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 restauration 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.

We thank Anonymous Referee #2 for helpful comments and suggestions to improve this manuscript. We hope that the revised manuscript is satisfactorily modified. Below you will find the comments from the Referee #2 followed by our responses which are marked in blue.

EC methodology is a well consolidated technique to calculate fluxes of trace gases with 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 characterize 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…)

We are currently describing the comparison between the mineral soil site and the organic soil site in the abstract and introduction. Also, a separate section covering the general background of the organic soil site was added to the materials and methods.

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 restauration 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.

We agree with the referee on the limited data comparison. However, to our knowledge, there are no published eddy covariance data on CO_2 exchange of reed canary grass cultivation on mineral soil site. Also, to our knowledge, there are no annual CO_2 exchange data measured using eddy covariance on other crops on mineral soil in Finland nor in other Nordic countries. This limits our options with the data comparison in the discussion.

We have mentioned in the conclusion, that from the CO2 exchange point of view, the RCG cultivation is environmentally friendly and that only through a full LCA (including other GHG emissions and management costs and biomass burning) we are able conclude more on the performance of this crop. Also, we do not try estimate what would be the GHG balance of the site if it was cultivated with another crop and if some of the emissions would be possible to avoid with RCG cultivation.

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, 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 generalized: 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.

The discussion section was revised accordingly.

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.

We agree with the referee that reporting uncertainties with the results is always a good practice. However, it is also important to make a clear distinction between random and systematic uncertainties, since the relative significance of random uncertainties diminishes with integrating, i.e. their effect on the uncertainty of annual balances is most likely negligible, whereas systematic uncertainties are not affected by averaging or integrating processes.

The random uncertainties of EC fluxes stem mainly from one-point sampling of the flux, in other words from the fact that a finite sample of a stochastic process (turbulence) is used to calculate the flux (e.g. Lenschow et al., 1994). The random errors of 30-min averaged EC fluxes are commonly within few tens of percentages of the flux (e.g. Mauder et al., 2013).

The random error in the present study was determined and added "*The random errors of 30-min averaged and quality controlled CO_2 fluxes were determined following Vickers and Mahrt, 1997). The random error was 14%, 16% and 14% during July-September 2009, May-September 2010 and May-September 2011, respectively.*"

The systematic errors are primarily caused by 1) the limitations of the EC measurements (e.g. inadequate high frequency response of instruments) or 2) unmet assumptions and methodological challenges (Richardson et al., 2012). The first source of systematic uncertainty was already minimized by carefully processing the EC data (see Sect. 2.2 in the manuscript). However, the second source of systematic uncertainty is more difficult to assess, since that requires estimation of e.g. advective fluxes. This is a challenging task (e.g. Feigenwinter et al., 2008) and out of the scope of this paper. Nevertheless, the energy balance closure (EBC) can be regarded as an estimate of the flux systematic errors (Mauder et al., 2013) and an analysis of the EBC is already included in the manuscript. The section on EBC was revised and modified accordingly.

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 characterising NEE.

Changed to "*Carbon balance and its regulatory factors were compared to the published results of a comparison site on drained organic soil cultivated with RCG in the same climate. On this mineral soil site, the RCG had higher capacity to take up CO_2 from the atmosphere than on the comparison site*."

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.

Added *"Additionally, we aim to compare our findings from the mineral soil site to the published data on of a RCG cultivation system on a drained organic soil (referred to hereafter as comparison site) in the same climate region."*

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.

Added

1. paragraph "*2.5 Comparison site characteristics*"
2. for u*star "*We plotted the night-time NEE with u* and found no correlation between the two. Nevertheless, a default u* filter of 0.1 m s^{-1} was used.*"
3. for footprint model "*Footprints were calculated for each 30-min averaging period with the analytical footprint model developed by Kormann and Meixner (2001). The model is valid within the surface layer and it utilizes power law profiles for solving the footprint sizes analytically in a wide range of atmospheric stabilities. Based on the analysis, 80% of the flux was found to originate from within 130 m radius from the mast*."
4. methods for 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 restauration, with a brief discussion on expected differences with RCG.

