The study presents the CO_2 fluxes from a mineral soil in Finland cultivated with reed canary grass, a promising energy crop in northern Europe. The study is topical in the context of increasing interest of bioenergy to combat climate change. However, the study does not compare the energy crop cultivation with current/other land use options, and thus provides only little information for policy intervention. The study uses a state of art method to measure CO_2 flux and describes it well in the manuscript. The results are also well presented but the discussion is not very impressive (see the comments below). I suggest publishing these papers in Biogeoscience but the authors need to address these suggestions and corrections:

We thank Anonymous Referee #1 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 #1 followed by our responses which are marked in blue.

Major comments/suggestions

1. Finland has been leading country in terms of RCG cultivation for bioenergy production in northern Europe. Please provide the current status (area and cultivation and if it is increasing or decreasing; primary use of biomass eg. Combustion or biogas) of RCG cultivation in Finland.

During the recent years, the reed canary grass production has decreased in Finland due to technical problems associated with the burning process. As a result, the land area under reed canary grass production has decreased to 5839 ha in 2014. Although the current area coverage is low, the potential to produce reed canary grass in Finland still exists as there is an interest on using the biomass on e.g. biogas production. For the future policy making, the knowledge on climatic impacts of reed canary grass cultivation on different soil types is still needed.

Added to introduction "*Cultivation of RCG has been popular in Finland since the mid-1990s and at the peak approximately 19 000 ha (2007 and 2008) were cultivated with RCG. However, owing to technical difficulties with the burning of the RCG biomass in combustion plants, the scope of RCG as a source of biomass bioenergy has declined in the last few years. In 2014, the average cultivation area was around 6000 ha. Nevertheless, the scope for RCG as a source of liquid biofuel, a digestate in biogas plants, oil spill absorption and a buffer crop between terrestrial and aquatic landscape is wide (Pasila and Kymäläinen, 2000; Partala et al., 2001; Powlson et al., 2005; Kandel et al., 2013b)*".

2. In discussion, the paper compares the CO2 fluxes results with many different types of biomass crops cultivated in different ecological zones which I think is not so interesting and useful. For example, comparing CO2 fluxes from RCG cultivation in Finland with hybrid poplar in Canada or Switchgrass in USA is neither useful to validate the results nor for policy intervention. It would be more useful to compare the results with previous studies in Finland which have measured CO2 fluxes from mineral soil with arable crop cultivation. Such comparisons would provide idea for land use change to bioenergy systems from arable cropping systems.

â A c The paper has also particular focus in comparing CO2 fluxes from mineral soil and cutaway peatland. The cutaway peatland is a margin soil and we can expect very small biomass production, and thus GPP and TER from such poor soil. Nevertheless, these types of soil can be useful to cultivate bioenergy crops even the biomass production is small. As oppose to the cutaway peatlands, there are many options to cultivate in the arable mineral soil. Therefore, as mentioned earlier**,** a comparison with current crop cultivation in mineral soil and biomass crop cultivation would be much more interesting. It would be best if the study had also included parallel CO2 flux measurement with arable cropping system but comparing results from previous studies will also be useful to understand environmental impact before changing land use to biomass crop cultivation.

The referee is correct on bringing up challenges with the data comparison. To our knowledge, there are no published data on CO_2 exchange of reed canary grass cultivation on mineral soil site. That is why we compared the results to that of reed canary grass on organic soil. In order to give the reader, and policy makers, a better understanding how our results fit scale of published CO_2 exchange results, we included other CO2 exchange studies done on bioenergy crops/forests to the comparison. The references used in our paper, report annual CO_2 exchange which were obtained using eddy covariance method and the sites were mineral soils. To our understanding, this type of data on crops or grasses in Finland is not available.

We also agree with the referee's suggestion that a concurrent measurements of different crops would have been interesting. However, due to the requirements on the eddy covariance method, we were not able to do it.

