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Biogeosciences Discussions

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 #2

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General comments:
This paper is the first to investigate the applicability of a recently proposed conditional sampling approach (Thomas et al, AFM, 2008) for calculation of daytime subcanopy respiration fluxes in forests at a grassland site. The authors go beyond the original scope of the Thomas et al. paper and attempt to add the information of high-frequency measurements of stable carbon and water isotopes to the conditional sampling scheme to investigate the effects of management practices (grass cut) on the conditional flux sampling scheme and its associated quadrant analysis, as well as the gross carbon fluxes of respiration and photosynthesis. The analysis is based on 4 days of eddy covariance data collected at a single height and concurrent mean CO2 concentration observations in a vertical profile to estimate the storage term.
Although the application of the conditional sampling approach in short canopies such

as grasslands may have a large practical and theoretical appeal for the flux and micrometeorological communities, its success is questionable as some of the basic assumptions of the method are likely to be not or to a significantly lesser degree fulfilled by the flow over short vegetation. The authors do not address the flow properties in sufficient depth to be able to understand why the method failed in this experimental setup. The addition of stable isotopes bears a very large potential for this method in either forest canopies or – if applicable at all- over short vegetation and needs to be introduced more thoroughly. The addition of stable isotopes density observations is the strength and the conceptual novelty in this paper, which deserves adequate attention and sufficient depth. In particular, I believe isotopes cannot be used to 'validate' the method, but could add a very useful additional layer of information (a third dimension to the traditional 2-D quadrant analysis) that would provide additional constraints on when to conditionally sample events and the origin of the events. It is not clear to me why the authors introduced the concept of water use efficiency (WUE) into the discussion of the method, as it diverts attention from the main objectives of the paper and is not essential to the method. In fact, WUE is a ratio that can be derived from similarity arguments and is widely used because is provides a convenient way to model carbon and water fluxes, but should not be used to derive similarity theory. The language and length of the paper are appropriate, the presentation of the figures clear and precise.
In summary, I believe the paper provides useful information and deserves publication, but needs to undergo major revisions based on comments indicated below. The authors should clearly state that its an exploratory paper and provide detailed information as to why they believe this initial attempt failed, which will be very valuable to similar experimental studies in the future, and recommendations as to what needs to be improved. An expansion of the theoretical concept of adding stable isotope is also highly desirable.

Detailed comments:

1) The Thomas et al. method is based on the premise that eddies originating from different parts of the canopy are able to transport the corresponding signals of scalar sinks and sources (fingerprints) through the canopy to the observation height/ sensor while keeping structurally intact. Thomas et al. also explored the limitations of the approach and found that a very dense, multi-layered canopy and too intense turbulent mixing will smear these fingerprints, which ultimately leads to a loss of the signal of interest and a failure of the method. The current paper lacks detailed information or analysis of the transport paths that eddies carrying the information of carbon dioxide, water vapor and stable isotopes might take in/above grasslands. Such an analysis must include a discussion of the turbulent stochastic and organized motions as a function of proximity to the canopy, the latter of which is believed to be the primary transport mechanism connected to the occurrence of coherent structures or sweep/ejection cycles above rough surfaces. In some sense, the authors decided to take the second step before the first by applying the method without evaluation its premises.

2) The authors attempt to explain the lack of the signal of interest (Q1 in the $c\prime - q\prime$ plane) by a too intense turbulent mixing before the cut, and by a lack of mixing after the cut, and relate to this to the presence of the roughness sublayer (RSL). There is clearly a lot of confusion about the vertical extent and the definition/properties of the RSL (not only in this paper, but throughout the more applied flux literature). Many independent studies using a broad range of laboratory/ experimental setups and sensors showed some consensus that is vertical extent scales with the roughness of the surface/ height of the roughness elements, ie the height of the canopy (h_c) here, and typically doesn't exceed z/h_c=3 to 5, where z is the sampling height. The data presented in the manuscript were taken at z/h_c = 10 and 35 before and after the cut, respectively, ie, well above the RSL in either case. The authors have to demonstrate that the bigger eddies are not convective eddies impinging on to the surface from above, but eddies originating from the roughness of the canopy to be able to connect the sampled signals with the physiological activity of the grass canopy.

