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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating a 3-D transport model of atmospheric CO_2 using ground-based, aircraft, and space-borne data" *by* L. Feng et al.

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Evaluating a 3-D transport model of atmospheric CO_2 using ground-based, aircraft, and space-borne data

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We thank the referees for their constructive comments on our paper. We have addressed all the comments received in the revised manuscript, and believe the modifications will enhance our discussion of the work shown.

This paper is mainly focused on evaluating the GEOS-Chem CO_2 transport modeling by comparing model atmospheric CO_2 concentrations with observations at different altitudes. One obstacle of such a comparison is the lack of accurate information on surface CO_2 fluxes, in particular, the information on land biosphere CO_2 fluxes. As pointed out by the referees, current prior flux climatologies underestimate land sinks, resulting in an unrealistic annual increase of model atmospheric CO_2 concentrations. To ensure a meaningful comparison with observations, we have introduced corrections to the prior surface fluxes from 22 TransCom regions by fitting the model CO_2 concentrations to the ground-based observations at 66 GLOBALVIEW sites below 3 km, using an Ensemble Kalman filter.

One major concern from the referees' comments is about the accuracy of the fluxes estimated by our EnKF approach, which is similar to the common batch inversions used in

ACPD

10, C10829–C10842, 2010

Interactive Comment

the TransCom 3 experiments but with a lag window of 8-months, (c.f. 3 years for batch inversions). We agree that changes in flux inversion configurations such as the flux spatial resolution, the selection of observations, and the assumptions on a priori fluxes can affect the a posteriori estimates. For example, a much longer time span in principle could result in more observation constraints on regional flux estimates. However, the nature of top-down approaches requires that the a posteriori should be inspected together with the associated uncertainty. When accompanied by the diluted signals of the monthly region fluxes, model errors associated with long-distance transport will limit the benefits from much longer time span in terms of both the uncertainty reduction, and bias corrections (considering that unevenly distributed constraints from the current observation network would not be dramatically improved from measurements on outflows far from their origins). Our results on the global annual net fluxes are in agreement with other long-term experiments, including Carbon-Tracker 2009, LSCE v1.0, and JENA S99 v3.2, usually well within the posterior uncertainties.

Uncertainties of the flux estimates, sparse existing observations, and the representation errors of the coarse spatial and temporal model resolution compromise our ability to evaluate model transport. This paper instead examines the main temporal and spatial structures of the model atmospheric CO_2 concentrations simulated by the GEOS-Chem transport model with two (GEOS-5 and GEOS-4) meteorological fields.

The following details our replies to the referee's comments (denoted by italics).

Review report 1

ACPD

10, C10829–C10842, 2010

1 General Comments

1.1 Paragraph 3, Page C8005

I would like the authors to clarify which GLOBALVIEW sites were used in the flux inversion: it is too difficult to tell from Figure 1. Were any of the biweekly aircraft profiles used? This is very important for interpreting the results: if only surface sites were used, then the comparison with the aircraft data after the inversion can be interpreted more easily in terms of vertical mixing errors, whereas if there are vertical profiles in the flux inversion, then interpreting the a posteriori fits depends more on where one looks.

A table for the sites used for flux inversions has been included in the revision as Table 1. Here we have included aircraft measurements at 4 sites, none of which reaches above 2.5 km.

Secondly, the global mean flux results obtained from the flux inversion are larger for 2004-2006 than a simple fossil fuel minus atmospheric increase calculation would give. I assume this is because an additional anthropogenic input due to biofuel burning has also been added, correct? If so, please give the annual totals assumed for this term. Further, when comparing your results to those of previous analyses (Transcom) that did not use this term, it might be useful to subtract off the contribution from this term, or at least mention the impact in your discussion.

Our work includes an additional bio-fuel emission of 0.75 PgC/yr, of which 0.34 PgC/yr comes from the northern hemisphere continents. We have revised the manuscript to make comparisons with other inversion experiments clearer.

