**Atmospheric
Chemistry
and Physics**
Discussions

Interactive comment on "Sensitivity of the recent methane budget to LMDz sub-grid scale physical parameterizations" *by* R. Locatelli et al.

Anonymous Referee #1

Received and published: 20 May 2015

This study investigates the importance of atmospheric transport model uncertainties for the estimation of global sources and sinks of CH4 using inverse modeling, applied to surface measurements and satellite retrievals. As in an earlier study a bias was found between GOSAT retrievals and a model that is optimized using surface measurements. The interesting new finding is that after improving the representation of the stratosphere in the model, the agreement with GOSAT gets worse. The underlying cause remains unclear, but the study sends out the important message that further research is needed to solve this problem. Other than this general message, the study could have benefited from a more detailed analysis of the results. As a result, the significance of the retrieval bias and transport model uncertainties for the quantification of surface fluxes is not so clear, as will be explained below. Repairing this will probably only require minor

revisions after which the manuscript can be accepted.

GENERAL COMMENTS

The manuscript refers to an unpublished manuscript of Monteil et al. However, in most cases it would be better to refer to a JGR paper that has already been published (Monteil et al, 2013). That paper provides a quantification of the bias using the TM5 model, which would be useful to compare with the results obtained in this study using LMDz.

In the conclusions it is mentioned that transport model errors lead to flux errors up to 50% at regional scales, but I do need see that back in any of the presented results. Looking at Figure 5, I wonder how significant the differences are, given the posterior flux uncertainties and the change from the prior. The figure shows a horizontal bar, which is not explained in the caption, but may actually be the prior. It is not only relevant to assess the uncertainty in the regional flux, but also the robustness of deviations of the inversion-derived fluxes from the prior (and their significance given the uncertainties).

Further information is needed about the treatment of the initial concentration and the atmospheric oxidation in the inversion. Are they optimized? If not, could an inconsistency between the initial concentration field at the start of the short-window satellite inversion compared to the longer window surface inversion explain differences in the derived global total? How about the global sink? Even if the oxidant fields are the same, the lifetime may be different due to differences in transport.

From the results it is clear that some representations of transport are more realistic than others. It would be interesting to know of this translates into optimized models that are more or less realistic. The comparison with HALOE in figure 3 is clear, but it is unclear whether improved performance can also be demonstrated in the troposphere – which might relate more directly to the accuracy of the inversion-derived fluxes.

SPECIFIC COMMENTS

Page 11862, line 12: "Monteil et al, 2013"

Page 11864, line 5: Has the HALOE dataset been corrected for the CH4 increase since those measurements were made?

Page 11864 last paragraph: I don't understand the second step of the inversion. In the second step the bias is quantified at each surface side, but how is that use in the second inversion step?

Page 11865 first paragraph: How is the global sink treated in the inversion?

Page 11866, line 6: This could be, but it depends on where the surface measurements are made (it woudn't be the case e.g. for SPO).

Page 11867, line 19: How do you define IH gradient here? Shouldn't it rather be called "hemispheric difference"

Page 11868, line 20: This conclusion is very sensitive to the relative weights of different measurement datasets in the inversion. If the weight of GOSAT is less than that of the surface network, that may also explain why the transport parameterizations have less impact on the fluxes.

Figure 3: Do the model contributions to the total column account for the averaging kernel of the satellite retrievals? This should be made clear.

TECHNICAL CORRECTIONS

Page 11856, line 1: "SCIAMACHY" i.o. "SCHIAMACHY"

Page 11857, line 4: "surface" i.o. "surrface"

Page 11859, line 28: "presented" io "presenteed"

Page 11864, line 25: "methane flux" i.o. "methane fluxe"

Page 11868, line 20: "that" i.o. "than"

Page 11871, line 20: "span" i.o. "explore".

Table 2, caption: "shown" io "showed"

Figure 4, caption: "institute" io "institude"

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