

Interactive comment on “Remote sensing of CO₂ and CH₄ using solar absorption spectrometry with a commercial low resolution spectrometer” by C. Petri et al.

Anonymous Referee #2

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General comments

The paper investigates whether low resolution infrared spectrometers of the Fourier transform type (FTIR, commercially available) can achieve a precision and accuracy that are similar to the ones achieved by the high-resolution Bruker 125HR FTIR spectrometer which is the current standard in the Total Carbon Column Observing Network (TCCON). It studies this question separately for CO₂ and CH₄, the present standard products of TCCON.

The question investigated in the paper is a very relevant question and it is addressed properly.

A general remark however is that the number of measurements days (9) and thus the statistics are very limited for drawing general conclusions. The question to what extent the conclusions depend on the specific measurement site (level of humidity, altitude, . . .) is not addressed at all, and still, the conclusions are presented as if they are valid in general: the authors should comment on the overall validity of their conclusions. Moreover the authors do not investigate in depth the reasons that they suggest for the observed differences between the two instruments regarding the observed precision of the CH₄ and CO₂ measurements and their sensitivity to misalignment, a priori profile, interferences with H₂O, etc.: they just make a few suggestions but do not make much effort to verify or justify those suggestions. More effort in this respect is highly recommended. The good points of the paper are that the study has been performed with the two types of spectrometers operating simultaneously, and that it has been performed at Bremen where the interfering water vapour signatures are not negligible, as would have been the case at a high-altitude site, and so this effect can be investigated.

Specific comments and questions

Pg. 247 Eqs (1) and (2): here the notation XCO₂(O₂) and XCH₄(O₂) have been introduced, whereas often, in the rest of the paper, the (O₂) 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 (O₂) is assumed silently and no longer written explicitly.

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 ?

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

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

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average deviations of both instruments is 0.15%...'? Maybe average deviation points to 1-sigma standard deviation ?

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) ?

Eqs. (3), (4) and (5): there is a problem with the numerical values. Pg. 253, l. 17: '...offset of 0.26% in agreement with the estimation given above', 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.

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.

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, they should at least try to explain why the dependence of the retrieved CH₄ 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 CH₄ profile essentially

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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 CH₄ columns to the resolution is errors in the spectroscopy. If this would be the case, then why should we trust the CH₄ 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 CH₄ line(s) ? The authors should give more insight in this argument.

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 apriori 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.

Sec. 5. Pg. 256 line 5: It seems to me that you cannot exclude – with the material you have - an influence of the interferences with H₂O on the offset between the high- and low-resolution measurements of XCH₄, even if you do not observe an anti-correlation between the behaviours of the XCH₄ and the H₂O concentration.

Figs. 3, 7 and Fig. 5: in Figs. 3 and 7, the IFS66 measurements for XCH₄ 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, 'Difference' is generally smaller than 'sigma' for the IFS66, and the same is true for the averages. So does the whole discussion about offsets make sense ???

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

Fig. 9: The standard resolution of the TCCON measurements is 0.02 cm⁻¹, corresponding to OPD=45 cm. It is not clear from Fig. 9c that XCH₄ 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.

Technical comments

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

Pg. 248, l. 19: use 'traveling standard' instead of 'travel standard'

Pg. 251 l. 21: add 'OPD' after '8.1 cm '

Pg. 252 line 6: 'comparable' instead of 'comparability'

Fig. 1 caption, last line: dot after IFS66 should be deleted.

Fig. 3 caption: '..for the 9 March 2010. . .': the word 'the' must be deleted.

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