The a priori land biospheric fluxes used in this study (from the CASA model) are balanced over the coarse of a year, or multiple years (that is, the overall magnitude of respiration is chosen to balance photosynthetic uptake), whereas we know that, on a global scale, the land biosphere actually has been taking up carbon in recent decades,

ACPD
10, C10829–C10842, 2010

however. The ocean fluxes have no inter-annual variability. And the fossil fuel fluxes assumed here have no seasonal or diurnal variability (which is fine, since a good global model for these does not yet exist)...

We agree with the referee's views on the land biosphere prior fluxes. In particular, land sinks have been severely underestimated, which leads to unrealistic annual increase of model atmospheric CO_2 concentrations. In this study, we introduced corrections to the regional fluxes by fitting models with observed CO_2 concentrations at selected sites by using EnKF, so that we can make meaningful comparisons between model simulations and observations. We rely on the EnKF to account for the year-to-year changes in the land flux.

The fluxes that need to be corrected by this inversion have an impact on atmospheric concentration gradients that takes years to fade away. There are still significant gradients in the meridional direction after three years of mixing, and gradients between the troposphere and stratosphere take even longer to smooth out. If errors are made in the flux optimization step, these errors will corrupt the trends, seasonal cycles, and vertical gradients discussed in this paper, and the conclusions about the model transport may well be incorrect.

We agree with the referee that errors in the flux estimates will affect the structure of the model CO_2 concentrations, and some errors in the structure could take many years to fade away. Under the current observation network and the optimal estimation frame, even the most elaborate inversion system will have large uncertainties and inter-model differences, which, together with other problems such as representation errors, limit our ability to reproduce observed atmospheric CO_2 concentrations. The results from the comparisons hence require careful interpretation of the results, considering, for example, that an uncertainty of 1 PgC/yr in the annual global flux estimates, implies an uncertainty of about 0.4 ppm/yr in the model annual global CO_2 growth rate. We hence limit ourselves to some major respects such as the model seasonable cycles at different altitudes.

ACPD

10, C10829–C10842, 2010

To do this flux correction properly, then, it seems to me that a data span longer than the three (or four) years used here would have been more appropriate....

We agree that in principle longer data span could improve the model agreements with observations. But as mentioned before, under the current observation network, the resulting fluxes will still have large uncertainties, possibly with biases entangled with systematic model transport errors. In fact our three-year net emissions agree with other experiments well. So benefits from longer data span will not be expected to change or improve our current evaluation of model CO_2 transport in troposphere dramatically.

If there are significant errors in the concentration field assumed at the beginning of the span (and from the discussion in the text on page 18031 line 16-on, it would seem like there are, since only a global offset was added to correct initial errors), then some method of correcting errors in fluxes before the start of the measurement span is required, otherwise these errors will corrupt the fluxes estimated during the span; since this does not seem to have been done here, this initial condition error may well penetrate into the span for a year or two, at least

We applied an adjustment to the CO_2 concentrations over the southern hemisphere by fitting the initial distribution on January 1, 2003 to GLOBALVIEW observations averaged over three latitude band of 30 degree during the first month of 2003. As a result (see Figure 6 in the old main text), the a priori surface CO_2 concentrations (the green line) are in reasonable agreement with the observations (the black line) at the right beginning of 2003 over different latitude bands. However, we agree with the referee that the vertical structure may still be unrealistic, which could affect the following 4-year flux inversions, although most of the effects are expected to be absorbed in 2003. On the other hand, inter-comparison between the model a posteriori CO_2 concentrations with independent observations, such as CONTRAIL, shows no dramatic changes from 2004 to 2006. Based on these observations, we do not expect that a longer spin-up

ACPD
10, C10829–C10842, 2010

time will change the major conclusions in our current study. Changes have been made in the revised text to stress these points.

