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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Estimating regional fluxes of CO_2 and CH_4 using space-borne observations of $XCH_4 : XCO_2$" *by* A. Fraser et al.

A. Fraser et al.

pip@ed.ac.uk

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First, we would like to thank both reviewers for providing thorough and useful comments that will strengthen the paper. We have addressed all reviewer comments (denoted by italics).

Reviewer 1 C4816

Section 2.1: Please include a few more details on the filtering of GOSAT data instead of making the readers hunt for it in other publications. For example, a few key sentences from Parker et al. (2011) on the cloud filtering would be helpful. Also, please point out here that data over oceans are not used. It would be nice to briefly describe validation of data with TCCON, even if the versions or quantities are not exactly the same. This

section reports that Fraser et al. (2013) used "the previous version of the data." Please provide more details on the differences between the versions (do you just mean XCH4 or is it more than that?).

Point taken. We have included more details on the cloud removal procedures. We have also clarified the key differences between data versions, including, for example, updates to spectroscopic inputs.

Section 2.1: Section 3.1: Please list the resolution of the GEOS-Chem model here rather than later (from Sect. 3.2, we find that it is the same 4 x 5 degree resolution of the GEOS-5 meteorological data). Please give a general reference for GEOS-Chem and GEOS-5.

Unless we are misunderstanding the comment, the resolution of the model was provided in Section 3.1. In the revised manuscript we have provided a general reference for GEOS-Chem and GEOS-5.

Section 2.1: Section 3.2: More detail is needed here. For the prior error covariances, please state that the values were empirically chosen if this is the case or provide more detail for the estimates of these numbers. For the GOSAT observation error covariance, it is stated "For GOSAT, we use the provided measurement error." Who provided this error? Is it the radiance error propagated through to retrievals? It is also stated "When we average we sum these errors in quadrature." Please be more specific, average what and which errors?

We agree. The prior error covariances are empirical. They are based on previous work [Palmer et al, 2006], which is now stated in the revised manuscript. We have also clarified the meaning of measurement error.

Section 2.1: p.15875, L12: Please explain why this is "as expected".

We expect a positive model bias for CO2 because our knowledge of the natural uptake is incomplete and generally underestimated. This is now clarified in the revised paper.

Section 2.1: Figs. 4-6: The captions are very long, repeat information from the figures and the text (and each other). They can be shortened with no loss of information. For example, it is not necessary to repeat in Figs. 4 and 5 that "The model has been sampled at the time and location of the GOSAT observations, and convolved with scene-dependent averaging kernels." This information is already provided in the text and doesn't need to be repeated. You could just say "see text for more details".

Our philosophy is to ensure all figures and their captions can be understood without the main text. With all due respect, we have retained the captions as originally submitted. We believe the statement about sampling the model is sufficiently important to repeat so that the reader does not second-guess whether we have addressed this issue.

Section 2.1: Fig. 4 caption: Please state that the variations are computed about the "annual" mean values as in the text (or just say see text for details).

We thank the reviewer for spotting this oversight in our caption.

Section 2.1: Fig. 5: The lines are sometimes overlapping for XCH4 and XCO2. I think it would be better to put these on separate panels. There is a lot of white space in the ratio plots that could be reduced. It's not clear how useful the correlation coefficient is when there is not much variability (i.e., low values do not necessarily indicate a poor result). Since the correlations are not discussed in the text, they could be removed.

The purpose of the figure is to show that XCH4 and XCO2 are noisy with similar variations – plotting them on separate panels would make it more difficult to assess their similarities. The white space is necessary as we are keen to retain a consistent y-axis range for all panels. The point about correlation coefficients is well taken but we have retained this information for completeness.

Section 2.1: p.15879, L15-16: Subject-verb plurality does not agree.

We thank the reviewer for spotting this grammatical error.

Reviewer 2 C5639

Section 2.1: One of the more important specific comments is that in Figure 6, it seems that for experiment b, the a posteriori XCH4:XCO2 must be quite different from (much lower than) the observed values given the very large a posteriori CO2 fluxes. How is such an unexpected outcome possible? Could you have made a mistake in generating or displaying these results? At the least, an explanation in the text is needed.

In this experiment we have increased the prior fluxes by 20% so that the prior XCH4:XCO2 ratio is larger than the observed ratio. However, CO2 and CH4 fluxes have different geographical distributions so the simultaneous increases do not necessarily cancel out in the ratio. For this experiment, the inversion compensates for the "model" ratios being higher than "true" values by decreasing CH4 and increasing CO2 fluxes both of which would act to reconcile the model and observed values. If this reviewer had difficulty interpreting this result we anticipate that other readers will also have similar difficulties so we have clarified the description in the revised manuscript.

