Atmos. Meas. Tech. Discuss., 7, C808–C812, 2014
www.atmos-meas-tech-discuss.net/7/C808/2014/

Atmospheric Measurement Techniques Discussions
Open Access

AMTD
7, C808–C812, 2014

Interactive Comment

Interactive comment on “Derivation of tropospheric methane from TCCON CH₄ and HF total column observations” *by* K. M. Saad et al.

Anonymous Referee #1

Received and published: 5 May 2014

General comments

This paper describes how tropospheric CH4 columns can be derived from ground-based total column measurements, at 8 different sites covering a wide latitude range. This provides an interesting additional data set to the monitoring of tropospheric methane. The subject of the paper matches therefore the scope of this journal. The methodology is well-explained and the manuscript is usually clear and concise. Some validation of the proposed products is made to give confidence in the methodology.

My main concern is the lack of a quantitative assessment of 1) the precision achieved in the derived tropospheric CH4 column, and of 2) the benefits of using the authors methodology (i.e. compared to the Washenfelder et al. (2003) paper). Therefore, I would recommend the publication of this paper in AMT, but with some few remarks given below.

Specific comments

1) Abstract: Add in the abstract the precision (in %) of the derived tropospheric CH4 columns.

2) Introduction: the TCCON and NDACC strengths/weakness are discussed here. It would be interesting to add the NDACC precision on tropospheric CH4 (if the authors could find any reference(s)), to be compared with the one achieved with their methodology.

3) Section 2:

a) p. 3476, l. 7-9: The sentence "Several TCCON stations are near in situ sites,..." would be better in the Introduction part where the description of the paper should be given. (the validation is made in Sect. 3 not 2.)

b) p. 3478, l.3-7: A qualitative description of the tropospheric error is given. The precision achieved with the methodology described by the authors is an important result, therefore I would give more details here: values (or range of values) of errors propagating in Eq. 6 (total column errors, summary of β error values – even if these one are given in details later,...). The authors include "only those measurements with final errors of less than 1%". This does not give the information of the mean (or median and/or range) error achieved. Which proportion of the measurements have to be removed because they have an error more than 1%,...

c) Sorry, I might have missed something, but I do not see the difference in the so-called β in Fig.1 and in Table 1, except that for Fig. 1 the period is 2004-2012, while Table 1 (and inset of Fig.2) shows annual values of β. Therefore I do not understand why the

AMTD
7, C808–C812, 2014

values of β in Fig.1 are larger than the means of annual values given in Table 1. This difference should be clarify. Maybe a different "label β" should be given in both cases.

d) Any idea about what happened in 2011 in the Southern Hemisphere (inset of Fig.2) ?

e) p. 3479, l.26-27: provide the minimum retrieval altitude (mean, median or range) of ACE-FTS profiles, please.

f) p. 3480, l.14-17: "The underestimation... in NH.. is a result of the lack of secular trend in the TCCON a priori HF profiles". I'm not sure I understand well the justification. Would this lack of trend in the HF a priori not impact the slope (calculated vs integrated) rather than the offset ? Could you clarify your statement, please ? Also, maybe these offsets (Fig. 3) could be quantified, in order to help for the discussion of the validation results (p. 3482, l. 22-25).

4) Section 3:

a) p. 3481, l.1-3: "...except in the case of Izaña". I do not see why the effects of the stratospheric variability would not be removed at Izaña. And, it seems to me (Fig.4a) that, indeed, the variability is reduced at Izaña (i.e. in 2011). Could you clarify ?

b) p. 3481, l.8-11: "The variances of the tropospheric DMFs over a given day are generally equivalent to those of the corresponding total column DMFs, although...": what do you mean by "variances" ? If you mean "individual uncertainties", then why the tropospheric uncertainties are equivalent to the total columns uncertainties (we would expect larger values due to propagation of errors in Eq. 6) ? Also, the fact that the tropospheric standard deviations are larger than the total columns ones seems to reflect indeed larger individual uncertainties of tropospheric columns, so this would mean that your "variances" are underestimated. But, if I misunderstood this text and if "variances" means something else, could you clarify what are these "variances" ?

c) Section 3.1: to me it is not clear enough from Fig. 8 that the new method is an

AMTD
7, C808–C812, 2014

improvement of the Washenfelder et al. (2003) method. I suggest a more quantitative assessment (e.g. provide the mean standard deviations in both cases, and check if the comparisons with in situ measurements are improving using the new method compared to Washenfelder).

d) p. 3482, l.12-14: "... individual errors of less than 10%...": the criteria is less strict for Fig. 8 comparisons than for the rest of the paper (Figs 4, 5, 6 and 9). Is it to increase the number of coincidences ? What is the main driver of a factor of at least 10 between the individual measurement uncertainties ?

e) For Fig. 9, 10, and 11, some quantitative comparisons should be made with the Washenfelder method (e.g. correlation coefficient, and/or slope, and/or standard deviation of the differences,...) to prove that the new method is an improvement.

5) Section Conclusions, l.4-7: "The methodology described here refines earlier tracer proxy methods for estimating stratospheric methane".

a) Give a quantitative proof of this statement (see remarks 4c, 4e) .

b) A paper in Discussion in AMTD (Wang et al., AMTD, 2014) is treating a similar subject, and by testing the two approaches of using HF and N2O (also available in TCCON measurements), reaches the conclusion that the N2O choice is improving the precision of the derived tropospheric CH4. Did you also test this N2O approach ? Do you see some arguments in favour of HF vs N2O ?

Technical / minor comments:

- Fig. 2 should appear as being "Fig. 1" since it is made reference to Fig. 2 before Fig. 1 in the text. (or introduce Fig. 1 before in the text).

- Fig. 1: add in the legend that data cover 2004-2012 period

- I would plot Fig 4, 5, 6 in a single figure 4a) to 4h). Also I would add a grid on these plots, to help visualizing the shift in seasonality from total columns to tropospheric

AMTD

7, C808–C812, 2014

columns.

- Figs. 8, 10, 11: I would remove the total columns in blue. They are not used anymore in the text, and this would enlarge visibility for tropospheric columns. (only suggestion)

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3471, 2014.

AMTD

7, C808–C812, 2014

Interactive Comment