GENERAL COMMENTS AND MAJOR SPECIFIC COMMENTS

Overall, this study involves novel ideas and quite a large amount of work, presenting useful forward and inverse modeling results for assessing the utility of XCH4:XCO2 ratio data for understanding the carbon cycle. However, the manuscript is in a somewhat rough state: many of the ideas and methods are not explained clearly, readers could be helped greatly by the addition of more details in many places, and there are numerous imprecise phrases and grammatical errors. Some of the assertions in this paper are very difficult to evaluate based on the scant information provided. Granted, the paper covers some complicated material, but additional work to improve the presentation could make it stronger and more easily digested. I provide detailed suggestions in the Other Specific Comments section below. Note that I do not list all the grammatical errors and typos, leaving that task to the authors.

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 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.

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.

Although this paper is definitely potentially worthy of publication in this journal, I do not think it is quite ready in its current state, and recommend that it be revised and preferably reviewed again before being approved for publication.

OTHER SPECIFIC COMMENTS

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.

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.

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.

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.

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

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.

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

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

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.

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?

p. 15874, lines 14-15: Where can we find the provided measurement errors?

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?

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

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

p. 15875, line 8: Does "Common features" refer to the model and observed $XCH_4:XCO_2$?

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

p. 15875, line 12: Why is the XCO_2 bias "expected"?

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

p. 15875, lines 16-17: Clarify that "smallest" refers to comparison with XCH_4 and XCO_2.

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

p. 15875, lines 24-27: This doesn't seem so clear to me based on the figure. It seems that XCH_4 contributes to the peak in the ratio just as much as XCO_2 does.

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

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.

p. 15876, lines 27-28: For both CO_2 and CH_4 fluxes? And what about negative biospheric CO_2 fluxes? Are they multiplied by 1.2, or increased by 20% of the absolute value? Please be clear.

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

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

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?

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

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?

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.

p. 15878, lines 12-13: Explanation?

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

p. 15878, line 15: Replace “larger from” with “larger than from”.

p. 15878, lines 16-18: Explanations?

p. 15879, line 3: What does “other” refer to?

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.

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?

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

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.

p. 15880, line 10: “slightly larger reductions” in what?

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

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.

Table 2, caption: Specify whether “land fluxes” are biospheric only or the sum of all fluxes.

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

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

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

Figure 8: Specify whether the “land fluxes” are biospheric only or the sum of all fluxes.