Section 2.1: In the abstract, you prominently describe the use of a priori error covariance for CO2 and CH4 fluxes from biomass burning, and you imply that the error covariances do not have enough of an impact to allow the true fluxes to be recovered by the inversion. However, you don't actually quantify in this study the effect of the error covariances by comparing these results with an OSSE in which no covariances are included. Thus, you should not include any conclusions about the effect of the error covariances, unless you conduct an additional OSSE and analyze the effect of the covariances.

Based on past numerical experiments [Palmer et al, 2006] it is clear that such a weak correlation is insufficient to separate information about either gas from the ratio. It does not require an OSSE to understand that.

Section 2.1: Also in the abstract (lines 24-26), you state that "using real GOSAT XCH4 : XCO2 ratios together with the surface data during 2010 outcompetes inversions using the individual XCH4 or the full- physics XCO2 data products." But you do not show in *this paper that the XCH4:XCO2 inversion can actually outcompete the XCH4 or XCO2 inversions. The regional uncertainty reductions in the first are not consistently larger than those in the last two. Thus, this conclusion needs to be changed.*

Fluxes inferred from the observed ratios generally have lower uncertainties. Given that these data are more plentiful and also do not suffer from systematic error to the extent the single gases do, we feel justified in our conclusion. However, we will temper the conclusion to reflect that there are a number of exceptions where uncertainties from the ratio are larger than those from individual gases.

Section 2.1: Abstract, line 3, and other locations: You refer to the retrieval of the ratio of XCH4 to XCO2 as a "proxy method", but strictly speaking, the proxy method uses model XCO2 to derive XCH4 only. I suggest that you give the XCH4:XCO2 retrieval another name.

Technically, the approximation lies in the assumption that the light paths through the atmosphere are the same for the different wavelengths used to fit, and subsequently ratio, XCO2 and XCH4. To address this point we taken the reviewer suggestion and renamed the ratio.

Section 2.1: Abstract, lines 24-26: It is not clear from this sentence whether surface data are used in the inversions with XCH4 or XCO2 data. Thus, the reader may wonder whether the surface data are actually helping to outcompete as opposed to the ratio data.

We have clarified this point in the main text.

Section 2.1: p. 15870, lines 17, 19: You use the term "data assimilation" to refer to your Bayesian inversion system, but my understanding is that data assimilation generally refers to more complex systems such as variational data assimilation and ensemble Kalman filters.

Data assimilation is of course scientific jargon but it should include the theoretical ba-

sics such at Maximum Likelihood to more complex filters. We have clarified this in the main text.

Section 2.1: p. 15871, lines 13-17: This description is too concise, and doesn't explain things such as the quality-of-fit filters, and why data with certain SZAs and medium gain are omitted. Also, what does "previous version of the data" refer to? And a description of the full physics retrievals for CH4 and CO2 needs to be provided, given that those data are shown in Figure 2 (and Figures 4 and 5?). You seem to provide a little information on this at the end of Sect. 4.1, but more information should be provided here.

Agreed. This is criticism common to both reviewers and we have clarified this point in the main text.

Section 2.1: Line 18: What filters were applied to the full physics retrievals?

We have added to the text that describe the filters associated with retrieved aerosols amounts, geophysical characteristics, and consistency between band 2 and band 3 of XCO2.

Section 2.1: p. 15872, lines 17-18: You should state here that these are bottom-up, a priori flux estimates as opposed to inverse estimates from previous studies.

Agreed. Manuscript changed accordingly.

Section 2.1: p. 15872, lines 19-21: A little more detail on the OH would be helpful, such as resulting methyl chloroform and/or CH4 lifetime.

Agreed. Manuscript changed accordingly to include MCF lifetime.

Section 2.1: p. 15873, line 28 and p. 15874, line 1: More precisely, "flux errors" rather than "fluxes".

Agreed. Manuscript changed accordingly.

Section 2.1: p. 15874, lines 2-3: It would be helpful for the reader (and the referee) if you provided a bit more detail here and explanation for the correlation coefficients used.

Agreed. This is criticism common to both reviewers and we have clarified this point in the main text.

Section 2.1: p. 15874, line 7: I think it is important for you to report the amount of noise in the observed ratio. Also, it seems to me that this noise (specifically the standard error of the monthly grid- level mean) ought to be included in your estimate for the measurement error. Why do you not account for it?

We do account for this observed variation in the ratio. We have changed the manuscript to reflect that.

Section 2.1: p. 15874, lines 14-15: Where can we find the provided measurement errors? ï£ij We have changed the text to trace back our source of single measurement error to the appropriate literature.

