

Interactive comment on “Conditional CO₂ flux analysis of a managed grassland with the aid of stable isotopes” by M. J. Zeeman et al.

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On behalf of the authors: Please refer to our responses (in bold text) to the comments made by the anonymous reviewers below.

Anonymous Referee 1

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

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(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 make this much stronger.

Answer: We have made numerous revisions to our manuscript in order to improve its contributing value and hopefully answer the reviewers critiques. We would like to repeat what we stated previously (see Biogeosciences Discuss., 6, C544–C545, 2009), that we believe that with four days of data it is certainly possible to draw first conclusions.

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.

Answer: We have corrected the mix-up between Q2 and Q4 in Figure 1 and the text.

Second, the profiles of CO_2 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.

Answer: The profiles between CO_2 and δ should not be a symmetric mirror image as the relation between δ and CO_2 is not linear. The relation between δ and $(\text{CO}_2)^{-1}$ is 'linear', but is not given here. We have added a definition for δ_R as the isotope ratio of respiration and modified the graph to be more realistic.

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.

Answer: We added wording to be more precise in stating our goal and added text to accentuate the preliminary nature of the study.

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.

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Answer: As the methodology aims at separating fluxes from different sources under daytime (assimilation) condition, we have added more information on the stable isotope signatures of various sources under these conditions.

The isotope data here are unique but they don't shed much light on the usefulness of the Thomas ap- proach 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.

Answer: These measurements were computed from 5 Hz data. The Tuzson paper indeed only mentions results for integration periods, without giving the output frequency of the instrument. The QCLAS is able to retrieve isotope ratio data with a theoretical speed of about 20 Hz. However, in practice we can only achieve 10 Hz resolution for a smooth hardware operation. Moreover, the air sampling setup including mass flow controllers, tubing and temperature stabilisation unit as well as the finite pumping speed, hence the absorption cell response time, will reduce the real instrumental response to 5Hz. The spectrometer is capable to operate at high flow rates (up to 450 l/min), but its precision decreases with the square root of the operating frequency, i.e. by a factor 2.24 at 5 Hz relative to the one second time resolution as given in the Tuzson et al. paper. In the cited paper the instrument was investigated regarding precision and accuracy and its input was validated by comparing it with standard IRMS.

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 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.

Answer: While Buchmann et al. 1997 show similar results for a C3 crop rota-

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tion, a more recent a study for a C4 crop rotation by Zhang et al. 2006 shows a range of -4 to -12 permil. We argue that these values can be expected on a high-production grassland with high LAI within a larger agricultural area at the end of a sunny day in summer.

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

Answer: We have removed most of the wording that implies WUE.

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

Answer: We have added wording in the Methods section that shows that the use of the storage term is limited to the presentation of diurnal patterns of NEE (such as Fig.4). The storage term is indeed ignored in the conditional flux analysis.

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”.

Answer: We have made the suggested changes.

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.

Answer: We have calculated the offset for each averaging period. This is mentioned in the methods section. A cross-correlation function was used for the calculations. In the text we state: “The time lags between the closed-path QCLAS,

the open-path IRGA and the sonic anemometer is calculated for each half hour (9.3s for the QCLAS, 0.15s for the IRGA) and the time series is shifted accordingly before computing covariances.”

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.

Answer: We state the opposite in the manuscript: the Thomas et al. Approach works well for tall vegetation, because measurements are made in the RSL. Short statured vegetation is the exception that asks for lower measurement height than typical.

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!

Answer: The discussion of the second goal of the paper continues in discussion of the third research goal (verification) and therefore receives more attention than this paragraph alone.

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

Answer: Figure 7 is added to support figure 6. Figure 8 and 9 show only a small

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aspect of the data.

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 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.

Answer: We agree that the general consideration between dependency of isotopic directions as expressed by the reviewer deserves more attention in future studies. The presentation of 'relative' delta values is not the only way to present such data, but appeared very helpful to make the patterns visible per averaging period, and allow comparison of these patterns from one averaging period to the next (e.g. fig 8 an 9). Based on our available dataset we agree with the reviewer that this is probably the best way to present the data.

Figure 4: The y-axis label for the lower panel says $\delta^{13}\text{C}$ of CO₂, but the caption says " $\delta^{13}\text{C}$ 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?

Answer: We believe that what is shown in fig 4c can be described as "the $\delta^{13}\text{C}$ value of a CO₂ flux", as by our definition $\delta^{13}\text{C}$ is calculated from mixing ratios (eq 12). We have not defined $\delta^{13}\text{C}$ as a ratio of fluxes (as is used in the Griffis et al. 2008 work). We have revised the caption to more clearly state it concerns a graph of $\delta^{13}\text{C}$ of CO₂ in Figure 4c.