It is true, that there are many after-use options for cutaway peatlands. However, the primary study site the present paper focuses on a mineral soil and thus a discussion on peatland restoration is outside the scope of this manuscript.

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).

In the present paper, we aim to report the annual NEE of RGC cultivation on mineral soil and to determine the controlling factors of the NEE. Also we aim to compare the findings on mineral soil to that of RCG on organic soil, from a comparative analysis point of view. We do not aim to determine whether CO_2 emissions were substituted while RCG was cultivated on mineral or on organic soil. Currently we are not even able to do that as, to our knowledge, there are no annual CO_2 measurements done with eddy covariance method on crops on mineral soil in Finland or in similar ecosystems. Our original use of the term "reference site" for the organic soil site is wrong in the present paper as it has different meaning on the LCA studies. We have replaced the "reference site" with "*comparison site*" in the manuscript in order to clarify the purpose of the comparison in our work.

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 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.

The discussion section was revised accordingly.

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.

We agree, it is good to shortly discuss the possibility that better energy balance closure (EBC) could be achieved if the angle-of-attack correction would have been implemented. We opted not to do the correction, since in our opinion, it still lacks a thorough validation in the field. We are aware of the progress made in this regard (Nakai and Shimoyama, 2012), but we still feel that a solid long term validation of the angle-of-attack correction method is needed.

We also agree that there is a difference in the age of the crop stands between the present study site and the comparison site. However, the age of the stand in the comparison site is still far from the end of the life cycle of the crop that lasts 10 to 15 years. Estimation of the other energy inputs, management effects etc. is part of an LCA, which is not in the scope of the present paper. Also, due to the fact that we started the CO_2 measurements after the soil preparation work, we cannot discuss the effect of those on the CO_2 exchange. Also based on our data, it is not to possible to discuss the effect of the herbicides on the CO_2 exchange as the measurements were started only few days before the herbicides were applied and stopped few days after for approximately three weeks.

Technical comments:

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

Changed to "*To quantify the CO_2 exchange of this RCG cultivation system, and to understand the key factors controlling its CO_2 exchange, the net ecosystem CO_2 exchange (NEE) was measured from June 2009 until the end of 2011 using the eddy covariance (EC) method*."

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.

Random and systematic errors were estimated and added "*The random errors of 30-min averaged and quality controlled CO_2 fluxes were determined following Vickers and Mahrt (1997). The random error was 14%, 16% and 14% during July-September 2009, May-September 2010 and May-September 2011, respectively.*" and "In this paper, the EBC is regarded as an estimate of the flux systematic errors following Mauder et al. (2013)."

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

Added "*(plants and micro-organisms)*"

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?

Changed all yields to kg DW ha^{-1}.

The range is not global. Changed to "*The annually harvested yield up to 12 000 kg DW ha^{-1} has been reported (Lewandowski et al., 2003).*"

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 some factors (respiration and not GPP rate)

We aim to larger perspective with this section, as not only GHG balance is different between annual and perennial agriculture. Changed to "*As a perennial crop, it has advantages over the annual cropping systems. The crop growth following the first overwintering starts earlier as the re-establishment of the crop in the spring is not needed. This cultivation style also reduces the use of machinery at the site since e.g. annual tilling is not required.*".

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

Added "*to our knowledge*"

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.

Added "*Additionally, we aim to compare our findings from the mineral soil site to the published data on of a RCG cultivation system on a drained organic soil (referred to hereafter as comparison site) in the same climate region..*"

L26, P16675: Typo: quantify.

Changed "quantity" to "*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

While checking the data, we noticed a mistake with the data processing. The values were updated and details on sampling and methods were added.

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

It is a common practice to harvest the crop for the first time after the second growing season.

Changed to "*The biomass produced during the first growing season was not harvested but left on the site. During the following years, the harvesting was done in the spring after the growing season (April 28 in 2011 and May 9 in 2012). Thus, the spring 2011 was the first time when the crop was harvested after its establishment in the summer of 2009.*"

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")

Changed to "*Except for the wind sector from 85° to 130° downwind of the instrument cabin, all wind directions were acceptable because no other obstacles were present and the sonic anemometer in use had an omnidirectional geometry.*".