Section 2.1: p. 15874, lines 16-17: What does this sentence refer to? Be more specific. And shouldn't there be a factor of 1/$\sqrt{N}$ for the error of an average quantity?

The standard error does contain a factor of 1/$\sqrt{N}$

Section 2.1: p. 15875, lines 5-6: I'm not sure I follow. Please explain.

If the seasonal cycles are 6 months out of phase they will tend to cancel each other out in the ratio. Unfortunately, this means that we will have reduced sensitivity to either seasonal cycle using the ratio. We have clarified this point in the main text.

Section 2.1: p. 15875, line 7: To be clearer, I suggest "spatial variability of the annual average" instead of "annual variability".

Agreed.

Section 2.1: p. 15875, line 8: Does "Common features" refer to the model and observed XCH4:XCO2?

We have changed to the sentences to reflect this.

Section 2.1: p. 15875, lines 8-9: Should be "interhemispheric gradient in the ratio".

Agreed.

Section 2.1: p. 15875, line 12: Why is the XCO2 bias "expected"?

This is addressed in our response to Reviewer 1.

Section 2.1: p. 15875, lines 12-13: Could you please provide some numbers here in the text?

Agreed. Text change accordingly.

Section 2.1: p. 15875, lines 16-17: Clarify that "smallest" refers to comparison with XCH4 and XCO2.

Agreed. Text change accordingly

Section 2.1: p. 15875, lines 19-20: The bias looks more or less constant to me.

The bias is small because we report column data for two years but there is a progressively negative model bias. We will clarify this point the main paper so the reader is not left squinting at the Figure.

Section 2.1: p. 15875, lines 24-27: This doesn't seem so clear to me based on the figure. It seems that XCH4 contributes to the peak in the ratio just as much as XCO2 does.

The XCH4 is in ppb and XCO2 is in ppm so seasonal variations in the ratio are dominated by changes in XCO2.

Section 2.1: p. 15876, line 3: Specify that these "variations" are likely associated with *retrieval errors.*

Agreed. The text has been changed accordingly.

Section 2.1: p. 15876, line 27, and other locations: Figure 6 doesn't show a posteriori uncertainties, so the reader cannot determine for her/himself how the flux differences compare to the uncertainties.

We reported the mean difference and 1-sigma and the gamma value for all plots. This is what the text is referring to. We will clarify that point in the revised manuscript.

Section 2.1: p. 15876, lines 27-28: For both CO2 and CH4 fluxes? And what about negative biospheric CO2 fluxes? Are they multiplied by 1.2, or increased by 20% of the absolute value? Please be clear.

We multiplied by 1.2. This point has been clarified in the main text.

Section 2.1: p. 15877, lines 6-8: Please provide explanation.

Agreed. We provide more explanation in the revised manuscript.

Section 2.1: p. 15877, lines 12-13: How does that compare to the OSSEs using both GOSAT and surface data?

This information is in available in the Figure. But we will include numerical examples in the revised manuscript.

Section 2.1: p. 15877, lines 24-28: This pre-processing analysis is not explained well. I am not sure I understand what exactly it involved and the purpose of it. What's the significance of the "mean annual difference"? And if you know the difference between the model and the data, couldn't you fit a 2nd-degree polynomial as opposed to 4th-degree so that the bias is completely removed? I suppose you wanted to simulate the real world, where the functional form of the bias is not known?

The aim of experiment 4 was to test the sensitivity of flux inversions to the temporal

and spatial variations in bias on retrievals of XCH4:XCO2 ratios. We assumed the imposed biases were latitude-dependent, described as a second-degree polynomial, to mimic the observed differences between the GEOS-Chem model and the UoL GOSAT XCH4:XCO2 retrievals. These biases were added to the synthetic observations, together with random error, on a monthly basis.

Two parallel inversions were conducted in experiment 4. First, we assumed no pre-processing to detect/remove the artificial biases. The resulting flux estimates showed obvious departures from the "true" fluxes. Second, we attempted to the remove the systematic bias by pre-processing the data. For the pre-processing step, we assumed the biases were in the form of a 4th-degree of polynomial. We intentionally used a different model to what was used to define the bias, as would be the case for real observations.

Section 2.1: The coefficients for the bias-correction function were then calculated by fitting to the annual mean deviations between the GEOS-Chem model (forced by the prior fluxes) and the synthetic XCH4:XCO2 ratios with added biases. Our results show that the pre-processing bias-correction led to flux estimates that were close to the "true" fluxes.

We have modified the text to reflect this improved description of the pre-processing.

Section 2.1: p. 15878, lines 1-2: How close? Could be more quantitative.

See above response.

