

Interactive comment on “Carbon dioxide exchange of a perennial bioenergy crop cultivation on a mineral soil” by S. E. Lind et al.

Anonymous Referee #2

Received and published: 26 November 2015

General comments:

The manuscript focusses on the potential climate mitigation of reed canary grass (RCG), and is novel in the fact that it deals with a RCG cultivated in a mineral soil, while most of the existing studies reported in the scientific literature concern RCG in organic soils, e.g. for restoration of drained organic soil. The CO₂ balance of the RCG is computed combining eddy covariance (EC) methodology and LAI analyses, and then compared with a reference study of a RCG on organic soil. The manuscript is well written and interesting. However, minor revisions are required in my opinion in order to be acceptable for publication on BG, especially in the discussion section that needs to be extended.

EC methodology is a well consolidated technique to calculate fluxes of trace gases with
C7971

the atmosphere, and so to extrapolate budgets of these gases in the studied ecosystems. However, this technique alone cannot provide a fully comprehensive budget, as non-turbulent fluxes escape this computation, like off-site emissions involved in the management and the C exported in biomass. Furthermore the study only focusses on CO₂ fluxes: it is well known that other fluxes than CO₂ have a high importance in the evaluation of the warming mitigation potential of cultivation. That said, the interest of the manuscript is in the fact that this type of cultivation is not well studied in mineral soils, and that a CO₂ balance can provide a clear message on the biological CO₂ exchanges of RCG. This is why I found crucial the comparison with a reference study on organic soil, which is a more explored field. Comparing the same factors in the evaluation of the cultivation increases the robustness of the message the authors wish to give. This aspect seems to be treated more accurately in the discussion section, but not having the right importance in the Introduction. The authors declare they aim to characterise the NEE of the site, which would not be enough. I suggest the authors to clearly state and underline in the manuscript that their objectives include the comparison of the study site with a reference study, especially in the introduction and the abstract. All the main passages of the manuscript should deal with this comparison, in particular analogies and differences between the sites should be described not only for what concerns the results, but also about general site characteristics (climate, management, use. . .) The comparison with other bioenergy crops, and to cropland in general (especially the crop types that used to be cultivated before the installation of RCG) should also be strengthened in the discussion and referred to also in the conclusion section, as the reference site was evaluated not as a bioenergy crop per se, but as a restoration of drained organic soil, with an expected high respiration rate. The studied site of the manuscript was instead installed in cropland, and the simple fact that the CO₂ balance is negative in the three years is not enough to evaluate whether or not the RCG plantation is “environmental friendly”, as stated in the conclusions.

From a technical viewpoint, the structure of the manuscript sometimes suffers of some lacks, especially in the discussion section: while some aspects are very well detailed,

C7972

some others seem to have been excluded, while they might have an importance in explaining the observed results. The differences between the study site and the reference site are not always discussed in the proper manner, as it is assumed that they are due only to the different soil type, while it is necessary to add some considerations on other possible reasons. Also, some operations that are correctly reported in the material and method section, and that might have an influence on the studied aspects, are not considered at all in the discussion section (e.g. the fact that the aboveground biomass is left in the field during the first year, or the use of herbicide). I suggest to add some considerations in the discussion section in that.

Another weakness of the study concerns the fact that conclusions are sometimes too generalised: the study site cannot be considered representative of all the RCG in mineral sites. Also, differences between the study site and other studies on RCG are sometimes too easily attributed to the difference on the soil type (mineral/organic), while other site characteristics (climate, type of management, etc.) should be taken into account. I suggest deepening the parts of the discussion where differences with other studies are illustrated, including clear statements on other possible reasons that might explain the found differences.

As a last general comment I underline the fact that EC methodology is for its complexity subject to several sources of uncertainty. I understand that for the same reason is hard to quantify this uncertainty, and there is not a standard procedure. However, as the manuscript is mainly based on EC, uncertainty quantification is recommended based on existing papers (e.g. Hollinger and Richardson 2005, Papale et al., 2006).

In my opinion, after having implemented the suggested changes and discussion parts, the manuscript will be more robust and adapt for publication in BG.

Specific comments:

Abstract: the abstract is synthetic and concise; however I suggest adding a sentence on the comparison with the reference study, instead of only reporting the aim of char-

C7973

acterising NEE.

Introduction: In this section it should be clearly indicated the aim of basing the evaluation of the performance of the RCG cultivation on mineral soil on the comparison with studies performed on organic soil.

Material and methods: this section shortly describes the site and provides some details on the micrometeorological and companion measurements, and also in the formulas used for the data analysis. However, as the CO₂ balance is mainly based on the EC technique, a deeper description of the steps used to get calculated fluxes is needed: how did you select the ustar threshold? Which model(s) did you use for footprint calculation? Also other methods should be more carefully described, e.g. soil analyses. Results: this section is complete and detailed. Results of micrometeorological measurements, climatic pattern, trends and drivers are carefully illustrated, and the CO₂ annual budget is reported at last.