3. The maximum crop yield in winter is about 11 and 16 ton DM ha-1 in 2010 and 2011, respectively. However, it seems the senescence and dispersal loss of biomass is quite high as the spring harvest only yielded about 6.2 and 6.6 ton DM ha-1 in 2010 and 2011, respectively. Although the biomass quality can be improved with spring harvest, but it may not be an economically better option as 44 and 58% of total aboveground biomass (the difference between autumn and spring harvests) was lost in spring harvest. It is surprising to see the large dispersal loss as the leaf may have only about 20% of total biomass after the growth season in autumn (Kandel et al., 2013. Bioresource Technology, 130, 659–666). Probably the concentration of minerals in biomass does not change considerably when the growth of the crop stops. If that is the case, harvesting very late in spring may just contribute to reduce harvestable biomass yield. More discussion is needed on autumn and spring harvest time as the difference in biomass removal in autumn and spring harvest is very large which can have large effect in CO2 fluxes. Probably, much higher TER can be expected in coming years if biomass is harvested in spring as major portion of biomass is left in the field.

We agree with the referee with the difference in the autumn and spring harvest. However, the values are not directly comparable as there are difference in the sampling scale and sampling method which have an effect on the results. The autumn collected biomass samples was sampled from a 20 * 20 cm^2 area from three locations within the field. This type of small scale sampling is prone to the variation in the plant growth and density within the field. Also the sampling was done by manually clipping the individual plants as close to the soil surface as possible and collecting them with care. The spring harvested yield is collected with a field scale machinery following the common practice of the reed canary grass biomass harvesting in Finland. A biomass loss between 20 to 30 % has been reported when using field scale harvesting (Pahkala et al 2005. Maa- ja elintarviketalouden tutkimuskeskus, Jokioinen, 2005 (report, in finnish only)). It has been reported earlier that the total dry matter harvested in the spring time has been higher than that in autumn time when same method of harvesting was used (Pahkala and Pihala, 2000, Industrial Crops and Products, 11: 119–128). In our study, the difference between autumn and spring harvest is not 44 to 58 % for 2010 and 2011, respectively, but less than that if the same method would have been applied.

It is true that the leftover biomass at the site will have an effect on the TER. In our study, we did not determine how much of the biomass was left to the field after the harvesting. And based on our study, we cannot draw conclusions whether there would be difference in the leftover biomass if the harvesting was done in the autumn or in the spring.

We think that the timing of the harvesting should be selected based on the use of the biomass. In the spring time harvest the biomass quality will be different when compared to that of autumn harvest and also the ratio of stem to leaves in the harvested biomass will increase in the spring than in the autumn (Pahkala and Pihala, 2000, Industrial Crops and Products, 11: 119–128). In our case, the crop was produced for biomass burning and we followed the cultivation practice that aims to produce biomass with best possible quality for the burning. As we mention in our paper, the spring harvested biomass has been found to be better suited for burning when compared with the autumn harvesting.

The methodological differences of biomass collection and the reason for spring harvesting were clarified accordingly in the manuscript.

4. Although it is mentioned that a detailed LCA is out of the scope of this paper, including biomass removal (calculation of net ecosystem carbon balance) would be interesting to judge the sustainability of the ecosystem. Also, I suggest calculating fossil fuel displacement by the harvested biomass to get a more complete atmospheric impact.

We agree with the referee that these steps would increase the value of the paper. We have chosen to include the CO_2 exchange aspects in this paper as we are also working on a complete LCA analysis of the reed canary grass cultivation on mineral soil in a subsequent paper.

5. There is no mention about energy balance in title, abstract and introduction of the manuscript. It seems the objective of manuscript is nothing to do with energy balance. Therefore, I suggest either to remove energy balance part or to describe more in introduction why it was important to measure. It is used for calculation of water use efficiency of RCG but that is also not a main objective of the paper.

It is true that the energy balance is briefly handled in the manuscript and it is not mentioned in the title, abstract or introduction.

We use the energy components in energy balance closure determination to show how successful the measurements were and also to determine the systematic error in the measurements. It is also important to show the raw data behind the results upon which we based our conclusions. Therefore, we have increased the information on the eddy covariance method in the manuscript.

Minor comments:

Abstract, Line 16. The study period is not clearly defined in abstract. Therefore, either define it clearly or delete that sentence.

The time period in the abstract was defined clearly and the sentence is written now:
"*Throughout the study period from July 2009 until the end of 2011, cumulative NEE was -575 g C m^{-2}*."