3) Sampling in the RSL does not exclude EC observations a priori, but it becomes a sampling problem above short canopies as the size of the eddies scales with the distance from the displacement height, and smaller eddies cannot be resolved because of the increasing influence of path length averaging/ high-frequency loss in closed-path gas analyzers. EC can be used to estimate the flux in a certain point in space that may or may not be within the RSL, the question is then how representative the flux is given a certain degree of horizontal surface heterogeneity (see eg Mahrt, BLM,2000, Vol. 96, Pg 33-62 for some discussion). The RSL is not a layer of insufficient mixing per se, but might be heterogeneous due to influence of individual roughness elements, which I doubt would occur in case of a short grass canopy.

4) The authors merely evaluate the conditional sampling scheme of the Thomas et al method, without presenting any flux estimates, which is the ultimate goal of the method. This exploratory nature of the analysis should be stated clearly, and reasons for its success or failure discussed.

5) As mentioned in the general comments, the benefit of adding stable isotope data has to be discussed more thoroughly including advantages, shortcomings, and limitations. This is potentially a very powerful tool for diagnosing metabolic and air transportation pathways, so it needs to be appropriately introduced. Of particular interest is the question how meaningful a perturbation from a 'mean isotopic δ^{13}C' value is, as per definition it presents a ratio of ratios. Hence, the δ^{13}C may not change, but numerator and denominator may change which leads to limitations of what signals can be used and detected. It was not clear to me how the indicator function in Eq. (13) was used in combination with those listed in Table 1, and where the $\mu_{1/2}$ comes from.

6) How did you compute the footprint? What were the reasons to discard data from most wind directions and keep data only from a 80○ wide sector? Under weak wind situations independent of stability, meandering may lead to abrupt changes in wind direction bringing in signals from flagged wind directions.

7) Page 3493, Lines 3ff: Any turbulent flow is intermittent and instationary to some degree depending on the time scale of the underlying process in relation to the

reference window used for analysis. Hence, it is not surprising that the arbitrarily selected averaging and perturbation time scale of 30 min is comprised of shorter ‘events with the same slope but different offsets’ as the authors describe it. This may be remedied by selecting a perturbation time scale more appropriate for the surface and flow conditions.

8) It is not clear to me, when the authors compute the net CO_2 exchange as the sum of turbulent flux and change in storage term, and when they exclusively use the turbulent flux data. Accounting for the change in storage term is important only when presenting the ensemble average of the diel NEE dynamics (as done in Fig. 4), but periods when the change in storage term is different from zero imply non-stationary conditions on time scales of the averaging interval and thus pose questions marks on the conditional flux analysis as it requires stationary conditions. It is further not clear to me if the authors evaluated only daytime, or day- and nighttime observations. This has a significant impact on the conditions selected for identification of the events of interest.

9) How do you define ‘subcanopy’ in a grass canopy? Is there sufficient separation between the main respiration source (ie the soil) and the assimilating grass to allow for different fingerprints? Have you observed water vapor and CO_2 profile in a grass canopy? I can imagine that such observations are very challenging from an instrumentation perspective. Your Fig. 1 and the corresponding paragraph in the body of the manuscript describe a decrease of specific humidity close to the surface. I would argue that this depends on the amount of surface soil moisture and plant density, which determine how much light penetrates to the surface ground providing the energy to evaporate the water. I suggest to omit the vertical profile of relative humidity as it is poorly constrained and is not meaningful in this context.

10) Did you apply any spectral correction to the air sampled through the 55m long tubing? How did the spectra/cospectra of the in-situ open-path Li-7500 and the QCLAS compare?

Technical comments:

a) Eqs. 1, 6: the negative sign of the RHS term is incorrect, it is rather that WUE is defined positively so that the magnitude of the RHS term is of interest.

b) Pg 3487, line 22: rather than introducing each variable separately, the authors should generally define their notations of $x\prime$ and (x) etc.

c) Pg 3488, Ln 6: omit ‘reciprocal’.

d) Please be more precise in your wording when referring to up- and downdrafts in combination with specific quadrants. Although similarity theory generally predicts up- and downdrafts to be located in certain quadrants of the $c\prime - q\prime$ plane, turbulence is a stochastic process with a large degree of inward interaction leading to the spread around the similarity theory prediction.

e) Page 3490, Line 21: How meaningful are distances accurate to within 1 cm above vegetated surfaces?

f) Fig.3 is not referenced in the text.

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