Section 2.1: p. 15878, lines 7-8, and other locations: You associate with the reference "Feng et al., 2011" an estimate of CO2 fluxes using an ensemble Kalman filter and GOSAT XCO2 data. However, that study did not use any XCO2 data in its inversion. Are you actually referring to a different study?

The statement refers to the application of the EnKF rather than use of these data. We have revised the manuscript to clarify this point.

Section 2.1: p. 15878, lines 6-8: It would help the reader if you provided a bit of description of these previous XCH4 and XCO2 inversions here. Among other things, do the inversions include in situ data as well as GOSAT? If so, do they use the same network of sites as the ratio inversion? These are very important to know.

They include the same surface stations. We have clarified this point in the revised manuscript.

Section 2.1: p. 15878, lines 12-13: Explanation?

We are unclear what the reviewer is asking. Explanation for the higher regional fluxes? We could offer an explanation but the reason may well be different for each region. We have revised manuscript to include some explanation for this result for our study period but we will follow this up with another paper that goes into more detail for each region for several years.

Section 2.1: p. 15878, lines 13-14: Clarify that this is in combination with in situ data.

In section 4.2 we state that combining the GOSAT ratio and in situ data is our control experiment.

Section 2.1: p. 15878, line 15: Replace "larger from" with "larger than from".

We have revised the manuscript to clarify this point.

Section 2.1: p. 15878, lines 16-18: Explanations?

We discuss this in the concluding remarks.

Section 2.1: p. 15879, line 3: What does "other" refer to?

This has been clarified in the revised manuscript.

Section 2.1: p. 15879, lines 3-5: That "the associated error reductions for the CO2 fluxes inferred from the XCH4 : XCO2 ratio data are typically larger than those for CH4" is not clear to me from Figure 8.

Typically is the wrong word. Generally is better with the caveat that the larger improvements are over regions with larger seasonal cycles and where we typically have the least understanding.

Section 2.1: p. 15879, lines 5-6: Please be more specific than "different from". Are any of the differences robust, and if so, can you explain them? Why aren't the uncertainty reductions all larger for the ratio inversions?

Good point. These are good questions that have been addressed in the revised manuscript. To address the last point: uncertainties associated with data that include uncharacterized systematic bias as is the case for XCO2 and, to a lesser extent, XCH4 may provide a false sense of knowledge.

Section 2.1: p. 15880, line 2: I suggest "flux adjustments".

This wouldn't make sense given the context.

Section 2.1: p. 15880, lines 8-9: You should try to discuss some more the implications of the results for understanding of the carbon cycle. For example, although you note the effect of the ratio data on the Tropical South America carbon balance, you could discuss further what the results suggest regarding the location of the global terrestrial CO2 sink. You should also discuss implications for the global CH4 budget.

The focus of the paper was to characterize the method and show it works. We will provide a brief analysis of any impact on the carbon cycle, but to address that properly will require a dedicated analysis of multi-year dataset.

Section 2.1: p. 15880, line 10: "slightly larger reductions" in what?

Typo. Should refer to error reductions.

Section 2.1: p. 15880, lines 16-17: You have not shown this in the paper.

We believe we have. We are using data that is much less compromised from systematic bias, and we have more data. As a result we are more confident of our flux estimates and their estimates.

Section 2.1: Table 2: This table duplicates the information provided in Figure 8. I think the visualization of the info in Figure 8 is helpful, so I suggest omitting this table.

Some readers appreciate seeing numbers as well as the Figure. We have decided to retain both the Figure and the Table.

Section 2.1: Table 2, caption: Specify whether "land fluxes" are biospheric only or the sum of all fluxes.

The land fluxes are the sum of all fluxes. We have clarified this point in the revised manuscript.

Section 2.1: Figure 2: You should specify what full-physics observations are plotted here–XCO2 or XCH4?

Agreed.

Section 2.1: Figure 4: You should clarify whether the 1-sigma values in the bottom row are calculated using monthly means or individual observations.

This is described in the main text but will clarify in the caption. We calculate the 1-sigma values using individual observations.

Section 2.1: Figure 6: Are these annual totals? Also, you should mention that these are the sum of all flux types or sectors.

This is for 2010 as stated in the caption. We have made the text clearer in the revised manuscript.

Section 2.1: Figure 8: Specify whether the "land fluxes" are biospheric only or the sum of all fluxes.

The land fluxes are the sum of all fluxes. We have clarified this point in the revised manuscript.

References

Palmer, P. I., P. Suntharalingham, D. B. A. Jones, D. J. Jacob, D. G. Streets, Q. Fu, S. Vay, G. W. Sachse, "Exploiting observed CO:CO2 correlations to improve inverse analyses of carbon fluxes," J. Geophys. Res., 111, doi:10.1029/2005JD006697, 2006.

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