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Comment

## ***Interactive comment on “Calibration of column-averaged CH<sub>4</sub> over European TCCON FTS sites with airborne in-situ measurements” by M. C. Geibel et al.***

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*Reply to the specific comments (original referee's comments in **bold**)*

### **Manuscript structure**

**The introduction should provide a clear and succinct up-front description of the remote sensing measurement process, the reasons why the calibration to the in situ trace gas measurement scales is needed, and how this calibration can be achieved using aircraft data. Introductory material from sections 4.1 and 5 should be moved here and reworked. The column-average dry air mole fraction should also be defined here or early in the 'FTS data processing' section.**

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The introduction will be rewritten and the material from 4.1 and 5 moved as suggested.

## Section 2: IMECC campaign

**There is insufficient detail on the the aircraft in situ data: What is the continuous in situ instrumentation? How is it calibrated? What are the statistics of the differences between flask and continuous analyser measurements? The scale used (NOAA04?) should also be noted explicitly.**

We did not want to repeat things that were already described in detail in Messerschmidt et al. 2011. However a paragraph describing the in-situ instrumentation will be added.

## Section 3: FTS data processing

**The text here is weak. Would it not suffice to say all data were processed in accordance with the TCCON data protocol, using the same version of the standard TCCON processing algorithms to transform the interferograms to spectra (including correction for variations in solar intensity) and perform the trace gas retrievals. It is important to note how the retrieval a prioris were adjusted (non-standard TCCON procedure). I would suggest the information on the GFIT a prioris (last paragraph of Section 4.1) and the adjustment procedure (section 4.2) be moved to section 3. Figure 6 does not provide enough information to be a useful illustration of the method, and should be revised or cut.**

The text will be shortened, the mentioned paragraphs moved and Fig. 6 removed as suggested.

**Regarding the discussion of the SIV correction, this is part of the standard TCCON data processing, so one wonders why non-standard and suboptimal procedures are discussed in some length here (a more useful discussion might have been the contrast between measurement precision (for retrievals using SIV correction) in clear and partially cloudy conditions i.e. how did sky conditions impact the calibration uncertainty?). .... On the other hand, we lack informa-**

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tion on whether pre- or post-processing QC based on solar intensity variations was applied? Information on the the observation time windows which were applied when defining the FTS-aircraft coincidences and post-retrieval QC which are given in the results section (5.1) would usefully be moved here.

Agreed, this section is not useful for the general reader. It will be reduced to a paragraph on cloud conditions during the campaign and the total effect of the applied QC.

**If for retrieval biases due to laser sampling errors have been empirically corrected, this should be noted/described here.**

The effects of these so-called ghosts were not corrected. The information on the amplitude and sign of these biases for the individual instruments was not known at the time of the data analysis. For the Jena instrument, it could not even be determined any more as the instrument was moved for a campaign in early 2010.

#### Section 4.1

**The definitions of the variables in the second term of the right hand side of the equation 1 are insufficient to determine how this term can dimensionally be a dry air mole fraction.**

Units for the variables will be added.

**It would be much clearer for the presentation of the methodology if the authors defined the 'smoothed tracer profile' which should be integrated to estimate the tracer vertical column (and then divided by the dry air total column to infer the column-average dry air mole fraction). An equation defining the calibration scale factor would also be useful (more useful say than paragraph beginning line 5 of page 1524). Similarly for a brief description of the calibration regression methodology (in a separate subsection if needed).**

We basically applied the same method to CH<sub>4</sub> that Wunch et al. 2010 and Messerschmidt et al. 2011 applied to CO<sub>2</sub>. Except for the discussion in Sect. 6, our terms,

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variables and equations are the same. It is not clear how the 'smoothed tracer profile' should be defined in this context. The smoothed aircraft DMF (smoothed with the FTIR averaging kernel) is defined but this is just a single number.

The calibration scale factor is introduced and defined in Sect. 5.1. It is not the right place to do this here. That is why that paragraph only has a short reference to that section so that the reader knows where to find the details.

**The wording of this section is very poor. The presentation would be simpler if the theory for the calculation of the FTS observation equivalent given a perfectly known tracer profile  $x_{true}$ , was described first, and issues arising with the approximation to  $x_{true}$  given by the aircraft data ( $x_h$ ) were discussed in what is currently section 4.3 (but will become 4.2 assuming the GFIT a priori discussion is moved to the present section 3). This would also help in the presentation of results in section 6 (see below).**

The wording will be improved but we would prefer to stay with the same definitions and equations as Wunch et al. 2010 and Messerschmidt et al. 2011.

## Section 5

**The material in the first paragraph of this section should be in the introduction.**

Will be moved as suggested (see above)

## Section 6

**I do not follow how the statement 'the aircraft column has to be extrapolated with a calibration-factor-corrected GFIT a priori profile' and the figure 13 can be reconciled with the formulation of equation 3 (which additionally repeats the error noted for equation 1). ...**

I think there is a major misunderstanding here, arguably caused by an unclear description of the iterative approach.

- $x_h$  is the extended aircraft profile (in Eq. 1 and 3). The case differentiation is not necessary as this has already been done when the measured aircraft profile was extended to calculate the total column. A note will be added to Fig. 7 and 13 that the extended profile  $x_h$  is shown there.
- all equations are the same as Wunch et al. 2010 with the exception of Eq. 3, which has been modified to take the calibration factor into account.
- the calibration factor  $\psi$  is retrieved from *all* profiles – not from a single one.
- Figure 11 shows an example of a single profile (among the others) that has been reduced in altitude coverage. Figure 11a) shows that following the original Wunch et al. 2010 procedure, this would lead to a bias of  $\psi$  towards one. Figure 11b) shows how this bias can be compensated by the iterative approach. Instead of being biased towards one,  $\psi$  approaches the value determined by the other (!) profiles.
- without any aircraft data (no information) the best guess for the calibration factor would be  $\psi = 1$ .
- Equation 3: for  $\psi_0$  we get the original equation from Wunch et al. 2011. The result is  $\psi_1 = \psi_{std} = 0.978$  which is the same as the one determined with the original Wunch et al. 2011 approach (p. 1526). It has been derived from *all* profiles. Further iterations produce  $\psi_n = 0.974$  which has also been derived from *all* profiles.

The iterative approach avoids biases caused by profiles that contain less information than others. It does not lead to non-sensical results. In the extreme case of a profile with zero vertical coverage (no information) the calibration factor will be determined from the other profiles only. In the original Wunch et al. 2011 approach, this zero-information profile would have biased the whole result towards one.

The text will be improved to make this clear and avoid the misunderstanding. We would like to thank V. Sherlock for her detailed comments on this issue.

### **Additional technical corrections**

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All technical corrections will be applied as suggested.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1517, 2012.

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