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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modeling of CH_4 emissions for 2010–2011 using different satellite retrieval products from GOSAT and SCIAMACHY" *by* M. Alexe et al.

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Interactive comment on "Inverse modeling of CH4 emissions for 2010–2011 using different satellite retrieval products from GOSAT and SCIAMACHY" by M. Alexe et al. Anonymous Referee #2

The authors thank the referee for her/his comments. We have included comment-specific replies (AC) in blue below.

This manuscript presents a large amount of inverse modeling work aimed at using satellite data constraints to improve our knowledge of surface CH4 emissions. CH4

column retrievals from multiple instruments, as well as multiple retrieval algorithms, are used to investigate the sensitivity of estimated fluxes to the different representations of the atmospheric XCH4 by these products. The most important result appears to be that the inverse estimates of the flux are rather robust, and that the improvement of GoSAT retrievals over SCIAMACHY is clearly seen for all scenarios investigated. Sensitivity to the bias correction scheme for XCH4 seems to be small, while model deficiencies are mentioned multiple times as a possible cause for model-observation differences.

Overall, this paper is well written and the study is conducted with a good eye for details, confirming the excellent track record in CH4 inverse modeling of this research team. Two major concerns that I have therefore are not about the validity of the results, but about the scope in which they are presented. I would like the authors and editors to consider this before publishing this otherwise solid investigation.

My first concern is that the paper teaches us very little about the global CH4 budget, despite using more constraints than many previous studies and spanning a substantial time scale. Perhaps a more detailed paper about the actual fluxes is coming, but in that case I would strongly suggest to send this methodology paper to another journal (such as GMD) and to publish the next paper in ACPD. Its much higher impact factor and broader readership is more suited for actual inverse results than for inverse study design in my opinion. Alternatively, it could be that this paper is an expansion of a piece of work done for the MACC project, and originally constituted a technical report. In that case I would ask the authors to try and expand the scientific content, possibly guided by my comments below.

AC: We will expand the discussion of the inverted fluxes in the revised manuscript, especially regarding the derived spatial emission patterns over North America and tropical Africa (please see also the responses to the comments of Peter Rayner).

My second concern is that this paper has a large amount of overlap with a previous publication from the TM5 group, Monteil et al., 2013, (JGR-Atmospheres). The authors

actually state that there are significant differences (p11498, line 17), but when reading more closely these are at a level where only a true expert would be able to judge them and one actually needs both papers side-by-side to know what really differs. To an average reader, both studies use a 4d-var approach with the TM5 model based on the work of Meirink et al., 2008, both studies have SCIA and GoSAT retrievals included, both studies assess proxy and full-physics products, and both studies use some TCCON and HIPPO data to assess posterior CH4 mixing ratios. As a result, they actually have a number of co-authors in common, which makes the lack of extensive comparison and discussion of these two studies even more worrisome. I would like to see this overlap identified much more clearly in the current manuscript, possibly even with a table summarizing the differences and their potential impact (e.g., an optimized bias correction versus a fixed one would enter an extra degree of freedom to fit the XCH4 data, etc). Logically, this manuscript then would also discuss the difference in outcomes of the two studies, ideally by giving an overview of global/regional/category fluxes for the common year (2010). This would also enhance the scientific content of this paper and help to address my first concern, while the amount of extra work needed is not that large since both groups likely used the same output formats from the shared TM5 model.

AC: The revised version of the manuscript will include a table summarizing the major differences between the present study and that of Monteil et. al (JGR, 2013). The potential impact on the derived fluxes will be discussed. Furthermore, we will better compare and contrast the main conclusions of the two papers.

Once both these concerns are addressed in a revised manuscript, I can recommend publication of this study. Further minor comments and questions:

Title: To increase the scientific value, a title that identifies an outcome (instead of an activity) would be helpful.

AC: Despite the very encouraging results (overall good qualitative consistency for the

inversions based on the different XCH4 products) we prefer to keep the more general title.

Abstract: In addition to a brief discussion of the actual CH4 budget, I would like to see a message or conclusion that comes from this study. What does it mean to other readers that your inversions show very similar performance? Should we use one product over another or does it really not matter and we should start focusing on transport modeling? Have we now meaningfully constrained the CH4 fluxes from equatorial Africa since these are robust?

AC: We consider in particular the following aspects as important messages regarding the different XCH4 products:

- despite the known limitations of the SCIAMACHY products, the SCIAMACHY based inversions show reasonable agreement with the inversions of the different GOSAT XCH4 retrievals.

