

***Interactive comment on “Remote sensing of CO<sub>2</sub> and CH<sub>4</sub> using solar absorption spectrometry with a commercial low resolution spectrometer” by C. Petri et al.***

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We thank Anonymous Referee #1 and #2 for the constructive comments on our manuscript. The text below gives all points raised by both reviewers followed by our reply.

Referee #1

*1. Throughout the manuscript, the reader lacks information that is required to follow the rationale and to potentially reproduce the presented results. In particular, section*

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**Table 1.** Standard GFIT retrieval windows for CO<sub>2</sub>, CH<sub>4</sub> and O<sub>2</sub>

Gas	Center of window wavenumbers[1/s]	Width wavenumbers[1/s]
CO <sub>2</sub>	6220.00	80.00
CO <sub>2</sub>	6339.50	85.00
CH <sub>4</sub>	5938.00	116.00
CH <sub>4</sub>	6002.00	11.10
CH <sub>4</sub>	6076.00	138.00
O <sub>2</sub>	7885.00	240.00

*3 must be extended: What are the spectroscopic databases used? What are the spectral ranges used? Is retrieved [O2] actually used to calculate XCO2, XCH4? What is the retrieval error plotted in Fig. 2 ff? Is it detector noise fed into error propagation? I recommend revising the manuscript with an eye on reporting duties, keeping in mind that the general reader might not be an FTS expert.*

Our retrieval software, GFIT, uses the HITRAN spectroscopic database. The spectral windows used for the analysis are the standard TCCON spectral windows. We added this information as shown in Table 1 in the revised manuscript.

There is no possibility to feed detector noise in the GFIT retrieval.

The errorbars shown in Fig. 2 ff result from the residuals of the individual fits for each single spectrum. These residuals are calculated as the difference of the measured spectrum minus the best simulated fit.

*2. Before evaluating performance for XCO2 and XCH4 (Fig.2 through Fig.5), the individual total columns [O2], [CO2], and [CH4] are to be evaluated in order to disentangle effects coming from [O2] and the actual target gases.*

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We did this, but we did not show this. O<sub>2</sub> and CO<sub>2</sub> agree within the error bars, expect for very high solar zenith angles (SZA) in the early morning and late evening. In the ratio this deviation is canceled out. CH<sub>4</sub> differs in the same way as XCH<sub>4</sub>, since O<sub>2</sub> agrees. This is now mentioned in the text.

*3. Retrieval errors related to uncertain knowledge of the instrument line shape (ILS) or errors related to spectroscopic line broadening parameters typically result in an airmass dependence of the retrieval parameters. The longer the atmospheric path, the larger the errors. Unfortunately, the study does not cover measurements for a large range of solar zenith angles (as far as I can judge, information on observation conditions is rudimentary) although ILS and spectroscopy related error sources are highlighted several times. The revised manuscript should cover a discussion on airmass dependencies.*

The measurements shown in Figures 2, 3, 6 and 7 have been selected to cover a large variety of SZA's, from sunrise to sunset, corresponding to a large variation in the airmass factors. We found that the results for CO<sub>2</sub>, CH<sub>4</sub> and O<sub>2</sub> show a slight dependency for very high SZA's and the ratios CO<sub>2</sub>/O<sub>2</sub> and CH<sub>4</sub>/O<sub>2</sub> do not show a significant dependency on the SZA. Furthermore the difference of XCO<sub>2</sub> and XCH<sub>4</sub> as measured by both instruments do not depend on the SZA. It should be mentioned that we did not eliminate spectra with large residuals, to show that even for spectra recorded at large airmass factors, and having larger residuals, the results of both spectrometers agree. We now discuss this in more detail in our revised version.

*4. A crucial step toward making a low-resolution instrument appealing for monitoring networks such as TCCON is stability and robustness. This is acknowledged by section 4.1 and 4.5 and the finding that the optical alignment plays a key role for retrieval accuracy of the low-resolution spectrometer. Unfortunately, there is mostly*

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*speculations but no effort to actually assess stability of the instrument in a systematic manner. The rather large residual patterns in figure 1 (lower panel) might suggest that alignment issues contaminate the retrievals. (Is the ILS asymmetric?) The revised manuscript should elaborate in more depth on this aspect.*

The alignment of the IFS-66 is a difficult issue. Especially it was found that the alignment has a dependency on the temperature. The room where the IFS-66 was installed was not heated. The temperature in this room changed over a day within  $\pm 10^\circ$  C. Therefore the alignment did change, our measurements of the modulation efficiency showed differences up to 2%, and for a 'bad alignment' the ILS becomes slightly unsymmetric. But we found that even with a 'bad aligned' spectrometer the results for XCO<sub>2</sub> for both instruments agreed within their error bars. Since a variability of 20 degrees in temperature is quite high, our results for XCO<sub>2</sub> show that the instrument is quite suitable for field campaigns. The differences for XCH<sub>4</sub> we assign partly to wrong a-priori profiles or non optimized line parameters. This is now explained in more detail in the text.

