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Atmospheric Measurement Techniques Discussions

Interactive comment on "First intercalibration of column-averaged methane from the Total Carbon Column Observing Network and the Network for the Detection of Atmospheric Composition Change" *by* F. Forster et al.

Anonymous Referee #2

Received and published: 1 April 2012

The authors submitted a manuscript describing a method to intercalibrate MIR measurements done within the NDACC and NIR measurements done within the TCCON.

GENERAL COMMENTS:

The manuscript is an important contribution for both networks TCCON and NDACC. However, the used methods should be better described and the findings quantified. For example, in my opinion Figure 3 shows still a seasonality. The authors should come up with a benchmark to quantify the seasonality. Furthermore, the authors do not argue why they can assume a zero intercept in Figure 4. By forcing a zero intercept, the linear fit is dominated by the artifical datapoint in (0/0), because the measurements group around 1700 - 1800 ppb.

The authors motivate the intercalibration by the possibility to merge the NIR and MIR datasets. However, the authors never show a calibrated final dataset. It would be good to include a figure with a final dataset for Garmisch and Wollongong, calibrated with one global scaling factor. By now only the Garmisch dataset is shown, only calibrated with the site specific calibration factor.

The authors mention a seasonality in the difference timeseries between MIR and NIR measurements. It remains unclear to me how the standard NDACC and TCCON data products (retrieved with the standard a priori) can be intercalibrated with one global calibration factor, when the difference features a seasonality?

The Figures should be revised in regards of axis labeling (e.g. Does 2008 mean January 2008?) and errorbars should be applied. Additionally, the Wollongong dataset should be shown as well.

SPECIFIC COMMENTS:

1. page 1356, line 9: "shows a phase shift in XCH4". The authors mention a phase shift. In my opinion, Figure 2-4 show a time dependent bias, but not a phase shift. A phase shift would be a constant shift in time. The authors mention the phase shift only once. Throughout the text they speak about a time dependent bias. If the authors are convinced that it is a phase shift, it should be better explained or shown by a cross-correlation.

2. page 1356, line 14: "The difference time series [...] do not show a significant trend". This is a statement, which should be better shown and quantified.

3. page 1357, line 24: "They are representative of a much wider area". It is unclear to me what is meant with "wider area". Additionally a citation should be given, e.g.

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Interactive Comment

Keppel-Aleks, G., P.O. Wennberg, and T. Schneider, (2011), "Sources of variations in total column carbon dioxide", Atmos. Chem. Phys, 10.

4. page 1357, line 25: sententence "In situ measurements are more directly traceable [...] while column measurements provide the same quantity as satellites [...]. Why do the authors list these characteristic in contradiction? It is unclear to me what they try to argue for or why they list these characteristics. If they want to list the advantages and disadvantages, it is not a complete description.

5. page 1360, line 11: Why do Garmisch and Wollongong measure with different path differences? Does the path difference have an influence on the results?

6. page 1360, line 11: Why does Wollongong average only 2 scans, but Garmisch 6 scans?

7. page 1360, line 22: Do the authors mean Wunch et. al. (2010) or Wunch et. al. (2011a)? I would suggest to cite the TCCON paper (Wunch et. al., 2011a).

8. page 1360, line 17-22: The paragraph about the retrieval strategies is rather short. Do the authors apply the TCCON calibration factors? Do they exclude data due to flagging? Additional information on the retrieval strategies should be given, at least basic differences of the retrieval strategies. SFIT and GFIT are totally different methods and the authors should line the differences out.

9. page 1362, line 2: In my opinion the authors should show a figure of the averaging kernels, instead of citing other publications, because differences in the a priori estimates play a major role in this publication.

10. page 1363, line 10: The authors state that the bias can be attributed to differing spectroscopy. This is a statement that should be argued. Why do they think so? Which spectroscopy? In the MIR or the NIR or both? If the bias between MIR and NIR is due to spectroscopy, why does the difference still show a seasonality? How can this be explained? The authors calibrate the MIR to the NIR, because the TCCON

measurements are already calibrated to WMO standards. Are the calibrated NIR measurements shown, and if yes, which calibration factor was applied (Wunch et al., 2010 or Geibel et al., 2011)? If the NIR measurements are calibrated, is the bias then due to the MIR spectroscopy?

