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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The importance of transport model uncertainties for the estimation of CO_2 sources and sinks using satellite measurements" *by* S. Houweling et al.

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We would like to thank the referee for having a careful look at our manuscript. Our response to the comments is listed below, including the corrections that were introduced to the manuscript.

'Highlight the differences between transport models'

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Interactive Discussion

Discussion Paper

The referee is correct that the connection between transport model differences and underlying transport model dynamics is limited. As explained also in our reply to referee 1 this has not been assessed within this study and is actually a very difficult task given the competence of our team. We point to differences in resolution, stratosphere-troposphere exchange and boundary layer mixing between the models. To trace back the origin of specific differences which are found to the underlying differences in model dynamics requires a study of its own. In the context of this study, such as discussion would at best be highly speculative.

'Model – data comparison'

The reason we are careful drawing conclusions from the model-data comparisons is that the origin of the mismatch is not yet fully understood. In particular it is not yet clear what part of the systematic offsets is due to surface emissions, atmospheric transport and spectroscopic uncertainty. In the case of Darwin we have indications from another model that the surface emissions explain most of the differences. This is why we highlight the limited observational constraints of CarbonTracker in the manuscript. We agree with the referee that our estimate of transport model uncertainty is a conservative estimate of the real uncertainty. We have modified the manuscript is follows:

Abstract 'A variable, but overall encouraging agreement is found in comparison with FTS measurements at Park Falls, Darwin, Spitsbergen, and Bremen, although systematic differences are found exceeding the 0.5 ppm level. Because of this, our estimate of the impact of transport model uncertainty is likely to be conservative.'

Conclusions 'It this stage, it can be concluded that our estimate of the impact of transport model uncertainty is likely to be conservative.'

'P. 14739 Line 7. Is this common co2 flux priors?' 'a priori' has been added to the sentence.

'P. 14739 line 8 –" [Forward] simulations of column averaged CO2 ..." Has been cor-

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rected.

‘P. 14739 line 9. It should be noted that this is not the case south of the equator. It should also be noted that FTS is being used as a way to validate the forward models.’ This is still the abstract where we are supposed to be brief.

‘P. 14739 Line 16 - Be more specific about GOSAT (and OCO) requirements given the result that you have here.’ We cannot be more specific about GOSAT and OCO because we didn’t investigate it. Some speculative numbers would certainly not improve the manuscript.

‘P. 14739 Line 19. A path forward needs to be outlined about how one would identify a "good" transport field.

In our response to referee 1 we mention that the most promising way forwards would be an intensified collaboration between experts in atmospheric dynamics (who now about the compromises in favor of weather prediction) and atmospheric tracer transport (who know about the limitations reproducing measurements of passive tracers). We have modified the abstract and conclusions as follows:

Abstract “Improving the accuracy of these models should receive high priority, which calls for a closer collaboration between experts in atmospheric dynamics and tracer transport.”

Conclusions “Further development of these models should receive high priority. To improve the accuracy of atmospheric transport models we recommend a closer collaboration between experts in atmospheric dynamics and tracer transport.”

‘P. 14740 line 2 - heterogeneous is not the right word. "sparse" would be more appropriate.’ ‘Highly heterogeneous’ has been replaced by ‘sparse’.

‘P. 14743 Line 16. "transport model difference only". As mentioned later there this is not true since the inversion resolutions are different so the representativeness error will play a role.’ Resolution is a characteristic of the transport model error too. We

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explicitly mention the issue of transport model resolution and representativeness error elsewhere.

‘P. 14743 Line 17. Maybe it should be worded that "As a requirement for forward and inverse models used in this study it was confirmed that both the forward model and the inversion using forward model data had the same fluxes. Differences were all less than x and can be attributed to rounding errors(?).’ In practice the method was used as explained in the next sentence, which guarantees that this condition is met without (round off) error.

‘P. 14743 Line 18. Not clear what "this" is referring to. A further explanation of why inverting for the difference between two models is helpful is also needed.’ ‘which guarantees that this requirement is satisfied’ has been replaced by ‘which guarantees that no flux adjustment are found in this case’. ‘This case’ refers naturally to the case mentioned in the previous sentence.

‘P. 14743 line 25. Unclear what IFS was being used for.’ As mentioned in this sentence IFS contributes forward simulations. It means we have 4 forward models and 3 inverse models. Because of this each inverse model can take 3 sets of A-SCOPE samples (each generated by another model than used for the inversion). ‘models’ at the end of the sentence was replaced by ‘forward models’ hoping that this would avoid the confusion.

p. 14744 Line 1 not sure why representative error should be small is the foot print is 100m - thought that representativeness error was a function of small error.’ ‘this’ does not refer to the 100m footprint but to the difference in grid definition. What is meant here is that differences in Rf due to different grid definitions are expected to be small compared with the difference between a coarse model grid and the 100m footprint of A-SCOPE. ‘this’ has been replaced by ‘differences in grid definition’.