*5. The title emphasizes that the low-resolution spectrometer is a 'commercial' instrument. In my opinion, this is not a major scientific achievement. It should not be highlighted in the title of a research paper submitted to a scientific journal.*

We fully agree and have changed the title.

*Minor comments*

*p. 2, l.4: actually → currently*

Revised as suggested.

*p.3,l.4: It should be noted that the downside of total column measurements is their smaller sensitivity to surface fluxes compared to in-situ sampling.*

We agree and know about the differences between total column and in-situ surface observations. However, this is not an important point in our paper, and we did not use in-situ data. Therefore we did not mention this.

*p.3,l.13: obtain → to achieve*

Revised as suggested.

*p.3,l.14: missing  $\delta^{13}C_{CH_4}$ , named  $X_{CH_4}$*

Revised as suggested.

*p.3,l.20:  $\delta^{13}C_{VMRs}$  are achieved: Do you mean  $\delta^{13}C_{retrieved}$  columns might share systematic errors?*

Yes, a few errors will show up in the CO<sub>2</sub>, CH<sub>4</sub> and O<sub>2</sub> column. As an example, if our  $\theta_{sza}$  assumed is too low, may be the solar tracker looks on the upper part of the solar disc instead of focusing on the center, the calculated slant path airmass is too high, and the final vertical column is too low. Since O<sub>2</sub> is affected in the same way, and since the mixing ratio of O<sub>2</sub> is very constant the ratio of e.g. CO<sub>2</sub>/O<sub>2</sub> is nearly independently on a wrong  $\theta_{sza}$ . This holds also for other systematic errors. We now modified the text to make this more clear.

*p.4,l.7: remove "œnow"*

Revised as suggested.

*p.4,l.13: used to create → used to measure*

Revised as suggested.

*p.4,l.20: TANSO → TANSO-FTS onboard GOSAT*

Revised as suggested.

*p.4,l.20: Define acronyms.*

We now defined the acronyms.

*p.4,l.24: Except for SCIAMACHY, the other instruments resolve individual absorption lines.*

We agree, SCIAMACHY has a much lower resolution compared to the TANSO-FTS. The resolution of the TANSO FTS is 0.2 cm<sup>-1</sup>, comparable to our low resolution instrument.

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*p.6,l.19: fits → fit*

Revised as suggested.

*p.7, sec.4.1: Explain what modulation efficiency is and how it affects the ILS.*

This is now explained in more detail in the text.

*p.8, sec 4.3: With the information given, I cannot trace the numbers and I doubt that the calculation is complete. Elaborate on it or remove this section. Shouldn't the error bars in Fig. 2 and 3 essentially represent SNR?*

We did a simple estimation, based on the known optic and measurement parameters, like light throughput, field of view and the measurement times. This calculation essentially represents the SNR. We changed the text slightly to make this more clear.

*p.9, last line: GOSAT/TANSO has significantly better resolution than SCIAMACHY.*

Yes, we now mention this in the text.

*p.10,l.6: depending on the spectral band analyzed: Spectral bands have never been defined.*

See our first comment, we now include a table showing the spectral windows used.

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*p.11, sec.4.5: I do not understand the last sentence. Please rephrase.*

We did rephrase this sentence.

*Tab. 1: What is  $\text{XCO}_2(\text{O}_2)$ ? Is it  $\text{XCO}_2$ ? Avoid use of multiple flavors of notation.*

$\text{XCO}_2$  and  $\text{XCO}_2(\text{O}_2)$  is the same, we corrected the manuscript and do not mention  $\text{XCO}_2(\text{O}_2)$  anymore.

*Fig.1: upper figure  $\rightarrow$  upper panel. I guess  $\text{intensity}$  on the y-axis should be  $\text{transmittance}$ .*

Revised as suggested.