Discussion: This section is well structured. However, some discussions need to be added to reach a higher degree of completeness and robustness of the manuscript. In particular, it would be cited the fact that alternative options exist for peatland restoration, with a brief discussion on expected differences with RCG. Also, authors should keep in mind that a better performance of the studied RCG as compared to the reference study from the CO₂ balance view point is not enough to give a positive evaluation of it: this is related to the fact that 1. other fluxes exist that are relevant for climate mitigation (not only CO₂ and not only biological fluxes); and 2. to the fact that the reference site substituted a drained organic soil with likely strong positive NEE, while the RCG of this study was installed in a crop area. The discussion on the first point should be extended, and added for the second point, including comparison with CO₂ balances of crop systems similar to the ones present at the site before the seeding of the RCG (as found in the scientific literature). Moreover, when discussing the differences between study site and reference site, other reasons than soil type should be discussed: for example, different climatic patterns, or the fact that the biomass was left in the field

C7974

in the first year of cultivation of the study site, especially when discussing respiration patterns. Please add some comments on that to increase the robustness of this section. Also some discussions are missing related to some statements of material and method: for example, the energy closure balance problem is analysed in details, but no mention is made on the angle of attack issue, which has been reported as one of the possible causes for the imbalance (Nakai et al., 2006). Or the fact that measurements started 3 years after the seeding. At last, some considerations should be added also concerning the results of the first year, not only related to the emissions due to soil preparation, but also making some speculations on the fact that different management operations applied (i.e. use of herbicide after seeding). This might have implications in the patterns of fluxes and in the fact that the study site was a net source of CO₂ in the first year.

Technical comments:

L9, P16674: if measurements covered a period of three years, why you report only 2010 and 2011? Please clarify.

L15-16, P16674: please try to evaluate the uncertainty related to EC measurements, as it provides info on the reliability of the numbers you use to evaluate the CO₂ balance of the cultivation.

L24, P16674: please specify different sources of respiration (plant, soil, microorganism...)

L15, P16675: Please use SI units: Mg instead of tons. Check for consistency: in the abstract you used kg DW ha⁻¹ for biomass. In addition: is this range global?

L16-20, P16675: please specify this is a general rule concerning respiration. Another factor that might impact the NEE is the GPP rate (and not only length), while the C balance can be influenced by the biomass use. Please consider rephrasing: here you are considering benefits from a larger perspective (not only GHG), but including only

C7975

some factors (respiration and not GPP rate)

L23, P16675: do you have reference for no studies on that? Or is it your knowledge? Please specify

L25-27, P16675: As I already said, more relevance in the Intro should be given to the fact that you want to compare it to a reference study on organic soil.

L26, P16675: Typo: quantify.

L9-26, P16676: please provide further information on how soil analysis was performed. How many samples? Which methods? When? This will make more clear some sentences, e.g. if the found variability (reported ranges) was due to spatial or temporal variability

L6-8, P16677: does it mean it was not harvested after the first year? Please specify as it might be relevant in the analysis of patterns

L14-15, P16677: please provide justification to this sentence, e.g.: "because no other obstacles were present and the sonic anemometer in use had an omnidirectional geometry". Please consider moving this sentence at the end of the paragraph (i.e. L20, after "vegetation height")

L21, P16677: please explain acronyms: inner diameter, Polytetrafluoroethylene. And specify that reported values are lengths.

L6-8, P16678: does it mean the de-spiking procedure was applied only to CO₂ and H₂O concentrations? Please specify

L8-9, P 16678: the previous or next one? Please clarify

L11, P 16678: can you justify this sentence on angle of attack? This might have consequences in the energy balance closure problem

L17, P 16678: reference needed

C7976

L21, P 16678: the selection of a ustar thresholds should be carefully applied. Please provide details on how you chose the indicated threshold.

L22-23, P16678: what do you mean here with "stationarity"? Foken and Wichura, 1996 use the difference between the dispersion of an averaging period and those of sub-periods, and suggest non-stationarity is found when the difference is above 30%. If you use a different threshold, please specify. Please consider a different name for this indicator, as to avoid to state that if the "stationarity" is higher than a threshold, then the flux is non-stationary.

L27, P16678: which model or models did you use for footprint calculation? Please specify

L5, P16679: please consider rephrasing in "excluding gap filled data"

L16-20, P16679: Please reformulate this part. EBC as expressed here is a simplified formula valid for ideal surfaces (i.e. with no mass and heat capacity). A more precise formula would include energy storage of the layer considered (as you indicated below). I suggest adding references for eq. (2) (e.g. Arya 1988), and then clarify that the addition of the stored energy is expected to give a more precise estimation of energy balance. However incomplete closure is common also for other reasons: large scale eddies (which is Foken 2008 hypothesis) and angle of attack issue (see Nakai et al., 2006). Please consider rephrasing and discuss this issue in the discussion section, including considerations on angle of attack problem (which you did not correct)

L19, P16679: please insert a colon before formula

L23, P16679: missing term or 'a' not needed before common? Please check

L18, P16680: are you referring to incoming radiation here? Please clarify which is the variable affected by this issue.