‘P. 14747 Line 3. The lack of disagreement between FTS and forward models may suggest that the inter-hemispheric mixing is not accurate. It would seem like this kind

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of bias is demonstrative of what this study does not adequately test (i.e. large scale biases in mass transport that lead to biases in any inversion.) It should be pointed out that this is where the global data coverage provided by satellites will be very useful. We assume the reviewer means 'lack of agreement here'. As we explain, the fact that all models agree on the wrong seasonality suggests that the problem is rather in the surface fluxes. However, we agree that we cannot exclude the possibility of a common transport bias.

The following sentence has been added: 'However, we cannot exclude the possibility that the model-data mismatch at Darwin is caused by a common transport model bias.'

'P. 14747 Line 7. Explain offset why TCCON calibration might effect this in more detail.' It is exactly as mentioned here. TCCON applies a scaling factor to every site, which has an uncertainty. An error in the scaling factor leads to an offset. We suspect the cause of the confusion is the 0.2%, which doesn't refer to the offset but to the total column. To avoid this confusion we changed 0.2% into 0.75 ppm.

'P.14747 line 12 - not fully mass conserving' This has been changed.

'P. 14747 line 22- Aircraft data shows a more complicated set of north-south and east west gradients in FT which are seasonally controlled. This should be a reviewed reference.' We removed the part that referred to the AGU presentation of Keppel-Aleks.

'P. 14748 line 11. This statement is a little misleading since there should be averaging out on a monthly time-scale as well as the seasonal time scale.' 'avoid' has been replaced by 'reduce'.

'P. 14748 line 14 - sea should be ocean.' This has been done.

'P. 14750 Line 2 "also" should be as well.' This has been done.

'P. 14750 Line 9 put error in context of expected fluxes.' This is very tricky in the case of net biospheric fluxes, since they can be neutral. Initially, the error was formulated as 20% of the flux, but after a long debate it was decided that this was not meaningful and

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should therefore be replaced by an absolute flux.

‘P. 14751 Line 8. The supposition that influence of model error will be less in an inversion that accounts for errors in measurements and representativeness seems counter intuitive and should be explained further. In particular, it would seem like the reduction in precision noted would also be accompanied by a reduction in the ability of the inversion to give us a whole lot more information than we might get from the prior. In other words, it would seem like in addition to decreasing the precision the addition of transport error will give the prior more control over the outcome of the inversion.’

This is exactly what happens. The fluxes are constrained by the combination of measurements and model. If we account for model uncertainty, the overall performance goes down. To make this point clearer we added the following sentence: “This precision reduction reflects the fact that transport model uncertainties reduce the capability of a satellite to constrain CO$_2$ sources and sinks.”

‘P. 14751 Line 14-18. Explain how this relevant. Be explicit about how temporal and spatial covariance in transport will add to or take away from the uncertainty of the flux.’ This is a rather technical discussion, which we’d like to avoid here. The main purpose is to quantify a statement made earlier that transport model errors have an important systematic component.

‘P. 14751 line 20 should read "improve the [transport] model themselves". Some explanation should also be given in the text about the difference between the transport model in the context of the inversion at a specific resolution and the meteorological. We have added ‘transport’. For the general discussion on transport model uncertainty see the first paragraphs of this reply.

‘P. 14752 line 12. "... the fluxes that were ..." is this referring to prior fluxes?’ Yang et al. doesn’t deal with inverse modeling. In the context of forward modeling ‘prior’ has no meaning.

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‘P. 14752 line 15-16 "leads to [more] realistic simulation ..."’ This has been changed.

‘P. 14752 Line 19-21. not exactly clear what the optimized fluxes refer to but if the carbon tracker fluxes are what is being referred to they suffer from the same problem as the TRANSCOM fluxes in the sense that the prior for CarbonTracker is CASA which also has a seasonal lag. The lag in seasonal cycle will act to dampen the seasonal cycle postiori flux. See CarbonTracker comparison with data- the seasonal lag is still there even after it has been optimized.’ Relevant here is that the error in the seasonal cycle near the surface is much less than what is found for the total column.

‘P. 14752 Line 24-29. Compare FTS with aircraft to see if the amplitude is explained. This can be done with at LEF. Although LEF samples only go to 4000 m a 30% discrepancy should be detected.’ We are currently doing this. The problem is indeed that the aircraft profiles don’t go high enough.

‘p. 14753 LINE 20- the model to model comparison is a little bit fictitious because these are monthly averages.’ Because of the cloudiness actually not that many samples are averaged for a 1x1 grid box.

‘P. 14753 line 27 not sure that remaining spectroscopic uncertainies was explained in the text.’ Remaining spectroscopic uncertainties were discussed in the context of TCCON.

‘P. 14754 LINE 7 mentions "certain aspects of transport that need to be evaluated by tracers - what are these tracers?’ We added “such as 222Rn and SF6”

Figure 6 text is unclear. Maybe "Top panel: TM5 inversion using ..." no parenthesis. This has been changed.

Figure 7 – same as above This has been changed.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14737, 2010.

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