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Atmospheric Measurement Techniques Discussions

Interactive comment on "CO₂-gradient measurements using a parallel multi-analyzer setup" *by* L. Siebicke et al.

Anonymous Referee #2

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Title: CO2-gradient measurements using a parallel multi-analyzer setup

Authors: L. Siebicke, G. Steinfeld, and T. Foken

Summary: ——————————————————————————-

This paper describes a statistical technique (based on the degree of correlation between CO2 from different measurement sites or "atmospheric mixing") to correct biases between 10 CO2 instruments during a field experiment. The subject matter is appropriate for AMTD and the method is well-described and presented by the authors. I think this is an interesting attempt to deal with the problem of instrument biases, though I also think more caution about certain aspects of the method should be stressed by the authors which I will discuss in the "general comments" below.

General Comments: ———————————————————————

* Though I think the method used to correct the differences in the CO_2 data is reasonable, I also think it should also be mentioned in the conclusions that a much more preferable solution is to know why the sensors differ by such a large amount and fix the measurement problem(s) that causes the differences. Instead, the authors take an opposite tack—they promote this method as being useful to "fix" a wide variety of measurements issues (eg., the last several sentences in the Conclusions). Though I fully agree the method is useful and can be used for other situations/measurements, I recommend some caution/qualification be used in making these statements.

* In my initial reading of this manuscript I mentioned my surprise at how large the differences (order of 25 umol/mol) were between the various IRGAs despite all the careful procedures taken to ensure accurate measurements (listed on pp 4389-4390). In the authors reply to my comment, they wrote,

"We absolutely share your surprise at this point. However, there are plausible explanations for significant bias even after calibration relative to measured standards. We explain the remaining bias as different conditions during standard measurements and during measurements of calibration standards (as noted in the text)."

If I understand the reply correctly, the pressure differences between calibration gases versus air sampled from the inlet is is the main source of the large biases. Or, is it more than just pressure differences? I assume the (cell) pressure is measured during both calibrations and sampling periods—what is the magnitude of the pressure differences to cause such large concentration differences? Can the pressure differences be accounted for and used to correct the biases in the concentration data? The authors mentioned that this explanation is "noted in the text", but I did not see it...I think the possible reasons for such large biases between IRGAs should be clearly explained with a few lines within the text. What could be done to the measurement system to improve or eliminate this problem in the future?

* One possible drawback with this method is that the authors are making an assumption based on what they expect the answer to be. This, I believe, is a somewhat risky way to study the natural world. Though the LES model helps confirm the validity of these assumptions; the LES model almost certainly does not mimic all the complexities of the natural world as accurately as is needed to truly evaluate this method. For example, how does the LES deal with a canopy? I see the answer on p.4393 (lines 22-24), where the authors state, "The simulation does not intend to perfectly mimic subcanopy conditions but...". However, isn't a critical aspect of the measurements that they are made in a subcanopy? So, if the LES model doesn't accurately take this into account then it seems the LES results are not as relevant/convincing as they could be. For a relatively uniform canopy this might be ok, but in a highly heterogeneous forest I suspect this could be an issue....

* I think some period of inter-comparison between the instruments would have been very worthwhile as an independent way to evaluate these errors (and also confirm that the correction method works well). If might not have been possible to inter-compare all the inlets, but perhaps a few of the inlets could have been placed near each other (eg, "co-located") to evaluate the instrument biases. This might be suggested for any future (similar) experiments....

Specific Comments: ————————————————————

* p.4386, line 8, "Contrary, horizontal..."

do you mean "In contrast, horizontal..."?

* p.4386, lines 14-16, I think using this correction methodology in a heterogeneous forest could be problematic for the reasons I describe above....the (potential) problem I see is that the changes in the overlying canopy structure can result in different amounts of vertical mixing of CO_2 at each location...

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* p.4386, line 17, I looked for Siebicke, et al 2011 and did not find it...if it hasn't been officially accepted then shouldn't reference this paper (same with Foken et al 2011).

* p.4386, lines 20-25, your data set could be used to evaluate how much information is lost in the "sequential" approach (by simulating a "sequential" CO_2 measurement and comparing the results to your measurements made with the "parallel" approach).

* p.4388, lines 7-8, you wrote that there are 25m high trees on the upper section...what is on the lower section? no trees? smaller trees? it would be good to roughly describe how heterogeneous the forest at the site is.

* p.4389, lines 5-7, not quite clear to me how the calibration gases were used to correct the "raw" CO_2 measurements. Were all ten IRGAs sampling the calibration gases every 4 hours at the same time?

* p.4392, lines 10-14, I think using the "tilde" to emphasize this is the median is a good idea. However, I still think the nomenclature used in Eq. 1 is not very precise. Assuming that c1(t), c2(t), are the physical LOCATIONS where a measurement is made, (ie, c1 is CO_2 at location "1", c2 is at location "2", etc) then your formula for the median doesn't make sense....for example, if n=9 it will say that the median is c((n+1)/2)=c5(t) where "c5" is the CO_2 at location "5". And I don't think this is correct...To use your formula (shown in Eq (1)), you need to have SORTED the observations from the smallest to the largest concentration....which then changes the meaning of "1", "2" to being the smallest, next largest, etc (not locations)...this has nothing to do with the method, but it is simply a matter of getting the nomenclature correct....

In my statistics book they don't give a formula for the median, instead they write out the meaning in words, which is,

- "single middle value in the ordered list if n is odd" - "the average of the two middle values in the ordered list if n is even"

* p.4395, line 20, a heat flux of 0.01 K m/s seems pretty weak...but it must be enough

to develop a convective boundary layer? (just curious)

* p.4396, lines 15-20, why was M5/M8 used and not M5/M9? How did the along slope correlations compare to the cross-slope ones? (does this matter?)...how sensitive is the method to using TF=60 min? [I now see the TF issue is addressed on p. 4405].

* p.4396, lines 21-22, it's written that the MIc range was "empirically inferred"....how was the MIc chosen within the range? does this mean the MIc varied with time? One important result would be that the determined biases are insensitive to changing MIc...is this true? What happens to the biases if MIc is larger than 0.12? [I now see this question addressed in Fig. 10 and the discussion on p. 4403]. Are the data shown in Fig.4a from a single 1-hr period, or from all periods over the experiment? (this is not clear to me). * p.4401, line 14, Figs 9 and 10 are mentioned before Figs 7 and 8 have been mentioned.

* p.4403, line 5, should be Fig. 9b (not Fig. 9a).

* p.4404, lines 17-19, "It is well known..." I think there should be a caveat added that the forest should be relatively homogeneous. I don't think this statement is valid for highly heterogeneous sites.

* p.4404, line 11, "loosing" should be "losing".

* Fig. 7, are these distributions created from the 1-hz data (so there are 3600 samples to create each distribution)?

* Some Figures have the "(a)" and "(b)" labels below the figure panel and some are above or in the upper-left corner. Typically, I think the (a) and (b) should go above the panel or inside somewhere...similarly, the captions should be consistent, the (a) or (b) descriptions are either before or after the (a) and (b). For example, in the Fig. 9 caption the (a) and (b) precede the description, but in Fig. (8 (and in other figures) it seems to be the other way around.

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Interactive Comment

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4383, 2010.

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