L19-21, P16680: I suggest to check PAR data with short wave incoming data (if this is the variable you are talking about): such a big underestimation should be evident from

C7977

that comparison. It is crucial to be certain the instrument is underestimating before correcting, as this potentially affects ECB considerations. In the case that shortwave incoming radiation is actually biased, can you state that other related variables (e.g. shortwave outgoing) are not involved? Please specify. Please also indicate how you corrected data: by adding 35% to all data or taking FMI data for the short wave incoming radiation?

L1, P16681: please insert a colon before formula

L6-7, P16681: what are you referring to with "belowground"? Please clarify

L10, P16681: is there a reason for excluding 2011 from root sampling strategy?

L11, P16681: was this time period enough for a complete drying? If you test it, please clearly state. Otherwise can you provide references that such a short period at 65°C was found to be enough to dry this type of matter?

L2, P16682: please add reference for equation 4

L17, P16682: please add reference for equation 5

L22, P16682: TER was obtained by subtracting estimated GPP to NEE, so I would clearly expect a relationship between TER and GPP. Please consider rephrasing, e.g. "to test if the answers of TER and GPP to climatic patterns was the same, . . ."

L3-4, P16684: following 2009? Please clarify this sentence, also concerning what "9" is referring to

L16, P16684: if you gap-filled data, why does Fig. 3 contain gaps? Please clarify

L8-9, P16685: please consider rephrasing: "June presented conditions of high CO2 uptake during the day and of CO2 loss from the RCG cultivation system in night-time"

L24-26, P16685: please add in the discussion some consideration on the fact that you are comparing two variables that are related between them from the beginning, as they

C7978

are estimated from the same main variable (NEE)

L11, P16686: dot missing

L5, P16688: shown

L7-19, P16688: what about the biomass that was burnt? This is CO₂ that returns fast to the atmosphere. This is good to exclude from the comparison if in the reference study this is also not included; however, this sentence is not correct, please consider rephrasing

L16-19, P16689: consider rephrasing, it is redundant to repeat citations. I suggest to put a dot after "bioenergy crops", deleting anything else up to the next dot and then moving the next sentence ("compared...range") after citation of Grelle et al., 2007. Also, are these values averages on a long term or relative to one year? Please clarify.

L25-26, P16689: A bit too strong. Consider rephrasing in "the RCG of the present study showed a higher capacity..." This happens often in the manuscript to generalise the results from the RCG of this study, and I suggest to avoid it.

L4-6, P16690: please move this sentence to material and method section

L12-15, P16690: do these studies refer to the same sites? Please clarify

L15-18, P16690: please split this sentence

L11-13, P16691: please report reference values

L24, P16691: please report them

L13-15, P16692: are the ref site and the site of this study at the same latitude? Please add discussion on that (different latitudes would mean different PAR levels)

L16-18, P16692: is it a difference with the ref site? Please add some thoughts on that

L4-7, P16693: please discuss also climatic differences (respiration is driven by soil temperature as you say below: are soil temperature levels of the ref site the same?)

C7979

L9-10, P16693: please add "in 2010 and 2011, respectively" in the brackets. Also please check units are always reported in the manuscript

L23, P16693: please change "same crop" in "same crop type"

L28-29, P16693: For that reason I think you must focus on the comparison with the organic soil type, and add conclusions on this sense

Table 1: In caption please add reference to Fig. 6

Table 2: What is the reason to report data in two units? Please consider modifying this table: as the 2009 is not a full year, its relevance is due to the fact that it follows seeding activity. Please consider excluding it from Table 2 as it cannot be compared to full years (2010 and 2011), but use it to show the relevant release of CO₂ to the atmosphere following seeding activities. Otherwise you might consider of splitting data in Tab. 2 in periods (e.g. Oct to Apr and May to Sep, approximately corresponding to dormant and growing seasons), which would allow to leave also 2009 data.

Fig. 5: what are the open grey circles for? Please clarify

Fig. 7, (b): may this poor relationship be due to the fact that after the first year cultivation, the biomass was left on the field? Please consider touching this aspect in the discussion

References

Foken, T., & Wichura, B. (1996) Tools for quality assessment of surface-based flux measurements. *Agric Forest Meteorol.* 78:83-105.

Arya, S. P. S: 1988, Introduction to Micrometeorology.

Hollinger, D.Y., & Richardson, A.D. (2005). Uncertainty in eddy covariance measurements and its application to physiological models. *Tree physiology*, 25(7), 873-885.

Papale, D., Reichstein, M., Aubinet, M., Canfora, E., Bernhofer, C., Kutsch, W., ... &

C7980

Yakir, D. (2006). Towards a standardized processing of Net Ecosystem Exchange measured with eddy covariance technique: algorithms and uncertainty estimation. *Biogeosciences*, 3(4), 571-583.

Nakai, T., Van Der Molen, M. K., Gash, J. H. C., & Kodama, Y. (2006). Correction of sonic anemometer angle of attack errors. *Agricultural and Forest Meteorology*, 136(1), 19-30.

Foken, T.: The energy balance closure problem: an overview, *Ecol. Appl.*, 18, 1351–1367, 2008.

Interactive comment on *Biogeosciences Discuss.*, 12, 16673, 2015.