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Atmospheric Measurement Techniques Discussions

Interactive comment on "An airborne amplitude-modulated 1.57 µm differential laser absorption spectrometry: simultaneous measurement of partial column-averaged dry air mixing ratio of CO_2 and target range" *by* D. Sakaizawa et al.

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Dear Anonymous referee,

We thank referees for careful reading our manuscript and for giving useful comments.

We have revised the manuscript on the basis of the Referee's comments.

We look forward to a publication of our manuscript in AMT.

Sincerely,
Daisuke Sakaizawa

‡ **Bold fonts** means review's comment.

Overall, this is a good manuscript that describes use of a new instrument to observe CO_2 partial column densities and range over which those columns are measured. That information is critical for determining patterns of CO_2 emission and biological uptake. In general, the manuscript reads well and the conclusions appear sound. The largest criticism I would have with this manuscript is that the choice of comparing delta-tau calculated to observed is not really the comparison that one would scientifically want. From examining the experimental data, one sees that delta-tau is pretty much a function of target range (pathlength). That makes sense because CO_2 is fairly well mixed, but the desired observation is that of the CO_2 mixing ratio, not the fact that its optical absorption depends upon the path. Shouldn't it be possible to combine the information from Figs. 5 and 6, which contain delta-tau and range distance and then create a plot of the CO_2 mixing ratio as a function of position along the flight track? That spatial information is the goal of this technique, but is not shown in the manuscript. Specifically, the period of time around 1230 LT on the data shown in Fig 5 seem to show differences between delta tau and height,

which if real seem to indicate differences in dry air CO_2 mixing ratio. What was being done in the flight during this period, and is it reasonable that CO_2 was different?

Answer:
In accordance with reviewer's comment, we rewrote Fig. 8 by using XCO_2 instead of $\Delta\tau$.

We note that XCO_2 obtained from LAS had to be compared with in-situ XCO_2 using the reliable atmospheric parameters. The areas where in-situ and atmospheric parameters taken by radiosonde were available only comprised the level flights during decent spiral measurements.

However, we agree that additional information on XCO_2 along the track as suggested by the reviewer would be quite valuable. We are now investigating this consideration and intend to report our results in a later paper.

Please refer to supplement file (amt-2012-100-supplement.pdf) for revised Fig. 8, Fig. 5, and Fig. 6.

Some specific comments to the manuscript are described below:
p.4851, Title: I believe they are describing a "spectrometer" not a "spectrometry"

Answer:
In accordance with the reviewer's comment, we have changed "spectrometry" to "spectrometer".

p4852, line 17:
This section reads somewhat awkwardly and appears to need some references. For instance, the first two sentences seem to need references. The choice of this

spatial resolution becomes clear later, but is not clear here. Overall, this section could be reorganized to read a bit more clearly.

Answer:
We agree with the relevance and have added references.
"A global carbon cycle study using higher spatial resolution than an $8^\circ \times 10^\circ$ grid is currently required to improve the knowledge of the carbon cycle (Rayner and O'Brien, 2001; Baker et al. 2011). Transport models and observational data sets improve evaluations of regional carbon fluxes (Maksyutov et al. 2008)."

p4854, l5: Again, the instrument is a "spectrometer" in LAS

Answer:
In accordance with the reviewer's comment, we have changed "spectrometer" to "spectrometry".

p4854:, l20: the terms "onc" and "one" are quite easy to confuse. Maybe it could use a last capital letter for clarity?

Answer:
In accordance with the reviewer's comment, we have changed "λ_{onc}"/"λ_{one}" to "λ_{center}"/"λ_{edge}".

p4856, l22: The text says "Therefore, in the case of Table 1, the difference of $\Delta\tau$ of highly distributed CO_2 at lower altitude to $\Delta\tau$ " The expression "highly distributed" is awkward here. I think that a description of the profile might be more accurately said to be "a boundary-layer enhanced CO_2 profile". In the table, it should be made more clear if the profile is a "box profile" (e.g. 410ppm from ground to 1km followed by 398ppm from 1-2km and 385ppm above 2km) or a

linear interpolation between the described points.

Answer:
We agree that this point requires clarification.
I calculated linear interpolation between the 410 ppm on the ground, 398 ppm at the 1 km, and 385 ppm above 2 km. This assumption does not correspond to well-mixed and a boundary-layer enhanced CO_2 profile. Therefore, we have corrected the text and table as below:

Table 1. Estimated $\Delta\tau$ and sensitivity of lower altitude CO_2 for the center and edge wavelength. Atmospheric parameters are based on the AFGL mid-latitude winter. Two CO_2 vertical profiles (boundary-layer enhanced and constant along height) were assumed: for one, the CO_2 mixing ratio was constant at 385 ppm along height; for the other, the mixing ratio was 410 ppm from the ground to 0.5 km, 398 ppm from an altitude of 0.5 km to 2 km, and 385 ppm at an altitude above 2 km altitude, respectively.

	385 ppm constant		Urban area	
	Center	Edge	Center	Edge
$\Delta\tau$ at 7 km	0.970	0.261	0.975	0.266
XCO_2 at 7 km	385.0	385.0	387.2	398.9

Adding to this, we corrected the text on p. 4856 line 23:
"Therefore, in the case of Table 1, the difference of XCO_2 at a boundary-layer enhanced CO_2 profile is $+3.9$ ppm for the λ_{edge} $+2.2$ ppm for the λ_{center}."

p4856, l28: While it is true that the line edge position (one) is better at observing surface CO_2 due to the pressure broadening effect, the dependence of the signal at "one" on wavelength is much more severe than the onc position. Can the authors discuss that the variation on signal due to laser wavelength stability is sufficient to allow "one" measurements to observe surface CO_2 fluxes without

extra noise due to laser wavelength noise? This point is discussed on p4861, l5. The text could clarify that this information is calculated later.

Answer:
We agree that this point requires clarification.

Our proto-type LAS is optimized for ground-based measurement (long integration time is available due to low transmitting power and small receiving aperture) not for airborne measurement (speedy moving platform and a higher transmitting power). Our current proto-type (optimized for ground-based measurement) employs a wavelength stability of less than 12 MHz (corresponds to precision of 0.58 % at edge wavelength). The edge wavelength stability for a moving platform has to target an absolute precision of less than 1 MHz ($1\sigma < 300$ kHz). The system with targeted wavelength stability reduces the error due to an edge wavelength stability of less than 0.03 %.

We have added the text on p. 4856 line 24:
"For this purpose, the wavelength stability on λ_{edge} has to target an absolute precision of less than 1 MHz ($1\sigma < 300$ kHz). The system with targeted wavelength stability reduces the error due to the stability of the laser wavelength less than 0.03 %."

p4858, lines 8 and 10. In one case microradians is used and milliradians in the other. Make the two units the same (e.g. 0.12 milliradians transmitted, 0.20 milliradians receied).

Answer:
We have corrected this mismatch in accordance with the reviewer's comment.

p4864, l6-8: The final "conclusion" is really not a conclusion of this paper. The sentence before is a conclusion of what was done here, but the extension to a space-based platform was not calculated in this manuscript. It may be true, but

either the manuscript should show it to conclude it or this sentence should be moved to a discussion section or other.

Answer:
We agree that additional information on space-borne systems is required, as suggested by the reviewer. However, we would like to intend the performance evaluation of aiborne measurement using our LAS system in this paper. We will intend to report our results in a later paper. Therefore, we have corrected the text as follow:
"Our proto-type LAS, which is engineering designed, will be based on a near-future spaceborne system."

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/5/C2990/2012/amtd-5-C2990-2012-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 4851, 2012.