Technical Corrections with one exception (mixing ratios), this paper incorrectly refers to concentrations throughout the paper when mixing ratio or (better) mole fraction are

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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).

Answer: We have made the suggested change and replaced 'concentration' with 'mixing ratio' where applicable.

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

Answer: We have made the suggested changes.

3484 5: updrafts may carry information about the isotopic content of respiration, but that will be in the form of a mixing relationship – the $\delta^{13}\text{C}$ of updrafts will not equal $\delta^{13}\text{C}$ of respiration – this text is misleading

Answer: The have added 'the influence' to define it does not equals respiration $\delta^{13}\text{C}$ due to mixing.

3488 20: time series is 2 words

Answer: We have made the suggested changes.

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

Answer: The median values are calculated for each selection of data per time interval and in the graphs presented as deviation from the mean for that time interval. We have added text to explain the meaning of the median function (Eq. 13).

3491 19: ref needed here

Answer: We have added the reference.

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

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Answer: We have made the suggested changes.

Anonymous Referee 2

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

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method. In fact, WUE is a ratio that can be derived from similarity arguments and is widely used because it 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 it is 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.

Answer: See point 4.

2) The authors attempt to explain the lack of the signal of interest (Q1 in the $c - q$ plane) by a too intense turbulent mixing before the cut, and by a lack of mixing after

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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.

Answer: We completely agree. We thought that we have the writing clear enough to make sure that the reader understands that a typical measurement height over grassland is well above the RSL. We have added “is always” to emphasize exactly this point.

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.

Answer: We agree and also do not see where such grasslands might show significant heterogeneity in roughness. (Note: no change to text).

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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.

Answer: We have omitted showing respiration flux estimates while the method appeared only to be partly applicable to our dataset. This relates to the 'exploratory' nature of any analysis of our dataset, which is only a few days in length. We have added wording to emphasize this.

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 $\delta^{13}\text{C}$ ' value is, as per definition it presents a ratio of ratios. Hence, the $\delta^{13}\text{C}$ 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.

Answer: We have added wording to emphasize the shortcomings: "The shortcoming and limitation in the method with present day instrumentation remains in the difficulties to obtain the necessary precision to resolve small differences in the isotopic signatures in turbulent fluxes at high time resolutions."

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.

Answer: As indicated in the methods section, "a part of the field within the EC footprint was cut". We have added reference to the method of calculation of the

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footprint (following Kljun et al., 2004). The figure (Fig R1) added to this response shows the cut area (purple border) and the selected wind sector relative to the flux tower (red star). This shows the margins taken in selecting this wind sector.

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.

Answer: We completely agree with the view of the reviewer.

8) It is not clear to me, when the authors compute the net CO₂ 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.

Answer: The storage term has only been used in combination with the representation of diurnal NEE values (fig 4). We have added wording in the Methods section to more clearly limit the extend of its use in the paper.

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 CO₂ profile in a grass canopy? I can imagine that such observations are very challenging from an instrumen-

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tation 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.

Answer: We have added “For grassland sub-canopy vegetation must be defined as ‘lower canopy’ and soil respiration.” in the Theory section. We did not change the text or the figure with regards to the interpretation of RH, as we believe this is an important difference for grassland and consequent difference with the Thomas et al. described concept.

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?

Answer: We did not apply a spectral correction for the QCLAS derived measurements. An example of the spectra before and after cut can be seen in the attached figure (Fig. R2).

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.

Answer: We completely agree with the reviewer. To let WUE be a positive number, the '-' sign is added in the definition. In addition, we have removed the text mentioning WUE to prevent further confusion.

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

Answer: We have made changes to the text to improve the definitions of vari-

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ables.

c) Pg 3488, Ln 6: omit 'reciprocal'.

Answer: We have made the suggested changes.

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 $c - q$ 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?

Answer: We have changed value for the height of the EC instrumentation to 2.5m to better represent its accuracy in comparison to the surroundings of the instrumentation.

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

Answer: The figure has been removed. The values for the regression were already available in the text (section Methods).