*Fig.2 and all other figures that follow: What is  $\text{CO}_2(\text{O}_2)$  (or similar)? Avoid use of multiple flavors of notation. The label of the y-axis are only units. The label should make clear what quantity is plotted.*

We did correct this. We show transmittance, and modified the Figures accordingly.

*Fig.8, 9: Panels are not labeled c) and d) while the text refers to  $\text{ec}$  and  $\text{ed}$ . The caption should make clear what the panels show.*

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We did correct this.

*Fig.9: The three lower panels seem redundant. Is there more information in it than just dividing the three lower panels of Fig.8 by the upper panel of Fig.8 ?*

We agree, the Figures are somehow redundant. But since always the ratios are used within TCCON we found it necessary to show also the ratios in a separate Figure.

*Referee #2*

*Pg. 247 Eqs (1) and (2): here the notation XCO<sub>2</sub>(O<sub>2</sub>) and XCH<sub>4</sub>(O<sub>2</sub>) have been introduced, whereas often, in the rest of the paper, the (O<sub>2</sub>) specification has been left out in most of the cases. It would have been more precise to state that in the rest of the paper, the specification (O<sub>2</sub>) is assumed silently and no longer written explicitly.*

XCO<sub>2</sub> and XCO<sub>2</sub>(O<sub>2</sub>) is the same, we corrected the manuscript and do not mention XCO<sub>2</sub>(O<sub>2</sub>) anymore.

*Sec. 4.1 and Sec. 4.5: it is not clear whether the ILS information that comes out from LINEFIT has been taken into account in the retrieval or not ?*

So far it is not possible to consider the measured ILS in the GFIT retrieval. This is now mentioned in the text.

*Sec. 4.2 pg. 251 lines 25-26: this statement saying that the errors bar in Figs. 2 and 3 represent the typical one sigma variability in one day, is in contradiction with what is*

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*stated in the Figure captions themselves, namely that the error bar represents a single measurement uncertainty.*

We corrected this. The error bars shown in the Figures 2 and 3 represent a single measurement uncertainty as denoted in the Figure captions. The errors denoted in the Tables 1 and 2 represent the one sigma variability for daily means.

*Pg. 252 line 12: I don't understand how a bias of 0.15% survives after you have subtracted the bias ? Or what else do you mean by "After subtracting this offset, the average deviations of both instruments is 0.15%..."? Maybe average deviation points to 1-sigma standard deviation ?*

We subtracted a bias, which is the average for all measurement days after the alignment. Therefore the individual days still have a positive or negative bias, which is given as a mean of absolute values in the tables.

*Tables 1 and 2: It would be good to also in Table 1 make the average for the days with bad alignment and the days with corrected alignment separately (as done for XCH4 in Table 2), to really demonstrate that there is no impact of the alignment on XCO2. Explain in the caption how the difference (last column) is defined Explain in the caption of Table 2 how "Diff offset" is defined. Why is the day of April 4, 2009 from Figs. 6 and 7 not included in this Table ? It doesn't appear either in the Figs. 4 and 5, why ? Similar question for 4 September 2009 (Figs. 8 and 9) ?*

We have added this in the table. See also comment to reviewer #1. As mentioned in the text, we don't have measurements from the IFS 66 before November 2009. The truncation-test was only made with spectra taken by the IFS 125 HR and a day with a

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large amount of measurements was chosen.

*Eqs. (3), (4) and (5): there is a problem with the numerical values. Pg. 253, l. 17:  $\hat{\epsilon}_{IJ}$  : offset of 0.26% in agreement with the estimation given above  $\hat{\epsilon}_{IJ}^{TM}$ , which corresponds to 0.46% if I understand correctly. So where is the agreement ? Pg. 253, l. 24: is the so-called variability the one-sigma standard deviation between all measurements in 1 day ? It would be good to add the meaning of the error bars in the Figure captions themselves.*

We agree with this comment. However, our calculations in Figure 8 go in the right direction, but we do not get an full agreement. This is now mentioned in the text. The remaining 0.2% are within the uncertainties of XCH4. The bars in Fig. 8 and 9 just gives the numbers on a percentage scale, to indicate the variability expressed in percent. There are no error bars in these figures. This is now mentioned in the Figure caption.

*Figs. 8 and 9 and corresponding text pg. 253-254: The text discusses the variations in percentage values, whereas the figures show absolute values; therefore it is difficult to make the link. It would be good to add the absolute values between parentheses in the text.*

We feel that the values given in percent is much more appropriate to estimate the variability. Therefore we did not give the absolute numbers.

