

Interactive comment on “CO₂ flux history 1982-2001 inferred from atmospheric data using a global inversion of atmospheric transport” by C. Rödenbeck et al.

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We would like to thank Prasad Kasibhatla for his constructive comments.

Ad 1) Previous studies on interannual inversions (Rayner et al., 1999; Bousquet et al., 2000) only published land and ocean totals (or fluxes from selected regions). We agree that a comparison of these land and ocean totals is indeed interesting, and included it into the manuscript. Though an analysis at higher resolution would be interesting as well, a comparison with unpublished results would be outside the scope of this study. However, a comprehensive comparison is anticipated in the framework of the TransCom3 project. For long-term mean values, comparison with results of the inversion study by Gurney et al. (2002) is provided in Table 7.

Ad 2) The previous Tellus paper is a methodological study, focussed on the effects of

using interannual winds and the ‘real sampling’. In the present study, more refinements of the method were adopted (grid-scale flux resolution, data selection), and the focus is on the resulting interannual fluxes, their robustness, and biogeochemical implications.

Ad 3) We fully agree that the choices of error structure, etc., are assumptions based on reasoning, not on compelling evidence (see, e.g., Section 4.1.8, formerly 4.7). To deal with this problem, the approach adopted here is to compare with a set of sensitivity cases. As for the specification of concentration uncertainties, a new sensitivity case is now discussed, based on the spread in transport model responses that were run for the TransCom3 project, as suggested by the referee.

Ad 4) Information on the a-posteriori covariance structure is indeed some further helpful diagnostic (compare also P. Bousquet’s comment). As the full covariance matrix is huge, we tried to distill some relevant features (covariances of long-term mean fluxes at the resolution of the TransCom3 regions) and added them into the manuscript.

Ad 5) The use of independent sites as a check of the solution is a very good suggestion. We added such a comparison and discussed it in a new subsection.

Ad 6) As the records of POCxxx contain several longer gaps, they did not meet the employed homogeneity criteria.

Ad 7) and second part of 8) The robustness of the flux estimates at the regions of Figure 19 (formerly 16), and the robustness of the coherence, is now investigated in more detail, by adding further relevant sensitivity cases (as far as readability of the figure allowed). The behaviour of the time-correlated inversion in Central America and Indonesia is indeed counter-intuitive, as now discussed in a footnote.

Ad 8) The comparison between the flux estimates and fire counts presented here is clearly preliminary. As it just recently became available, we added the quantitative estimates of biomass burning fluxes of the Global Fire Emissions Database (GFED, version 1) into the comparison. We are very interested to collaborate with groups

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investigating biomass burning in order to improve this comparison.

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