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

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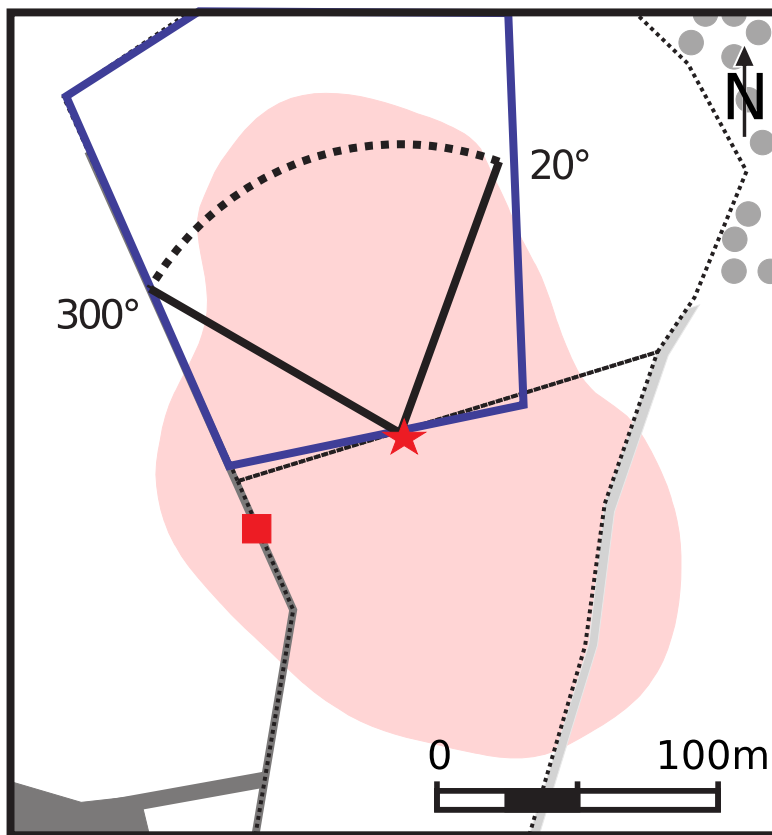


Fig. 1. (Fig. R1): Map showing the EC tower (*), the QCLAS container (square) the cut area (purple outline), the footprint (90%, pink area) and the wind sector considered for analysis after the grass cut.

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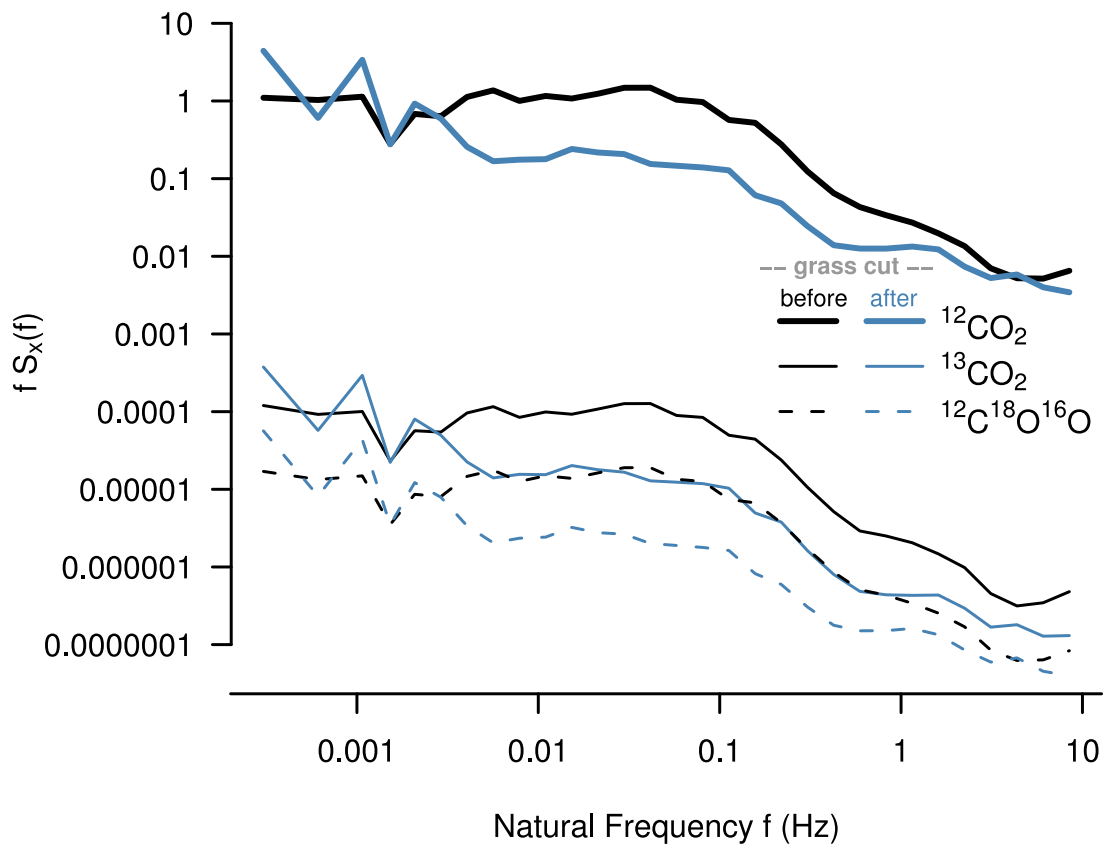


Fig. 2. (Fig. R2): Spectra for the QCLAS instrument for one hour periods of data (12–13h) before and after cut.

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