? I would very much like to see the effects of using a different error pattern on the results as part of a sensitivity study. Perhaps the forward model runs from the various groups in the TRANCOM effort (I believe the authors have access to these) could be used to develop alternate error patterns, which could then be used in the inversion.

4) There is very little discussion of the a posteriori error covariance matrix, which is a key component of the inversion result. A detailed analysis of the full (i.e. not just of the diagonal variance terms) a posteriori error covariance matrix must be presented.

5) While I understand the justification for not considering measurements from other networks or for stations that do not have a homogenous record, these additional measurements that have been withheld from the inversion should be compared with the model results using a priori and a posteriori fluxes.

6) Could the authors also provide some justification as to why the ship-based Pacific Ocean measurements (POCN and POCS) are not used in this study?

7) While the sensitivity run c shows that some aspects of the inversion are not too sensitive to the specification of fpri,NEE, I was wondering whether a similar statement could be made with respect to the coherence between flux anomalies and climate/ biomass-burning emission anomalies fire counts for this simulation

8) The coherence between flux anomalies and anomalies in fire counts is not well presented. Some discussion of the fire products should be presented including a discussion of potential biases (e.g., night time fire observations) in the context of their use in this study. In addition, it is not clear to me as to why the inversion results are only shown for 2 of the cases, and why the coherence seems to be stronger in Central America for the time-correlation case.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2575, 2003.

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