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Biogeosciences Discussions

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Interactive comment on "Conditional CO_2 flux analysis of a managed grassland with the aid of stable isotopes" *by* M. J. Zeeman et al.

Anonymous Referee #1

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General Comments

This paper describes application of a conditional sampling technique to a grassland during a brief measurement period. The technique, developed by Thomas et al. (2008) following on conditional sampling approaches by Scanlon and Alberton (2001) and many others, was designed to use information about particular transport events (sweeps and ejections going back to Shaw et al.) during the day to assess below-canopy processes (soil respiration) and within-canopy processes (net assimilation). The authors attempt to use stable isotopes of CO2 to "validate" the Thomas approach, although this is not a clearly-defined goal. The isotopes cannot be used alone to provide an estimate of daytime respiration (which is the goal of the Thomas approach), so how could they be used to validate it? There is likely to be a wealth of information to gain from combining high-frequency stable isotope measurements with conditional sampling using the Thomas approach, but in my opinion the present work is not yet ready to be published. There are only 4 days worth of data here, and the stated goals of the paper are not well addressed with this data. I think these authors stand to make a major contribution with this technique, but this contribution has not yet been achieved with the science they present here. More experiments and more thought will

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make this much stronger.

Specific Comments

There are some serious conceptual errors in Figure 1 and the resulting application of those concepts. First, Q1 and Q4 are consistent with Thomas or with general expectations, but Q2 does not represent downdrafts! Scalar-scalar plots contain no information about up versus down – these are typically presented separately for up and downdrafts as the authors have done in Figure 5. Q2 represents moist air (more humid than the mean) that is low in CO2 (compared to the mean). Your own Fig 5 shows that data plot in Q2 for both up and downdrafts. Daytime downdrafts over this canopy are likely to be dry (relative to the mean), not moist. Fig 1 conflicts directly with Thomas et al. (2008, their Figure 1) in this regard. Thomas interpret this quadrant (Q2) as events primarily originating within the vegetation canopy, and this is probably correct under appropriate conditions.

Second, the profiles of CO2 and δ in Figure 1 (bottom panel) should be symmetric. The top panel is correct, the bottom one looks fine to me for c(z) but delta should be a mirror image of c. δ_R is not defined in the figure or the text. If you mean the isotope ratio of respiration, then the plot is wrong as the measured air will never equal that – measured air reflects a mixing line between the CBL and the respiratory signature. In general the description of the Thomas method in this paper is not sufficient to understand the method. The intro needs more detail to achieve that. This paper needs to stand on its own. For example, the data points (squares) shown in Fig 2 are critical to the conditional sampling approach, but one can't understand that from reading this paper alone.

Goal a) is addressed to some extent with this paper.

Goal b) seems entirely unachievable with 4 days of data – this paper shows that cutting grass has an influence on measured quantities, but does not even begin to address how management influences daytime respiration.

Goal c) is definitely not well-addressed with this paper. To "validate" the Thomas approach, you need to be much more rigorous with considerably more data under more conditions. The isotopes will provide more information, but only under certain conditions. What are those conditions? When do they occur? etc.

The isotope data here are unique but they don't shed much light on the usefulness of the Thomas approach or on respiration. More detail is needed about the isotope measurements and why you think they can be trusted. The Tuzson paper cited did not present 10-Hz data (at least in the abstract), and Fig 3 does not provide any indication that the isotope instrument will work at 10 Hz. There are some data presented with isotope ratio as enriched as -4.5 to -5 permil (Fig 8). This will be associated with CO2 as

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low as 310-320 ppm, which is possible in a dense canopy but highly unusual in 2007 (maybe in 1997). This makes me seriously question the isotope measurements. More information about why you trust the isotope measurements is needed.

The rationale for the WUE analysis and related text is not clear.

Equation 2 ignores storage but you mention it later (eq 9 also).

Equations 3 and 4 are the correlation coefficients for the measured quantities w and c, or for w and q. They are not correlation coefficients for "net carbon flux" or "net water vapor flux".

More detail about the time lag through the 55 m tubing is needed. This time lag needs to be exactly right and unchanging (or correctly dealt with if it changes) for this analysis to work. Pumps change their pumping speed with temperature, for example. If you don't have an actively-controlled flow rate then the lag will change too. The large paragraph on pg 3493 is very confusing. For example, it refers to "updraft quadrant Q4" when it really means "the updraft panel on the plot, quadrant 4". Q4 can be associated with either updrafts or downdrafts of course. In general this whole paragraph is confusing. I picked through it very carefully and am generally familiar with these sorts of plots. The average reader will be terribly confused.

You make a good point on pg 3495 that, for the Thomas approach to work, you need to be somewhat near the canopy. There is of course a continuum between the roughness sublayer (RS) and the daytime CBL, the latter of which will be "fully mixed" or at least as "fully" as it gets. However, the presence of ramp structures in velocity and scalar time series is very common in the surface layer, even at appropriate measurement heights for EC. To make the claim on the one hand that the Thomas method does not work with the tall canopy because the air is "fully mixed", then show that once the canopy is cut (and hence you are then measuring well above the RS) and somehow the canopy is no longer fully mixed, does not make sense at all. For the eddy covariance technique to work, there must be variability in the measured quantities. Fully mixed would mean that CO2 or q were dead flat and not correlated with w (hence zero flux). There must be a vertical gradient for there to be a turbulent flux.

Page 3495 line 17: This short paragraph is all the discussion there is to address one of the major goals of the paper (the second research question). Not enough!

The last 4 figures are discussed in 1.5 pages. Not enough!

Pg 3496 line 1: The isotopic directions (more enriched, more depleted) are consistent with photosynthetic and respiratory signals, which is encouraging. Implied here but not directly stated is that those signals may differ (isotopic disequilibrium). The directional isotope changes you find here may result from 1) CO2 changes with no disequilibrium or 2) a disequilibrium and no net CO2 flux or 3) the more likely combination

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of 1 and 3. This could use some thought and maybe some discussion. Presenting the data relative to a mean delta is confusing, but this may be the best way to do it.

Figure 4: The y-axis label for the lower panel says d13C of CO2, but the caption says “d13C value of net ecosystem CO2 flux”. These are not the same thing! And your paper does not provide enough detail for me to understand which you are plotting. The Griffis et al. (2008) paper cited (their Figure 15) showed some very confusing estimates of the latter. Can your information shed any light on whether their results make sense?

Technical Corrections

with one exception (mixing ratios), this paper incorrectly refers to concentrations throughout the paper when mixing ratio or (better) mole fraction are correct page 3482 line 16: 13 should be a superscript

3482 20: this work has gone on much longer than one decade, even if you only consider the starting point as 1990 at Harvard Forest (there are papers from the early 80s by Verma’s group and earlier by Ed Lemon etc).

3483 23: diffusion and phase changes are not chemical reactions they are biophysical processes

3484 5: updrafts may carry information about the isotopic content of respiration, but that will be in the form of a mixing relationship – the d13C of updrafts will not equal d13C of respiration – this text is misleading

3488 20: time series is 2 words

3489 10 and 17: is it median or mean? (both are used)

3491 19: ref needed here

3491 23 and 3492 12: “basis” is correct, not “base”

Interactive comment on Biogeosciences Discuss., 6, 3481, 2009.