*Sec. 4.4 pg 254: the way that the authors deal with the influence of the a priori profile is very elementary: they just shift it down and observe that the sensitivity of the retrieved column to the resolution is less (smallest) for a downshift of 4 km. First,*

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*they should at least try to explain why the dependence of the retrieved CH<sub>4</sub> columns on the resolution is less with a "correct" a priori than with an erroneous a priori. But there is more: is it realistic to believe that the downshifted profile is better? The CH<sub>4</sub> profile essentially depends on the tropopause height and this is known better than within 4 km (and does not change by 4 km from one day (season) to another). So I have my doubts about the fact that the downshifted profile is a better a priori profile (unless the initially chosen a priori profile was really inappropriate) and that this is the reason why the sensitivity is less important. Moreover, another suggested reason for the sensitivity of the CH<sub>4</sub> columns to the resolution is errors in the spectroscopy. If this would be the case, then why should we trust the CH<sub>4</sub> column retrieved with the high-resolution instrument, and why would the sensitivity to the resolution level off at a sufficiently high resolution? How does the investigated range of resolutions compare to the intrinsic linewidth of the observed CH<sub>4</sub> line(s)? The authors should give more insight in this argument.*

We agree that a shift in the a-priori is unrealistic. However, in this way the modification of the a-priori is easy to understand. Also, the easiest way to consider changes in the tropopause height is to shift the profile up or down. We agree that the CH<sub>4</sub> spectral data might be wrong, but this is beyond the scope of this paper. However, we mention this now in the text.

*Sec. 4.5, last sentence: why would the column offset with a misaligned instrument depend on the appropriateness of the a priori profile? In other words, how is the misalignment (partly) compensated in the retrieval if the a priori is suitable? The paper does not really explain this observation; the conclusion is rather empirical: : : This is difficult to understand, especially since we don't know whether the retrieval accounted for the misalignment using the output from LINEFIT.*

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This is explained in detail in comments to reviewer #1, see page 3 and page 5.

*Sec. 5. Pg. 256 line 5: It seems to me that you cannot exclude  $\hat{\epsilon}^i$  with the material you have - an influence of the interferences with H<sub>2</sub>O on the offset between the high- and low-resolution measurements of XCH<sub>4</sub>, even if you do not observe an anti-correlation between the behaviours of the XCH<sub>4</sub> and the H<sub>2</sub>O concentration.*

We agree, and deleted this sentence.

*Figs. 3, 7 and Fig. 5: in Figs. 3 and 7, the IFS66 measurements for XCH<sub>4</sub> are higher than the IFS125HR measurements; in Fig. 5 it looks the other way around . In fact, if you account for the error bars, the offset is not significant in most of the cases. (It would be good to add the error bars to Fig. 7). This is also what you see in Table 2: for the well-aligned instrument,  $\hat{\epsilon}_{\text{Difference}}^{\text{TM}}$  is generally smaller than  $\hat{\epsilon}_{\text{sigma}}^{\text{TM}}$  for the IFS66, and the same is true for the averages. So does the whole discussion about offsets make sense ???*

After the alignment XCH<sub>4</sub> in Fig. 5 for the IFS 125HR is higher, compared to the IFS 66, in agreement with Figs. 3 and 7. The confusion might erase because before the alignment the situation in Fig. 5 is vice versa.

*Fig. 6 and 7: what happens at the end of the day? The VMR values decrease drastically.*

For very high SZA ( $\geq 85^\circ$ ) the errors increase drastically, due to the very low solar intensity.

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*Fig. 9: The standard resolution of the TCCON measurements is 0.02 cm<sup>-1</sup>, corresponding to OPD=45 cm. It is not clear from Fig. 9c that XCH<sub>4</sub> is already in the stable region. It would be good that the paper comments on the appropriateness of the choice of the spectral resolution in TCCON.*

We fully agree with this comment, and have included this into the text.

#### *Technical comments*

*Pg. 247, l. 23: TCCON stands for Total Carbon Column Observing Network, not "Observatory"™ Ibidem: the word "Jor"™ is missing in front of the acronym TCCON.*

Revised as suggested.

*Pg. 248, l. 19: use "traveling standard"™ instead of "travel standard"™*

Revised as suggested.

*Pg. 251 l. 21: add "JOPD"™ after "J8.1 cm J"*

Revised as suggested.

*Pg. 252 line 6: "comparable"™ instead of "comparability"™*

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