Sentence was moved at the end of the paragraph.

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

Changed to "*A heated gas sampling line (inner diameter 4 mm, length 8 m polytetrafluoroethylene (PTFE) + 0.5 m metal) with 2 filters (pore size 1.0 µm, PTFE, Gelman® or Millipore®) was used to draw air with a flow rate of initially 6 l min^{-1} (until 31 March 2011).*".

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

The de-spiking procedure was also applied to wind components (u = 10 m s^{-1}, v = 10 m s^{-1} and w = 5 m s^{-1}) and temperature (5°C). This was added to the manuscript.

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

The previous one. Corrected accordingly in the manuscript.

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

We opted not to do the correction, since in our opinion, it still lacks a thorough validation in the field. We are aware of the progress made in this regard (Nakai and Shimoyama, 2012), but we still feel that a solid long term validation of the angle-of-attack correction method is needed.

L17, P 16678: reference needed

The sentence was corrected as the point-by-point dilution correction was applied after the de-spiking, not after the spectral corrections as was incorrectly written in the previous version of the manuscript. We do not have a good reference for this.

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

Added *"We plotted the night-time NEE with u* and found no correlation between the two. Nevertheless, a default u* filter of 0.1 m s^{-1} was used. "*

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.

Changed to *"Flux was considered non-stationary following Foken and Wichura (1996). Generally, a threshold value of 0.3 is used. However in the present study, using this value would have caused a rejection of a lot of good quality data. Therefore, we used a limit of 0.4 (e.g. 40 % difference between the sub-periods and the total averaging period)."*

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

Added *"Footprints were calculated for each 30-min averaging period with the analytical footprint model developed by Kormann and Meixner (2001). The model is valid within the surface layer and it utilizes power law profiles for solving the footprint sizes analytically in a wide range of atmospheric stabilities. Based on the analysis, 80% of the flux was found to originate from within 130 m radius from the mast."*

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

Corrected accordingly.

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). 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)

The section was reformulated as follows *"The EBC is expressed in the following formulation (Arya, 1988) and it is a simplified formula which is valid for ideal surfaces, i.e. with no mass and heat capacity:*

$$Rn = LE + H + G \qquad (2)$$

The EBC was determined using data from only those 30 minute time periods when all of the energy components were available. The slope of the regression was 0.70 in May–September period 2010 and 2011. Incomplete closure is a common problem due to e.g. large eddies (Foken, 2008), angle of attack issues (Nakai et al., 2006) and also because part of the available energy is also stored in different parts of the ecosystem (Foken, 2008). Therefore, EBC was calculated so that it include different storage terms, i.e. heat in the soil, crop canopy, amount of energy used in photosynthesis, sensible and latent heat below the EC mast (following Meyers and Hollinger, 2004 and Lindroth et al., 2010) to give a more precise estimation of the EBC. With this approach, the slope increased to 0.75."

L19, P16679: please insert a colon before formula

Added ":"

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

"a" is needed before common.

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 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?

This section is now removed from the manuscript as it is not valid in the present situation.

L1, P16681: please insert a colon before formula

Added ":"

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

Changed "*below ground*" to "*below vegetation*".

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

All root samples collected in 2011 were lost prior to the analysis.

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?

The weight was checked few times when drying. When the sample weight did not change anymore, it was considered dry. Changed to *"Samples were drying in the oven (+65°C) until the weight of the samples did not change anymore (approximately 24 hours) and dry weight (DW) was measured*."

L2, P16682: please add reference for equation 4

Added Thornley and Johnson, 1990.

L17, P16682: please add reference for equation 5

Added Shurpali et al., 2009.

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,…"

It is true that GPP and TER are always connected. This sentence was not changed.

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

Added "*weeks*".

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

Added "*Measured 30 min values of NEE, H and LE during 2009, 2010 and 2011 prior to the gap filling are shown in Fig. 3.*"

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"

Changed to "*In both years, June presented conditions of high CO_2 uptake during the day and of CO_2 loss at night.*"

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 are estimated from the same main variable (NEE)

This is mentioned in the discussion that NEE is the balance between GPP and TER.

L11, P16686: dot missing

Added "."

