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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of CO_2 fluxes estimated using atmospheric and oceanic inversions, and role of fluxes and their interannual variability in simulating atmospheric CO_2 concentrations" *by* P. K. Patra et al.

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General comments

This paper presents results from a number of atmospheric inversions for CO_2 using the NIES/FRCGC transport model. The analysis is focussed on three areas: (a) comparison with fluxes estimated from an ocean inversion, (b) demonstration of the impact on the estimated fluxes of using interannually varying meteorology in the inversion and (c) using the estimated fluxes for a forward simulation to compare with observations. My

main concern is that the three sections feel rather unrelated and somewhat limited in their analysis. The paper gave me the impression of being a collection of small extra results that supplement Patra et al, 2005a,b (which focus on the interannual variability of the estimated fluxes). While I appreciate that there is often more to be learnt from a set of inversions than can be presented in one or two papers, I believe that each of the results presented here needs to be more thoroughly explored before it will add significant new knowledge about atmospheric inversions. I will discuss my general concerns about each area in turn before moving to specific comments on the text in its current form.

(a) Sentence 3 of the abstract (p6802, line 7-10) suggests that greater confidence be placed in the inversion results of this paper (87 site case) compared to previous inversions because of their better agreement with the ocean inversion results in the southern hemisphere. This encapsulates my concern with this section of the paper. The difference from previous inversions is very dependent on the inclusion or not of Easter Island data. It may well be significant that the ocean inversion supports the Easter Island atmospheric data but apart from the South Pacific region, I do not think the ocean inversion gives clear support for one atmospheric inversion over another. Also the good agreement with the mean ocean inverse estimates needs to weighed against the results for interannual variability where the 87 site case shows more variability than ocean models and other inversions (Patra et al, 2005a).

(b) This section would benefit from being placed in the context of previous CO_2 inversion work assessing interannual variability in meteorology (Dargaville et al, 2000, Rodenbeck et al, 2003). Also the uncertainty on source estimates needs to be considered when assessing the impact of using different meteorology.

(c) The analysis from the forward simulations is for a mix of sites that were or were not in the inversion. We should have different expectations for the quality of the fit in these two cases. This is not adequately explained. Also I am uncertain as to the value of the case with changed diffusion. What does it mean to estimate fluxes with one version

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of the model and perform a forward simulation with another? If the increased diffusion version was used for the inversion, the estimated fluxes would change and the fit to the data would likely be much the same.

I am uncertain whether this paper can be sufficiently revised to warrant publication in anything close to its present form. At the moment it seems to add little new knowledge to the atmospheric CO_2 inversion field. Rather any findings are incremental, adding only a little to work that has already been published. For this reason, my assessment of the paper leans towards a rejection rather than a major revision. However if revision is to be attempted, I would consider dropping the material on the forward simulations (c) completely and providing a more thorough analysis of the two remaining areas (comparsion with ocean estimates, IAV meteorology).

Specific comments

p6804, line 14-20: the justification given here for doing the forward simulations appears weak - it is written as though it is just a check that the inversion was set-up correctly. In fact we know that even with a correct inversion, using a transport model with non-linear advection will mean that a forward simulation with fluxes from an inversion will give different results from a simple sum of the response functions (Law et al, 2002) and any difference is likely to increase as more regions are included in the inversion. This would be one justification for the forward runs. A second justification is that most of the sites tested are not included in the inversion. Thus you are testing whether the estimated fluxes are sufficient to also model the sites that aren't included or whether these sites are inconsistent with the flux estimates.

p6806, line 9 - page 6807, line 12: It might be clearer if these paragraphs were a sub-section of 2, perhaps labelled 'Forward simulations'. You might also consider swapping the paragraphs so that you describe the forward simulation before you explain how you processed the timeseries.

p6806, line 9: 'weekly and daily mean'. Do you mean here that you fitted the pseudo-

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weekly GLOBALVIEW values as the observations and daily means from the simulations?

p6806, line 10: 'derive a best fit curve' The GLOBALVIEW data are already fitted with a smooth curve. Perhaps you should only remove the long-term trend from GLOBALVIEW without doing further fitting of the seasonality?

p6806, line 21: It is my understanding that no adjustment is made to individual records in GLOBALVIEW to reconcile differences found in round-robin testing of high pressure cylinders.

p6807, line 18: I would suggest replacing 'unprecedented accuracy' with 'low uncertainty'. It would also be helpful to note that the small uncertainties include some measure (assuming this is the case) of the range of results produced when different OGCMs are used in the ocean inversion.

p6807, line 19: I think you might want to treat the ocean inversion estimates as ONE yardstick but not THE yardstick.

p6807, line 24: The West Pacific has a larger difference than the North Pacific for the 87 site case. Also the tropical Indian region should be mentioned in the text as well as in the footnote to the table (Note that Baker increased the data uncertainty for Seychelles during periods when the data is thought to be less reliable).

p6807, line 3-14: Discussion of Fig 3. It would be helpful to include a measure of the uncertainty for each region in the figure. I assume that this would then show that most of the estimates agree within their stated uncertainties. The South Pacific may be an exception but neither Baker nor Gurney use Easter Island in their inversions which would explain the difference. You need to clarify what Rodenbeck results you are showing. In the text you suggest it is the case with 35 sites while in the figure caption it appears to be an average over a number of their cases with different numbers of sites and different time periods. If you are showing an average of cases, perhaps it