- the fact that the GOSAT proxy retrievals (with the known limitations of the CO2 correction) yield overall similar results as the inversions using the GOSAT full physics products giving overall some confidence in the major spatial patterns derived in the satellite based inversions.

Despite the overall good qualitative consistency of the derived spatial patterns (especially over the US and tropical Africa), the differences in other regions (South America, India, and Europe), and differences of total emissions of larger regions (as the TRANSCOM regions) will need further investigation in subsequent studies. There is clearly a need for more validation data closer to major emission regions, especially in the tropics to be able to better evaluate the performance of the inversions / different XCH4 products. At the same time, also the transport models need to be further improved.

We will better summarize the main conclusions of this study (along the above lines) in

the updated manuscript.

p11498, line 27: This network is typically referred to as the Cooperative Air Sampling Network, operated by NOAA ESRL.

AC: Agreed - this will be clarified in the revised version of the manuscript.

p11500, line 15: This adjustment by 2 umol/mol for years where actual XCO2 from Carbontracker is available seems strange to me. Can one of the co-authors who delivered these products comment? Also, the observed growth rates of CO2 for that year are 1.84 ppm and 2.66 ppm respectively, which means that the XCO2 modeled for 2012 would be 0.5 ppm low. This translates to a 3 ppb XCH4 low bias if I am correct? Please comment.

AC: We have re-run scenario S1-GOSAT-SRON-PX using updated RemoTeC Proxy v1.9/v2.0 files for 2011/2012. The updated XCH4 retrievals were calculated from CO2 fields from CarbonTracker 2013, correctly taking into account CO2 growth rates for 2011 and 2012. While there are small quantitative differences between the GOSAT SRON/KIT Proxy inversion results (pre- and post-update), the main conclusions of the manuscript remain unchanged. The updated text will include the new results.

p11500, line 19: This statement only makes sense if you replace 'measurements' by 'product'.

AC: We will replace 'total column methane measurements' by 'derived methane column-average dry-air mole fraction (XCH4)' in the revised text.

Can you comment on the quality of the modeled CO2 fields? If these are from Carbontracker then they also use the TM5 model including its poor north-south transport (hinted at in this paper and Monteil, see my later remark). How would a double bias (XCO2 modeled and XCH4 modeled) play into your results? I guess this might partially cancel errors?

AC: The bias in the north-south transport of TM5 is largely compensated in the (CO2

and CH4) inversions by the flux adjustments, i.e., leading to biases in the derived fluxes, while (CO2 and CH4) concentrations should be as close as possible to the used observations. Since we use from CarbonTracker only the CO2 concentrations (and not the fluxes), the impact of North-South transport bias on the CO2 correction should be only "second order"; we don't expect that the "double bias" will cancel out; the final bias in the ratio between NH and SH CH4 fluxes should be dominated by the bias in the North-South transport of TM5 (and depend much less on the potential bias of CO2 fluxes, as long as the North-South gradient in CO2 concentration is realistically represented in the CO2 model fields).

p11506, line 6: Do you mean the lifetime of CH4 here?

AC: Yes; we will correct this in the final version of the manuscript.

p11506, line 10: O1D is not an isotope, simply an excited state of the oxygen radical

AC: We will correct this in the final version of the manuscript.

p11507, line 21: This aim of the study requires a more extensive analysis of the inverted fluxes

AC: We will expand the discussion of the inverted fluxes in the revised manuscript (see also the comment below referring to CH4 emissions over North-America; the potential influence of wetland emission priors on the derived fluxes over Tropical Africa will also be discussed).

p11508, line 1: I strongly suggest taking the results and discussion apart, so that more room is created to put your results into context. By merging them, there is little room for the reader to find the larger implications of each figure or number presented.

AC: We will revise Section 4 but prefer to keep the presentation of results and their discussion together.

p11508, line 15: did you actually average all the standard deviations, or did you average

variances? Please clarify.

AC: We first averaged variances, then from these we calculated the standard deviations. This point will be made clear in the revised manuscript.

p11508, line 24: This “probably plays some role” could be clarified possibly if these results are compared more extensively.

AC: The revised version of the manuscript will contain a more extensive comparison of the results of this study and those of Monteil et al. (JGR, 2013).

p11511, line 1: This statement points at a possible scientific discovery: your quite robust satellite inverse results suggest a different CH4 emission landscape over the USA than our prior idea. Please expand this finding by adding a discussion section on recent insights on North American CH4 fluxes, and consider using this in the abstract as well. In my opinion, it is at this point that satellite inversions become very useful: they could identify regions for further investigation.