L5, P16688: shown

Changed "given" to "*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

We believe that our statement is correct. In the earlier studies, it has been shown, that while cultivated on cut-away peatland, the RG cultivation was a CO_2 sink (Shurpali et al., 2009). In a life cycle assessment at that site, LCA was negative during wet years and still better that the coal during dry years (Shurpali et al., 2010).

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.

Changed to "*During a four year study in Finland, an annual NEE ranging from -8.7 to -210 g C m^{-2} has been reported for a cut-away peatland with RCG cultivation in Finland (Shurpali et al., 2009) and during a one year study in Denmark, an annual NEE of +69 g C m^{-2} was reported for an organic agricultural site (Kandel et al., 2013a). Measurements of CO_2 exchange have been carried out also on other bioenergy crops. On average, annual NEE of switchgrass cultivation was -150 g C m^{-2} during a four year study in USA (Skinner and Adler, 2010). Annual NEE for miscanthus was -420 g C m^{-2} during a two year study in USA (; Zeri et al., 2011). Annual NEE of young hybrid poplar stand in Canada was +37 g C m^{-2} in a two year study (Jassal et al., 2013). Willow stands have been studied in Sweden with an annual NEE value of -510 g C m^{-2} in a three year study (Grelle et al., 2007). Compared to these studies, the annual NEE of the present study is within the range of these previously reposted values from various bioenergy systems.*"

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 generalize the results from the RCG of this study, and I suggest to avoid it.

Changed to "*So, RCG in the present study has a higher capacity for carbon uptake than Scots pine on mineral soils under boreal environmental conditions*." Also, we checked the way the results were generalized.

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

Paragraph from P16689 L27 to P16690 L6 was moved to materials and methods under the new section 2.5 Comparison site characteristics.

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

Changed to "The differences in the nutrient status of the soil types is further borne out by the fact that the mineral soil in the present study had a seasonal N_2O emission from this RCG cultivation system of the order of 2.4 kg ha^{-1} (Rannik et al., 2015), while the comparison site had negligible emissions (Hyvönen et al., 2009). "

L15-18, P16690: please split this sentence

Done.

L11-13, P16691: please report reference values

Values are given in materials and methods under a new section (2.5 Comparison site characteristics).

L24, P16691: please report them

Added values.

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

They are more or less at the same latitude (63.2°N present site, 62.5°N comparison site). Location information of the comparison site was added to the materials and methods section.

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

At the comparison site, the ET was higher than precipitation during the dry years. However, NEE was lower on the dry years.

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?)

The mean temperatures (May-September) at the topsoil were similar between the sites. This aspect was added to the discussion.

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

Added. Also check the consistency in the units throughout the manuscript.

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

Changed "same crop" to "same crop variety" as it has been used throughout the manuscript.

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

We agree that the comparison to organic soil site is important in this paper. However, there are limitations how far it can be taken. As we are only reporting the CO_2 exchange on mineral soil in the manuscript, we are not able to conclude more in relative to the life cycle of the RCG based on the findings on organic soil. For example, the N_2O exchange patters are most likely different between the two sites.

Conclusion was revised.

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

Added "*See Fig. 6 for the relationship of GPP to PAR*."

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.

The CO_2 flux results are reported in varying units in papers. We believe that a results table with different units would give the reader easily the idea of the range of the results in relative to the units that the reader is most comfortable with. Removed the other unit (g CO_2 m^{-2}) from the table.

Year 2009 was left in the table, as it shows the CO_2 exchange of RCG during the first season. We are missing January to end of June in 2009 when most of the time the site was not even cultivated with RCG.

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

There are no open grey circles in the figure.

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

It is true that the biomass produced in 2009 was left at the site and most likely contributing to the respiration in 2010. However, the respiration rate was increasing from 2010 to 2011 even though there was no extra biomass at the site in 2011. The yield of 2010 to 2011 increased also, so it is not possible to determine, based on our data, how much the extra biomass effect the TER in 2010.

Added "*The lack of GA correlation in 2010 could be attributed to the unharvested biomass from the 2009 season. The biomass left at the site may have affected the soil respiration rates in 2010.*".

References

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