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Interactive comment on "Sensitivity analysis of methane emissions derived from SCIAMACHY observations through inverse modelling" *by* J. F. Meirink et al.

Anonymous Referee #2

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Meirink and co-authors use a series of synthetic data experiments to explore the sensitivity of surface fluxes estimated from SCIAMACHY satellite observations. The authors present a method for estimating methane fluxes from satellite data using a 4-D var assimilation method that simultaneously estimates the initial spatial distribution of methane and its surface fluxes. This paper is on a topic of great interest to the readership of Atmospheric Chemistry and Physics Discussions, the approach is novel, and it represents a significant contribution to our understanding of how satellite data might be used to estimate surface fluxes. I consider the manuscript to be worthy of publication, but I would appreciate it if the authors would respond to my general and specific

concerns.

General Comments:

One of the authors' primary conclusions is that the SCIAMACHY observations can contribute to considerable uncertainty reduction in methane source strengths. This conclusion is based on sensitivity experiments with synthetic data that test the sensitivity of an atmospheric inversion using SCIAMACHY observations to random errors, errors due to clouds, and the specification of a priori errors. However, there are a number of other sources of error in the SCIAMACHY observations that need to be addressed before one can reach this conclusion. The authors have briefly listed them (Page 9408, lines 19-24), but do not discuss their potential effect on the inversion, or why they chose specifically to investigate sensitivity to random errors, clouds, and priors. It would also be interesting to see how the synthetic datasets generated for this paper compare to real SCIAMACHY observations.

This manuscript is dense with technical details and has limited explanations of some methods and terms that are specific to atmospheric tracer inversions and remote sensing. In particular, it would be difficult for anyone who is not a specialist these areas to understand the Methods section. Therefore, I recommend that the authors revise the text of the paper in order to make it more accessible for the general readership of Atmospheric Chemistry and Physics Discussions.

Specific Comments:

It would be interesting to see some discussion of how the uncertainty reductions from SCIAMACHY observations compare to similar inversions using flask samples alone.

From my reading of the manuscript, it is still a little unclear how the inversion distinguishes between source categories in experiment 13 (Table 2 of the manuscript). The authors should also briefly discuss how well the satellite observations are able to distinguish between source processes with overlapping spatial patterns.

ACPD

5, S4104–S4107, 2005

The authors assume the error covariance matrix associated with the concentrations is equal to the difference between two simulations of the same model using first a 24-hour forecast and then a 48-hour forecast. This neglects the possibility of underlying errors in the model transport and chemistry. I don't think anyone has come up with a completely satisfying way of quantifying these errors, but the authors should discuss the issue.

Technical Corrections:

P. 9406, l. 6, There should be a comma following "In this approach"

P. 9407, l. 5, "More than half of the methane emissions" is a plural subject; therefore, "is" should be changed to "are".

P. 9407, paragraph 2, The authors could also mention here that the atmospheric growth rate of methane has large variability, and that there is considerable debate about the causes for this variability.

P. 9407, l. 22, The authors state that "the observations contain statistically significant information on emissions only on continental scales." This statement should be supported by a reference, since atmospheric methane inversions have been done on the grid scale level (e.g. Houweling et al., 1999).

P. 9407, l. 25 - 29, The use of the term "point measurements" in describing stations that observe strong signals from the sources may be somewhat misleading. As written, it could be interpreted to mean that stations that primarily observe background air are not point measurements.

P. 9408, l. 2, Please provide a brief description of what limb and nadir modes are.

P. 9408, l. 5, and other acronyms throughout, Please spell out the full name for acronyms at the first use.

P. 9408, l. 24, A reference discussing the sources of error in the observations would

ACPD

5, S4104–S4107, 2005

be helpful.

P. 9409, l. 4, A sentence or two describing how the 4-D var data assimilation technique works would be helpful.

P. 9409, l. 24 - P. 9410, l. 2, The authors discuss how including initial concentrations in the control vector can minimize errors due to initial conditions in short term inversions, however it does not become clear what the control vector is and how introducing initial concentrations might be helpful until the following section. I recommend either moving this discussion to the Methods section or framing it in simple, physical terms.

P. 9410, l. 4, The authors state here that they will "show, using SCIAMACHY measurements, that it is possible..." This wording is misleading because the authors rely exclusively on model simulations that approximate SCIAMACHY observations.

P. 9411, l. 17, It is not clear from this paragraph exactly how the chemical sinks modeled in TM4 are used in the inversion.

P. 9412, l. 11, A brief description of how the authors arrive at the observation operators would be helpful.

P. 9413, Please include a little discussion of the importance of the B and R matrices in the inversion and the relative weighting of the observations and priors.

P. 9415, l. 26-27, Sentence fragment. Perhaps it should read, "In this study, all cloud fractions over desserts that were smaller than 0.35 were set to zero in our simulations."

P. 9420 - 9422, Much of this section reads as a catalog of figures, with too much emphasis on description of the figures rather than discussion of the results.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9405, 2005.