AC: We will expand the discussion on North-American CH4 fluxes. We will refer also the recent comprehensive review by Brandt et al. (Science, 2014), which point to a systematic underestimation of CH4 emissions from North American Natural Gas Systems in bottom-up inventories.

p11513, line 16: What is the current status of this transport model bias? Monteil et al., seemed to suggest a fix was available that really improved the match to SF6 and also caused a substantial shift of fluxes across the tropics. Why was this fix not used here, and how can this issue still be subject of further study in a 2014 paper using the same model?

AC: The issue of inter-hemispheric transport bias in TM5 is not yet fully settled. Most recently a new parameterization of convective fluxes has been implemented. This approach is based on the ERA-Interim convective fluxes [Berrisford, et al., 2011], whereas the old scheme was that of Tiedke et al. [1987]. The new implementation increases

inter-hemispheric transport; however, it also has a significant impact on the modeled CH4 mixing ratios in the continental boundary layer. Further investigations (outside the scope of this paper) of the new parameterization scheme are needed before a definitive conclusion can be reached.

p11511, line 26: This suggests that more is indeed known about the quality of the modeled XCO2.

AC: The quality of the modeled XCO2 is largely dependent on the spatial coverage of the constraining data (for further details we refer the reader to the cited paper of Schepers et al. [2012])

p11513, line 4: Could observations from the upper troposphere from Caribic, Mozaic, or CONTRAIL help?

AC: The study of Bergamaschi et al. (2013) validated the posterior mixing ratios against CARIBIC data. We think that a major problem is the low vertical resolution in the UTLS / upper atmosphere (in the current TM5 versions a total of 25 vertical layers is used; the vertical model axis is defined by hybrid sigma-pressure coordinates, see Krol et al. (ACP, 2005) for more details).

p11513, line 21: Since this bias was now reported in Bergamaschi (2013a) and in Monteil (2013), I do not think this needs to be part of the results of this paper anymore.

AC: We included a brief summary of the HIPPO validation results for a single satellite inversions (GOSAT-SRON-PX; Figure 10). The other scenarios are discussed in the supplementary material. The study of Bergamaschi et al. (2013a) covered the period 2000-2010, and compared their SCIAMACHY and NOAA-only inversions with HIPPO 1-3 campaigns (2009 / 2010). The target period of this study is 2010-2011, for which the additional HIPPO 4-5 campaigns are available. The authors believe that the extended HIPPO data set is valuable for the overall validation of the inversions presented here.

p11513, line 27: Are there indications that TM5 also has problems simulating the UTLS region and its stratosphere-troposphere exchange? Why do you suggest this would go away when using a higher model resolution, is this based on tests or speculation?

AC: At this stage it is indeed speculative. This issues will need further investigation in subsequent studies.

p11515, line 5: Same question here. And why would you ascribe N-S biases to STE instead of to the too slow mixing by TM5 here?

AC: While the TM5 bias in (tropospheric) inter-hemispheric transport is probably largely compensated by a bias in derived emissions (leading to an overall realistic inter-hemispheric gradient in tropospheric CH4 mixing ratios), potential deficiencies in the STE and stratospheric transport could lead to biases in the modelled stratospheric CH4 mixing ratios, with some impact on the column-average dry air mole fractions (XCH4).

p11534, Figure 6: These panels are very hard to judge by a reader, as one has to visually compare detailed patterns across the globe from five maps. The summary by latitude band is more helpful, but there it is tough to see the different categories. Could this figure be replaced by one that shows the latitudinal distribution of each source category, with all scenarios in one plot? After all it is the scenario differences that must be judged most easily.

AC: We will change the color scale of Figure 6 to better highlight both similarities and differences in the inverted regional fluxes across all scenarios. We believe that a map view (with the appropriate colorbar) would allow the reader to immediately identify land regions with qualitatively consistent/different emission patterns.

p11536, Figure 7: This figure together with table 4 actually are a very nice summary of the global CH4 budget that I would like to see discussed more. Just putting these numbers into context of what we know about CH4 fluxes from other studies would already be a good step forward. Again, this asks for a more detailed discussion section.

AC: We will expand the discussion of derived emissions, but focussing mainly on the spatial patterns (see replies above).

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