Atmos. Chem. Phys. Discuss., 14, C3267–C3271, 2014
www.atmos-chem-phys-discuss.net/14/C3267/2014/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
14, C3267–C3271, 2014

Interactive Comment

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.

Anonymous Referee #2

Received and published: 5 June 2014

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.

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

ACPD
14, C3267–C3271, 2014

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.

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 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?

p11498, line 27: This network is typically referred to as the Cooperative Air Sampling Network, operated by NOAA ESRL. p11500, line 15: This adjustment by 2 umol/mol for years where actual XCO2 from carbotracker is available seems strange to me. Can one of the co-authors who delivered these products comment? Also, the observed growth rates of COâĆĆ 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 $\sim$3 ppb XCH4 low bias if I am correct? Please comment. p11500, line 19: This statement only makes sense if you replace 'measurements' by 'product'. Can you comment on the quality of the modeled COâĆĆ fields? If these are from carbotracker then they also use the TM5 model including its poor north-south transport (hinted at in this paper and

ACPD
14, C3267–C3271, 2014

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? p11506, line 6: Do you mean the lifetime of CH4 here? p11506, line 10: O1D is not an isotope, simply an excited state of the oxygen radical p11507, line 21: This aim of the study requires a more extensive analysis of the inverted fluxes 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. p11508, line 15: did you actually average all the standard deviations, or did you average variances? Please clarify p11508, line 24: This 'probably plays some role' could be clarified possibly if these results are compared more extensively 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. 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? p11511, line 26: This suggests that more is indeed known about the quality of the modeled XCO2. p11513, line 4: Could observations from the upper troposphere from Caribic, Mozaic, or CONTRAIL help? 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 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? p11515, line 5: Same question here. And why would you ascribe N-S biases to STE instead of to the

ACPD
14, C3267–C3271, 2014

too slow mixing by TM5 here? 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. 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11493, 2014.

ACPD

14, C3267–C3271, 2014

Interactive Comment