1.3 Paragraph 2, Page C8007

The use of the ensemble Kalman filter (EnKF) for this study causes the largest problems, however, I think. The EnKF is an approximate estimation method that is best used on problems that are too large to use exact methods on. For the problem examined here (with 4x12x22=1056 flux variables being solved for), an exact inversion approach could have been used, such as the batch inversion method used in the Transcom study. First, in that case the exact covariance matrix could have been solved for, rather than the ensemble approximation given here. More importantly, though, in that case the impact of fluxes on concentrations could be carried out across the full 4-year span considered. By choosing to use only an 8-month window in the filter (or smoother) used here, the impact of fluxes on concentrations further away than 8 months has been severed...

As pointed out by the referee, the major difference between the EnKF approach and the batch inversions is the use of an 8-month lag window. As mentioned above, we find only marginal benefits from using a longer time span (such as 12 months), particularly when the uncertainties and bias from long-distance transport are taken into account. Our 3-year flux estimates are largely consistent with the recent inversion experiments.

1.4 Paragraph 2, Page C8008

I wish the authors discuss a bit more whether they feel the GEOS-5 fields result in more accurate CO_2 simulation than the old GEOS4 fields, when run through GEOS-Chem, what is reason for this in terms of the winds and vertical mixing fields ..?

ACPD

10, C10829–C10842, 2010

Following the referee's suggestion, we added a figure to the main text to directly compare GEOS-4 and GEOS-5 model CO_2 concentrations with the same a-priori surface fluxes. Major differences are found over tropical lands, due to their different convection schemes. As mentioned before, it is still difficult to judge which model is better.

Is GEOS-Chem 'bad' model in the context of that analysis because it gets such a large northern land uptake ?...

Accounting for additional extra biofuel emissions, our inversions using GEOS-4 meteorology agree well with estimates inferred from other approaches. Also, parts of the differences between GEOS-4 and GEOS-5 are related to the different vertical resolution (i.e., 30 vertical levels versus 47 vertical levels). Considering the uncertainties associated with current flux inversions, we can not conclude that the stronger uptake is certainly caused by the model defects.

2 Detailed Comments

L22-24: could this bias in trend reflect a lag of earlier fluxes reach the upper troposphere ? How is spin-up done here

We believe the bias in trend is more related to the quality of the current AIRS retrievals over mid and high latitudes, which have not been well validated.

P18031, L 16: Was the Palmer 2006 run done with the balanced CASA as well, or did it have realistic inter-annual variability in the land biosphere? If it used CASA as well, then I don't think this global correction is good enough, since most of the uptake may be in the extratropical NH, and a global correction would not correct these well. A lot of the biases seen here may simply be due to not having a spatially-correct correction of these initial errors.

Palmer et al 2006 also used the balanced CASA model. When we constructed the

ACPD
10, C10829–C10842, 2010

initial CO_2 concentrations in this study, in addition to the mentioned global one, we adjusted the initial atmospheric CO_2 in the southern hemisphere, by fitting model initial values at 3 latitude bands of 30 degree with the zonal means of the co-located GLOBAVIEW surface data during the first month of 2003. As a result (see Figure 6 in the main text of the old version), the a priori CO_2 concentrations at the boundary layer (the green line) are in reasonable agreement with the observations (the black line) at the beginning of 2003 over different latitude bands. However, we agree with the referee that the vertical structure may still be unrealistic, which could affect the following flux inversions in many years, although most of the effects are expected to be absorbed in 2003. However, Inter-comparison between a posteriori model CO_2 concentrations with independent observations, such as CONTRAIL, shows no dramatic year-to-year changes during 2004 to 2006. So, we do not expect longer spin-up time will change the major conclusions on the current evaluation of model transport of CO_2. These points have been added to the revised manuscript.