11. page 1363, line 13: The authors speak of "column uncertainty". It is more correct to speak of "absolute accuracy". Additionally, in my opinion the systematic biases in the spectroscopy are not the reason for the calibration campaigns, but a result of the calibration campaigns. In the TCCON calibration campaigns, it could be shown that the a priori information do not add a bias to the data, and therewith the bias was attributed to spectroscopy. Furthermore, the authors should argue why in their method the a priori information add a bias, but not in the TCCON calibration. Does this mean that the MIR a priori information cause the bias?

12. page 1363, line 20: Wunch et al. (2010) and Geibel et al. (2011) come up with two different calibration factors. The authors should at least discuss these differing results and should state which calibration factor they apply.

13. page 1364, line 9: "Also [...]" For Garmisch the uncertainty increases, but for Wollongong it decreases. This should at least be mentioned.

14. page 1363, line 22: The authors force a zero intercept in their calibration method. By this, they create an artificial data point and assume that nothing would be measured in the NIR in case nothing is measured in the MIR. This should be further discussed, especially because this datapoint (0/0) dominates the linear fitting, because the measurements group around 1700-1800 ppb.

15. page 1365, line 14: Why do the authors not apply this method now to further sites?

16. page 1378, Figure 3: In my opinion the seasonality is muted, but not elimated, especially for 2008 and 2010. The authors should come up with a method to quantify the time dependent differences.

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17. page 1373, Table 1,2, A1: Why do the authors show the calibration factor uncertainty with 3 \sigma and the uncertainty of the trend NIR/MIR with 2 \sigma. The methods and the error estimation should be better explained.

18. page 1375, Table A1: What is the Garmisch trend for the same time period like in Wollongong? How was the trend estimated? The authors should explain their method.

19. Figure 1-3: Please change x-axis to at least half-year values. It is not clear what "2008" means. Is it January 2008? Please apply errorbars to the data points. Why are the data in Figure 2 and 3 scaled with the Garmisch calibration factor? The authors argue that one single global calibration factor can be used, but they use the site specific value.

20. Figure 1 shows the same information content like Figure 2/3. It could be replaced by a new figure, that shows an estimate of the seasonality in Figure2/3 (for example a cross-correlation).

21. Figure 3. The residual shows the residual of the non-scaled values and not the residual of the shown data in the lower panel. This was confusing to me.

22.The authors should show the same findings for the data in Wollongong. Do the Wollongong data show the same seasonality?

23. Figure 4: The authors should include errorbars, and should explain their method better. Why can they use a zero intercept? By this they invent an artificial datapoint (0/0) that has a big impact.

TECHNICAL CORRECTIONS:

1. page 1356, line 3: Write out at least once the abbreviation "FTIR"

2. page 1358, line 11: "(CO2,CH4,N2O)" -> "(e.g. CO2,CH4,N2O)"

3. page 1358, line 18: include publication, Keppel-Aleks et al., 2012: Keppel-Aleks, G., Wennberg, P. O., Washenfelder, R. A., Wunch, D., Schneider, T., Toon, G. C., Andres,

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R. J., Blavier, J.-F., Connor, B., Davis, K. J., Desai, A. R., Messerschmidt, J., Notholt, J., Roehl, C. M., Sherlock, V., Stephens, B. B., Vay, S. A., and Wofsy, S. C.: The imprint of surface fluxes and transport on variations in total column carbon dioxide, Biogeosciences Discuss., 8, 7475-7524, doi:10.5194/bgd-8-7475-2011,

4. page 1358, line 21: include publication, Washenfelder et al., 2006: Washenfelder, R. A., G. C. Toon, J.-F. L. Blavier, Z. Yang, N. T. Allen, P. O. Wennberg, S. A. Vay, D. M. Matross, and B. C. Daube (2006), Carbon dioxide column abundances at the Wisconsin Tall Tower site, Journal of Geophysical Research, 111(D22), 1-11, doi:10.1029/2006JD007154. Available from: http://www.agu.org/pubs/crossref/200...JD007154.shtml

5. page 1358, line 22: 15 sites?, In my opinion at least 20 sites?

6. page 1359, line 14: "[...] TCCON network" -> "[...] TCCON"

7. page 1359, line 14: "[...] TCCON network and is based on a Bruker [...]" - change "based"

8. page 1359, line 20: change "were recorded" - "are recorded"

9. page 1359, line 21: change "were averaged" - "are averaged"

10. page 1360, line 8: rewrite sentence "[...] render [...] possible"

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1355, 2012.