

***Interactive comment on* “Field inter-comparison of two high-accuracy fast-response spectroscopic sensors of carbon dioxide” by B. A. Flowers et al.**

Anonymous Referee #3

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Review of Flowers et al. “Field inter-comparison of two high-accuracy. . . .”

Journal: AMT Title: Field inter-comparison of two high-accuracy fast-response spectroscopic sensors of carbon dioxide Author(s): B. A. Flowers et al. MS No.: amt-2011-135 MS Type: Research Article Special Issue: Carbon dioxide, other greenhouse gases, and related measurement techniques - 16th WMO/IAEA meeting (GGMT-2011)

This paper describes field and laboratory comparisons of two CO₂ analyzers based on recent technological advances. Such comparisons are very useful and this paper has the potential to be a valuable contribution to the field with some general clean up and some changes to the presentation of the results.

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General comments:

1) The most relevant metric for CO₂ analyzers deployed to make concentration measurements that will be compared in regional to global networks to measurements by other systems is compatibility (see WMO report from 15th CO₂ Experts meeting for definition: http://www.wmo.int/pages/prog/arep/gaw/documents/GAW_194_WMO_TD_No_1553_web_low_resolution.pdf (this is related to accuracy and comparability, but not exactly the same), and is much more important in the context of networks than short-term precision. One way to assess compatibility is through direct comparisons such as those described here, but the results also need to be presented in a way that allows compatibility to be assessed, for example as included in numeric form in the last paragraph (0.04+/- 0.4 ppm) and as not included in a timeseries plot of the differences. I do not know how to translate the correlation and ratio plots such as Fig. 1 and Fig. 4, and correlation and ratio numbers as given in the text, into a meaningful assessment of how these analyzers would perform in terms of compatibility. I suggest either removing these plots or adding text clarifying how they should be interpreted in terms of instrument appropriateness for specific applications, and I strongly suggest adding a timeseries plot of the differences between two measurements – are the differences random or do they vary systematically over time? Does their behavior provide any clues to their origin? In addition to a plot over the full 19-days, zoomed in plots of differences over shorter periods may be useful (e.g. another panel could be added to Fig. 3 showing the differences – as it is one can not see differences in this plot).

2) +/- numbers are not useful unless the statistical and time basis is also specified – state whether they are 1-sigma and over what time time unit (e.g. 1-Hz, 1-min, 1-hr) they have been calculated

Specific comments:

1) Page 5838 line 4 and page 5839 line 10: compatibility would be more appropriate

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than accuracy (see gen. comment 1)

2) Page 5838 lines 7-9: the mean values are not relevant/useful as the variability is too high to compare on this basis (the mean difference however is highly relevant). Also, see gen. comment 2.

3) Page 5838 line 10: say relevance of regression number (e.g. implies both sensors are linear)

4) Page 5838 line 11: this is absolute precision, not relative precision (relative precision would require dividing by ~ 380 ppm)

5) Page 5838 line 17: say relevance of ratio, say whether +/- is 1-sigma

6) Page 5839 line 4: “relative precision” is not particularly important, compatibility (and stability, linearity) are more so.

7) Page 5839 lines 5-7: the WMO recommendation is now for compatibility not comparability (cite most recent report linked above). Also, recommendation is for total CO₂, not 12CO₂ as stated here. Also delete “between operational techniques” as this is redundant with first part of sentence and not what is specifically recommended.

8) Page 5840 section 2: cite 13C effect studies: Lee, J.-Y., Yoo, H.-S., Marti, K., Moon, D.M., and Lee, J.B.: Effect of carbon isotopic variations on measured CO₂ abundances in reference gas mixtures, *J. Geophys. Res.*, 111, D05302, doi:10.1029/2005JD006551, 2006. Tohjima, Y., Katsumata, K., Morino, I., Mukai, H., Machida, T., Akama, I., Amari, T., and Tsunogai, U.: Theoretical and experimental evaluation of the isotope effect of NDIR analyzer on atmospheric CO₂ measurement, *J. Geophys. Res.*, 114, D13302, doi:10.1029/2009JD011734, 2009.

9) Page 5840 line 16: state height of trees and density of forest.