P18034: Given that the number of unknowns is only 4x12x22=1056 here, it seems like it would have been better to simply do a straightforward single matrix inversion, rather than using the ensemble Kalman filter. The EnKF has several disadvantages for this problem: 1. there are approximations involved in the ensemble implementation of the Kalman filter that introduce error and degrade the estimate beyond what the traditional Kalman filter would give; 2. the 8 month window assumed here introduces error by neglecting the effect of transport beyond 8 months; 3. a given month of flux is constrained by later measurements only out 8 months in the future, rather than out to the end of the span; and 4. the a posteriori covariance estimate is only an approximation (despite the use of a sample of ensemble members as large as the number of unknowns) of the true one

Please see the reply to the general comments in Paragraph 2, Page C8007.

P18036, L 1-4: Since you don't give a list of what these sites are, you should at least say how many sites there are in each category (white circles, red squares). The prob-

ACPD

10, C10829–C10842, 2010

lem is that some sites might be aircraft vertical profiles, in which case we do not know how many levels you use; also, some sites are close to being on top of each other and are hard to discern from the figure. A list would be best, but failing that, some more details in the text should be given

Following referee's suggestion, a table has been added in the revised manuscript to list the sites used in fluxes inversion. We also used some aircraft sites, but none of these data are above 2.5 km level.

P18036, L 5: The whole discussion of relative weights and the selection criteria for including stations in the study is not at all clear: please give more details. What do you mean by a relative weight of 6.0, for example? What are the weights relative to? How is the 1 ppm uncertainty for transport factored into the discussion?

The relative weights reflect how many real measurements are available during each one year period. Because we are assimilating the real measurements (as opposed of the extended data set with gaps fitted by extrapolation procedures), changes in the availability of observations may introduce noise in the a posteriori fluxes (as well as the model CO_2 concentrations). In this study we have chosen 66 stations which are under 3 km altitude, and have fewer observation gaps; the remaining sites are used as independent data for evaluating the model transport. The 1 ppm is added to reflect the model transport errors (see Rodenbeck et al., 2006).

P18036 L 20: the -4.4, -3.9, and -5.2 PgC/yr for land+ocean for 2004-2006 are a bit stronger than I get for those years, based on fossil fuel minus atmospheric increase. Is this because the land uptake must account for the biofuel and fire emissions, as well, for those years? If so, please provide us the global annual totals of biofuel and fire emissions that you assume for those years. If not, how do you account for the differences

The annual bio-fuel emission is equal to 0.75 PgC. Following the referee's suggestion, we have now included Table 2 to compare our annual global net fluxes with results from

ACPD

10, C10829–C10842, 2010

several long-term experiments including Carbon-Tracker 2009, LSCE v1.0, and JENA S99 v3.2. T he agreements between our result (in particular the G4 ones) and other experiments are generally well within the uncertainties.

P18036, L 24: Just pointing to the Stephens et al (2007) is not sufficient here: if you think that transport differences between G4 and G5 are at the root of the flux differences in 2005, you should give some evidence of this. What differences in transport, in particular, do you think explain these results

Following this referee's suggestion, we have added a figure (new Figure 1) for direct comparison between the simulated CO_2 concentrations for GEOS-4 and GEOS-5 meteorological fields, driven by the same a priori surface fluxes. The new figure highlights the differences associated with the GEOS-4 and GEOS-5 convection schemes. We have noticed that G5 tends to have a slightly higher north-south hemisphere gradient at the boundary layers, which may lead to more northern land uptakes. .

P18037, L 9: Given the great interannual variability that is generally thought to occur, especially in the land biosphere, there is little meaning in comparing your results for 2004-2006 with the 1992-1996 Transcom values. It would be more useful to compare your results to the various estimates for 2004-2006 found in the literature. Or, failing that, to estimates for longer spans that will be less liable to the particular span considered.