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would be simpler to compare with a single case as any network dependence is easier to diagnose. See next point for a comment on the Rodenbeck South Pacific estimates.

p6808, line 23-29: The discussion of the South Pacific should also consider the sites at the western end of the region. Law et al, 2003, showed that the Pacific ocean ship data (25 and 30S) and Cape Ferguson tend to push the South Pacific to a more positive estimate while Easter Island wants a more negative estimate. This would explain why the small Rodenbeck networks (without EIC) still give a negative (approx -0.8 PgC/y) estimate for the South Pacific since they do not include Cape Ferguson or the Pacific cruise data. It is also worth noting that the addition of EIC in the Rodenbeck case only decreases the sink to about -1.4 PgC/y (a difference of 0.6 PgC/y) whereas here and in the Law et al., 2003 case, the addition of Easter Island changes the flux by close to 1 GtC/y. Presumably this is because in the Rodenbeck case, the sources are estimated at grid-box (8x10) resolution rather than for the Transcom regions so EIC influences a smaller region in the Rodenbeck case than here. (Note that Rodenbeck et al, 2003 also briefly discuss the sensitivity to EIC in relation to their Fig 15). Although the inversion presented here has a larger number of regions than in TransCom, the division of the Transcom region preserved the longitudinal extent of the box so that the east-west 'conflict' in the data cannot be resolved. The ocean inversion does solve for the eastern and western part of the South Pacific separately. What does this show? Is there any indication of more sink in the east than the west of the basin?

p6809, line 10: 'not many systematic studies'. Both Dargaville et al. (2000) and Rodenbeck et al. (2003) discuss the impact of using IAV winds rather than a single year and come to rather different conclusions. It would be good to put your results in the context of their findings.

p6809, line 16-17: It would be good to have some indicator of how large the differences are for each region. Can you generate some measure of difference, such as root mean square difference between cases scaled by estimated uncertainty i.e. a large difference on a poorly constrained region may be less significant than a smaller difference on a

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well-constrained region.

p6809, line 24: 'This condition probably weakens the link ...' You should be able to test this statement by looking at the uncertainty on the tropical South American flux. Is it larger at times when you believe the link to be weaker? In general the uncertainty on tropical South America is large and the differences between cases may not be that significant relative to the uncertainty.

p6809, line 26: 'This suggests that the TDI/64-2000 inversion ...' This sentence is unclear.

p6811, line 20: Are these stations not used because their records are too short? If so, it might be worth saying this.

p6811, line 22: 'fairly ideal for our validation test' I think you should probably note here that you would expect to fit some sites better than others. For example you would expect to fit sites that are included in the inversion better than ones that are not. Also if you have multiple sites within a region then these will be less well fitted than sites which are alone in a region. The prescribed data uncertainty for a site will also determine how well it was fitted in the inversion and consequently its likely performance in the forward test.

p6811, line 26: Do the fossil emissions contribute to interannual variability in the prior growth rate?

p6812, line 4-5: 'overestimated by several tenths of a PgC globally' I don't think that a mismatch in growth rate at three sites is sufficient evidence to support this statement.

p6812, line 10: It seems you are calculating a mean square difference rather than a goodness-of-fit. Also I would avoid using chi-squared as a symbol as it may be confusing - the data contribution to chi-squared in inversion studies includes division by the data uncertainty which is not done here.

Table 1: the numbers in columns 8 and 9 are a repeat of those in columns 6 and 7.

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The numbers in column 4 do not seem to quite agree with those in Table 2 of Patra et al. (2005a) - what is the difference?

Figure 1: If possible it would be good to use a dashed line to indicate where the regions were divided to give the 64 region case. Perhaps remove the country borders if possible.

Figure 3: Can you show the uncertainty on the fluxes?

Technical comments

p6802, line 21: Langenfelds not Langenfeld, also this reference is missing from the reference list

p6803, line 1: Patra 2005. The references include Patra 2005, 2005a and 2005b. Should these be listed as 2005a, 2005b and 2005c? In which case all Patra references need to be checked for consistency.

p6803, line 3: Bousquet reference is 2000 not 1999.

p6805, line 20: Insert 'is' before 'analogous'

p6806, line 29: should 'or' be 'are'?

p6807, line 6: Randerson et al missing from reference list

p6808, line 1, line 6, line 13: Rodenbeck 2003a or 2003b?

p6808, line 11: replace 'this' with 'that' if referring to Baker inversion

p6808, line 13: delete 'in'

p6808, line 17: 'region' not 'regions'

p6808, line 18: delete 'the' before 'using'

p6809, line 22: Shu and Clarke not in reference list

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p6810, line 12: 'positive flux anomaly was deduced'

p6810, line 12: Baker is 2006 not 2005

p6810, line 24: Feely et al not in reference list

p6813, line 1: 'Le Quere, C.' instead of 'Quere, C. L.'

p6813, line 12: In text GLOBALVIEW is listed as 2005, here it is 2002

p6815, line 5: subscript 2 for CO2

p6815, line 19: subscript 2 for CO2

p6815, line 25: space or - between 'curve' and 'fitting'?

Table 1 caption: You should perhaps note that all the ATMOS-INV cases are with 64 regions

Table 2 caption: need 'increased' before NIES/FRCGC model diffusion ?

Figure 3 caption: Presumably this should be Rodenbeck 2003b. Suggest 'their study' instead of 'this study'

Figure 5 caption, line 3: 'a year' not 'an year'

References

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