10) Page 5840 line 21: clarify whether any user calibration had been applied before determination of this linearity number

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- 11) Page 5841 line 2 and line 17: state where reference gases were obtained and who assigned the concentrations, and what scale (e.g. WMO X2007) they are on. Are they natural air or synthetic air? If the latter, do they include oxygen and argon or not?
- 12) Page 5841 lines 10-13: briefly state what this procedure is. Also, consider moving this to the methods section.
- 13) Page 5841 line 19: where do these +/- numbers come from and what do they mean? They are smaller than the quoted compatibility of NOAA tanks. Also, how was 12CO_2 determined from NOAA's (total CO_2) numbers?
- 14) Page 5841 line 28: why are these forced values different than the earlier quoted values for the high and the low reference?
- 15) Page 5841 lines 28-29. What are the +/- values? 1-sigma? In the case of the unknown over what time basis? How is the RSD calculated? Deviation from what?
- 16) Page 5842 line 3: "same statistics" is ambiguous. State what is actually being reported here.
- 17) Page 5842 line 16: explain timing more clearly. Was a 1-time calibration applied to the CRDS before deployment and then no recal done for 19 days? Was a post-cal done or applied?
- 18) Page 5844 line 1: calibration is for accuracy (or compatibility) not precision.
- 19) Page 5844 lines 18-24: See general comment 1. The authors should discuss the relevance of these regression and ratio numbers.
- 20) Page 5844 line 27 – Page 5845 line 28. The suggestion that the instrument clocks leads to differences is surprising/interesting, but the authors should state what the volumes are of the sample cells and any other mixing volumes and then discuss how different integration times for the two instruments might also contribute to timing issues. Perhaps the optimization that is being done is just compensating for different smoothing

functions. Also, the instruments could spit out data in real time that could be logged on a common system to resolve whether it really is an issue of the instrument clocks moving back and forth (typically they just drift in one direction).

21) Table 1: the deltas are surprisingly large – are both sensors averaging exactly the same amount of data or did one or the other sensor have missing data? The very large offsets around 6 AM further suggest that a mixing volume effect is causing apparent offsets when CO₂ is changing rapidly.

22) Page 5845 lines 5-7: are the authors saying that the two instruments disagree by a constant value of 3.7 ppm? It looks like from the lack of primes on the CRDS variable that the uncalibrated values are being discussed for some reason – if this is the case it should be stated more clearly. Also, the difference between time-adjusted and non time-adjusted comparisons would be easier to interpret with the calibrated numbers and as differences rather than regressions or ratios.

23) Page 5845 line 26: it is not clear how to interpret the 1.8 ppm mean difference in light of the 0.1 ppm compatibility goals, but given the high variability I suspect it is not relevant

24) Page 5846 line 2: it is also not clear how to interpret the correlation factor – is this a good or bad number? What should we expect to see?

25) Page 5847 line 4: state whether the +/- 0.4 ppm is 1-sigma and over what time basis it is calculated (minutely, hourly?)

Minor comments (no responses required):

1) Pg. 5838 line 2: “summer and fall” implies much longer than 19 days – suggest giving actual dates.

2) Page 5838 line 22: this paper deals with concentration measurements (that can be used to make flux estimates) not direct flux measurements - suggest change of wording

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- 3) Page 5839 line 16: state common inlet used
- 4) Page 5839 line 17: protocols missing s
- 5) Page 5839 lines 2-4: redundant with lines 17-21
- 6) Page 5839 line 20: techniques missing s
- 7) Page 5840 line 10: add “for extended periods of time” - even Picarro acknowledge that for CO₂ network applications cal gases are needed.
- 8) Page 5840 line 13: specify dew point or H₂O concentration of drying
- 9) Page 5840 line 16: page 5844 line 6 says 5 m – make consistent
- 10) Page 5840 line 25: “in situ and with FTS” is ambiguous – move “in situ” out of parentheses and say “in conjunction” or “collocated” with an FTS instead – otherwise sounds like CRDS is doing the FTS measurement
- 11) Page 5841 line 3: 2% is 7-9 ppm
- 12) Page 5841 line 6: insert “were” before “purchased”
- 13) Page 5841 line 7: insert “test of” before “response” and replace “were” with “was”
- 14) Page 5842 lines 4-5: why do these numbers have an extra digit of resolution?
- 15) Page 5843 lines 20-24: this was already stated.
- 16) Page 5843 lines 22-25: put in methods section
- 17) Page 5844 line 13: replace “signatures” with “signals”
- 18) Page 5845 line 20: what to “top trace” and “bottom trace” refer to?
- 19) Page 5847 line 6: insert “compatibility” before “recommendation”
- 20) Fig. 5b y-axis label needs a “TDL”

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