See comments above, and Table 2

P18037, L 15-16: 'Our global annual G4 and G5 a posteriori estimates have much stronger sinks over northern continents: we acknowledge that the most likely cause is due to changes in biospheric activity.' I think that it is quite speculative to say that the most likely cause is an increase in biospheric activity. If you want to investigate that possibility, do the same analysis using data from 1992-1996 to see whether your results are more in line with Transcom or not. A more likely explanation is that using an 8-month window in the EnKF likely is too short to adequately constrain the tropical

ACPD

10, C10829–C10842, 2010

land regions: they will float to whatever value is convenient to satisfy the global constraint, allowing the northern land to go wherever it wants to go to satisfy the global extratropical N/S gradient.

As mentioned in the previous reply, some other inversions also show stronger northern uptake. However, considering uncertainties in these estimates, and the inter-model differences, we can not clearly identify the causes. We have revised the manuscript accordingly.

P18039, L 8-10: Why not avoid this circularity by only using those sites not used in the flux inversion ...

The observation gaps in those 'unused' sites prevent us extracting trends and season cycles in a consistent manner.

Fig6: hard to see the differences between model and data on this plot: a difference plot would be better for this (we know the a priori is bad, given the use of balanced CASA, that should not be the point of this graph, and that is all that one can see from it).

Figure 6 (new Figure 7) is revised following the referees' suggestion, so that differences between model and data are more clearly presented.

P18040, L 25, this inaccuracy in fitting the trend would improve, presumably, if a span longer than 3 years were to be used. As it stands now, there is not enough of a penalty in missing the trend to compensate for the benefit in other aspects of the cost function fit.

As mentioned above, we agree that a time span longer than 3 years may fit the model a posteriori results better to the observations. However, the main purpose of the current paper is to examine main spatial and temporary features in simulated CO_2 concentrations. More accurate and reliable surface flux estimates from more observations will be the subject of future work.

18041, L 1: ' why would the larger-than-expected land sink explain the inaccurate trend

ACPD

10, C10829–C10842, 2010

estimate? It is only the CHANGE in a sink over time that affects the trend, not the value of the sink itself ?

We are not sure we have understood the question correctly. The annual trend reflects the increase of CO_2 mass in atmosphere, which is mainly determined by the global net annual fluxes.

18041, L 20-21: ' Over southern middle latitudes the model has a smaller seasonal cycle and lower concentrations than observed by AIRS, suggesting errors in the fluxes and/or atmospheric transport.' Given the variability in the AIRS data, it is easily possible that the difference could be due to AIRS problems, as well. This ought to be noted..

We agree that AIRS have not been fully validated over these regions, and it is likely that partial of the deviations are associated with observation errors. We have clarified this point in the revised manuscript.

18041 L 22-25: 'Over northern tropical latitudes, the a posteriori model seasonal cycle is in good agreement with AIRS, but has an amplitude much smaller than the sparse GLOBALVIEW aircraft data that span 5–8 km. We did not observe the difference in seasonal cycle with the ground-based GLOBALVIEW data, suggesting that incorrect model vertical transport plays an important role in the discrepancy between the model and data.' You have not displayed the GEOS-Chem model results sampled at the location of the GLOBALVIEW measurements here, is it possible that sub-sampling the model properly might improve the fit to the GLOBALVIEW data

Thanks for the referee's suggestion, model simulations sampled at the time and location of each aircraft measurements have also been added. The results suggest that part of the underestimation is due to the applied vertical weighting functions. We have clarified this point in the revised manuscript.

Section 6.2.3, You might want to just remind the reader here that the CONTRAIL data were not used in the flux inversion, so that they provide a good test of the vertical

ACPD

10, C10829–C10842, 2010

transport in the model.

We have clarified this point in the revised manuscript.

3 Editorial comments

P18028 L13-14: ' You should reword this ...

We have changed the sentence accordingly

P18031 L13: 'inavailability'

The word has been changed accordingly

P18031 L17: 'correction' not 'correct'

We have changed the sentence accordingly.

P18031 L 9: 'geographical distribution' missing word...

We have changed the sentence accordingly.

P18035 l 6: instead of 'include', consider rewording this to accounts for

We have changed the sentence accordingly.

ACPD

10, C10829–C10842, 2010