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Comment

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.

Anonymous Referee #2

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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

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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 CO₂ is fairly well mixed, but the desired observation is that of the CO₂ 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 CO₂ 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 CO₂ mixing ratio. What was being done in the flight during this period, and is it reasonable that CO₂ was different?

Some specific comments to the manuscript are described below:

p4851, Title: I believe they are describing a "spectrometer" not a "spectrometry"

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.

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

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

p4856, l22: The text says "Therefore, in the case of Table 1, the difference of $\Delta\tau$ of highly distributed CO₂ 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 CO₂ 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

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from 1-2km and 385ppm above 2km) or a linear interpolation between the described points.

p4856, l28: While it is true that the line edge position (one) is better at observing surface CO₂ due to the pressure broadening effect, the dependence of the signal at "one" on wavelength is much more severe than the one position. Can the authors discuss that the variation on signal due to laser wavelength stability is sufficient to allow "one" measurements to observe surface CO₂ 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.

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 received).

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.

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

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