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Interactive Comment

Interactive comment on "Sensitivity analysis of the potential impact of discrepancies in stratosphere–troposphere exchange on inferred sources and sinks of CO_2" *by* F. Deng et al.

Anonymous Referee #2

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This study investigates the importance of atmospheric transport uncertainties in stratosphere – troposphere exchange for the estimation of surface fluxes of CO2 using satellite data. This is a very interesting topic and also timely, because of some other studies arriving at conclusions about regional carbon fluxes from the use of GOSAT that are heavily debated. Here a mechanism is proposed that has the potential to resolve part of this intriguing puzzle. The shift in inversion-estimated emissions with latitude seems a logical consequence of the upper air CO2 fluxes that are introduced. It is important to know, however, how justified these corrections really are, whether they make the model more realistic in the end, or whether the impact on the inversion results really repre-

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Interactive Discussion

Discussion Paper

sents that of the underlying transport model problem. My main requirement, before this study can be promoted to the next stage of ACP, is to demonstrate more clearly that this is indeed the case as will be explained in more detail below.

GENERAL COMMENTS

Further motivation and clarification is needed of the different time windows that are used. At the start of the method section it is mentioned that GOSAT data are used spanning July 2009 to December 2010, but surface fluxes are only optimized for the period March-August 2010. For the regression to HIPPO, March-April 2010 was used (the campaign is from March 24th to April 26th), The Osiris O3 simulation was from 20 March to 2 April, whereas the ACE-FTS validation was from 20 March to 3 April. As I understand it, the period of the HIPPO campaign is used to determine the CO2/O3 correlation, which is translated into a CO2 correction using the OSIRIS optimized model. However, this correction is then assumed to apply to the whole period from March to August. No information is given on whether or not this is justified. Moreover, the correction is quantified for the month of March, although the modeled O3 has only been optimized for the period 20 March to 2 April. It might imply that O3 was off in the first part of the month and that therefore the CO2 concentrations were off as well. Or has the CO2 correction, that has been derived for the period 20 March – 2 April been assumed to be constant for the whole period? In that case, it is not a surprise that the derived flux corrections are roughly the same for every month, but that doesn't imply that a constant correction is a valid assumption to make in the first place. In the revised version of the manuscript these issues should be explained much clearer than is the case right now.

The purpose of the ACE-FTS validation is not quite clear. First I thought that it covered a different part of the atmosphere (since it measures down to the mid troposphere), but the comparison is limited to pressures up to 200 hPa. Judging figure 4, this is probably just up to the altitude of maximum correction by OSIRIS. It raises the question in which pressure range OSIRIS data were assimilated (which I didn't find back), and

ACPD
15, C3058–C3062, 2015

Interactive Comment

if it extends to pressures above 200 hPa then why the comparison to ACE-FTS is limited to pressures up to 200 hPa. Presently the ACE-FTS validation seems just like a validation of OSIRIS (potentially gap filled using the model), rather than a validation of the O3 assimilation. I wonder actually why after optimization the general shape of the mismatch (under/overestimated O3 at higher/lower pressures) remains. The text mentions that the optimization significantly improves the agreement with ACE-FTS. Looking at figure 5, I wonder how significant this improvement really is, and why substantial differences remain.

Besides the O3 validation, I would have expected a CO2 validation against HIPPO after the regression correction is applied. Figure 3 only shows how the correlation between CO2 and O3 improves after assimilating OSIRIS data, but not how well the applied CO2 correction actually works. This could easily have been included in my opinion. In addition, I think further support is needed for the assumption that this correction is not just valid for part of the HIPPO 3 campaign, but also at other times of year. If the time window of the inversion had been shifted to also cover HIPPO 2, then this would have offered a truly independent validation substantially strengthening the case for the method that is used. I also find the validation of CO2 and O3 too much concentrated to higher latitudes, whereas a correction has also been applied to the tropics. Both latitudes are important since the question we are dealing with is about the partitioning of CO2 fluxes between the tropics and the extra-tropics. I see no reason, given the data that are available, not to extend the validation to the tropics.

In my opinion, one issue has been overlooked which could have implications for the size of the surface flux corrections that have been derived. This is that the CO2 correction fixes its concentration in the UTLS, but not the underlying transport problem. It means that the model is still mixing too fast, which the flux correction will need to keep up with. The larger this flux, the larger the compensating inversion-derived correction in the surface flux (because of the large-scale mass balance constraints imposed by the measurements). It means that the surface flux corrections should be an upper limit

ACPD
15, C3058–C3062, 2015

of transport model induced error at best. This should be made much clearer in the discussion and conclusions section.

SPECIFIC COMMENTS

Abstract, line 21: From the results I understand that the tropical correction comes on top of the correction in northern latitudes. This sentence suggests that in this case only an emission correction was applied to the tropics. Else I wonder why the correction is larger than that at higher latitudes. If it were the same then it would correspond to a pure mixing problem (from tropics to high latitudes). Which would make sense because I suppose tracer transport in the model is (at least close to) mass conserving.

Page 10822, line 23: Does BEPS simulate terrestrial ecosystem exchange outside the boreal zone? If not, then what was assumed for the a priori ecosystem exchange at mid latitudes and in the tropics?

Page 10823, line 1: Are the a priori uncertainties of GPP and TER assumed to be independent? How about the time correlation of the uncertainty in the 3 hourly fluxes.

Figure 1: From this figure it is actually not so clear how important the differences between HIPPO and GEOS-Chem really are. It depends on how the blue dots are distributed within their range at a certain latitude. Around the equator it is impossible to see if there are blue dots behind the red band. The fact that the data show a larger range may be because the model doesn't resolve small-scale variability. Are the differences significant after averaging to a resolution that the model can be expected to resolve?

Page 10826, line 16: does assimilation increase the bias between 65 and 75 degree north?

Page 10826, line 27: This sentence confuses me. I suppose what is meant is that the model shows similar CO_2/O_3 correlations at places where HIPPO data are available and elsewhere in the Arctic. Please rephrase.

Page 10828, line 1: “This is not a concern since ...” I don’t quite understand this sentence. To me it seems that the CO_2 source is meant to correct a mixing problem, i.e. a process that conserves mass. The dispersion of the signal to other regions might therefore worsen the agreement with HIPPO there, and influence the fit to GOSAT data and thereby the inversion-derived fluxes. We can exclude the possibility that GEOS CHEM is missing a 0.13 Pg/month sink of CO_2 in the UTLS. Since the dispersion into the tropical belt is seasonal it might well influence the seasonality of tropical fluxes (as is found for Tropical Asia)

Page 10828, line 13: Other then suggested here, the UTLS sink of 0.13 Pg/month is not small. It is of the same order of magnitude as the regional changes in surface fluxes that result from it.

TECHNICAL CORRECTIONS

page 10825, line 27: “latitudes” i.o. “altitudes”.

page 10827, line 25: I guess “source” should be “sink” here.

page 10828, line 13: I guess “source” should be “sink” here.

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ACPD
15, C3058–C3062, 2015

Interactive Comment