

Interactive comment on “A multi-year methane inversion using SCIAMACHY, accounting for systematic errors using TCCON measurements” by S. Houweling et al.

Anonymous Referee #1

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General remarks: The overall impression is that this is a solid and clearly presented study using sound and valid scientific methods giving credibility to the main conclusions. It clearly takes the method of inverse modelling to derive methane fluxes one step further. Uncertainties and challenges in the applied method are to a large degree well explained and the need for better coverage of measurement data is rightfully highlighted. I recommend publication but have some (mainly minor) comments I suggest the authors should address. See detailed comments below.

Detailed comments:

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Page 28120, Line 1-3: I miss references to former studies on these issues. A lot of references in the next paragraph none in this.

P28123, L 27: Missing “of”: measurements of SF6

P28124, L10: Why is Edgar 4.1 and not Edgar 4.2 used? This would avoid much of the extrapolation since Edgar 4.2 has emission data until 2008. If 4.2 wasn't available at the time of this study it would be good to compare the values you get from your extrapolation with Edgar 4.2 for total emissions and major sectors and state how the uncertainty in apriori emissions are affected by the extrapolation.

P28129, L28-29: “It is not clear whether water vapor is the cause of the seasonal bias discussed here, or that it only happens to covary with a different underlying cause.” I understand that the cause is uncertain. I would however think the cause is an area of research for those working on SCIAMACHY retrievals. Earlier in the text you write: “In practise, well quantified biases are usually directly corrected in the model or the measurements.” Do I interpret things correctly if this in the future (if the cause is found) could be a so called well defined bias corrected in the measurements (?)

P28130,L9: The TCCON network really plays a key role in this study. I miss a map-figure showing geographical coverage and a table with information on temporal coverage. What about retrieval errors, uncertainties and biases in these data. Though these things might be described in referred studies I suggest including such information here given the central role of the TCCON measurements.

P28131,L18: I suggest to use other names than “flex” and “fix”. They could be mixed and are hard to discern. Especially in figures with small text sizes.

P28136,L25-26: I am a bit doubtful about the usefulness of combining aircraft measurements from different locations and times into a single profile and then comparing it with a model with coarse resolution. I miss some information on how this is done and what temporal resolution you have in the model output that is used for comparison.

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P28139, L6-7: “This is not easily explained, as such a reduction would rather have been expected during the anomalous drought of 2005.” This is a bit unclear since you in the paragraph above state that there is a reduction in observed total columns over the tropics in the end of 2005. Does this mean that the drought was earlier in 2005? Please clarify.

Discussion: The inversion set-up does not separate fluxes from different sectors. It would however be good somewhere in the discussion to elaborate a bit more on the wetland emissions. Wetland emission inventories have large spreads both in total emissions and spatial distributions. What are the strength, weaknesses (you mention for instance missing year to year variation of wetland area) and uncertainties of the a priori LPJ-WhyMe emissions? How is LPJ-WhyMe different from other wetland emission inventories? Findings from other inverse studies are included in the discussion but could other type of studies (forward modeling, emissions inventory comparisons, LPJ studies) indicate support of a movement of LPJ/wetland emissions from the extratropics to the tropics?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28117, 2013.

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