Page 2, Line 10: Cutaway peatland probably do not emit large amount of CO2 from when the emission is compared with arable organic and mineral soils. A recent paper by Vanselow-Algan et al. (Biogeosciences, 12, 4361–4371, 2015) has shown very small CO2 emissions from cutaway peatland compared to other types of organic soil. Does the Kasimir-Klemedtsson et al., 1997 cited here mentions high CO2 fluxes from cutaway peatlands? Here, TER in this paper is much higher compared to Shurpali et al. 2009 which was probably contributed by high biomass yield in mineral soil. I wonder if it is possible to do estimate SR from both sites and compare SR results. That would be interesting as a major portion of TER may have come from the plant biomass and diluted the effect of soil types in CO2 fluxes.

In a review by Maljanen et al. 2010 (Biogeosciences, 7: 2711–2738) the average net CO_2 exchange of cutaway peatland in Nordic countries was estimated to be around 200 g C m^{-2} a^{-1}. The findings of Vanselow-Algan et al. 2015 (Biogeosciences, 12: 4361–4371) on the active cut-way peatland were equal with this. The net Co2 exchange of a cultivated organic soils range from 80 to 820 g C m^{-2} a^{-1} and of a previously cultivated but now abandoned sites from 110 to 810 g C m^{-2} a^{-1} (Maljanen et al. 2010). Based on this, the CO_2 emission is lower from the cut-away areas than cultivation sites. The Kasimir-Klemedtsson et al. 1997 (Soil Use and Management, 13: 245-250) are referring to emissions due to drainage of the peatlands and the decomposition of leftover peat.

It is correct, the TER in the present paper is affected by the higher biomass yield in the mineral soil. This is a topic for another paper and thus out of scope of the present one.

Page 2, Life 26: Change quantity to quantify

Quantity changed to "quantify"

Page 4, Line 8: It is not clear why the May-September precipitation was mentioned with focus. In the manuscript, it is not mentioned earlier that it was a growing period of RCG in Finland.

Length of the growing season varies between the years and it is not fixed to May-September period. The lengths of the growing seasons are given in the results section. The precipitation of May-September was given, in addition to the annual value, as it is a fixed time period and allows us to easily compare the long-term mean precipitation to that what we measured during our study.

Page 4, Line 15: It seems there was large difference in C concentration in soil. Was the land in the transition between mineral and organic soil? If so, was there a trend with higher TER fluxes measured from footprint which has higher soil C concentration?

The section in materials and methods considering the soil characteristics has been now properly checked and updated. Also, as the soil sampling did not cover the entire field (only three locations), we would have not be able to divide the flux data based on the soil characteristics.

Page 5, Line 14: Why was it discarded? This information is repeated in line 19, page 6

The presence of the cabin interferes the wind in the discarded direction. This part was clarified in the manuscript:

"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.".

Repetition of the information in line 19, page 6 was removed.

Page 9, Line 12: The results from fresh weight are not presented later in result section. Therefore, it is better to delete this sentence as fresh weight yield is not so interesting information in this manuscript. If the result is included, then it is important to mention why moisture content is an important quality for biomass conversion especially in spring harvest for combustion.

This sentence was removed from the manuscript.

Page 10, Line 9: Was this temperature relation not fitting well for gap filling purpose?

We have compared the results from this site with those from an earlier study by our group. To allow for a better comparison, and to make the analyses consistent, we used this relationship.

Page 11, Line 6: in the end of the sentence add 'than the long term mean'.

Added "*than the long-term mean*"

Page 11, line 22-26: Probably the relation between GPP and ER and WUE does not fit under this subheading.

ET is related to seasonal climate and plant growth. As WUE is calculated from ET and GPP, it was natural for us to include the WUE results together with ET.

Page 12, Line 4: This sentence seems incomplete. Is it 9 weeks?

Added “weeks”

Page 12 (Fig 3): Some scattered points in winter are showing up to 10 to -10 micromole CO2 m-2 s-1. Probably these points represents spikes as there is very less probability of having such large photosynthesis and respiration in winter in Finland.

The raw data was thoroughly checked and flagged prior to the calculations. In spite of this, some scatter was left in the data. However, there was no reason to remove those data points.

Page 14, Line 11. Place a full stop after respectively.

Added “.”

Page 19, line 21. Earlier studies have shown RCG can have maximum yield potential in 2nd to 3rd year of establishment. Therefore, a decline is more likely with ageing stand of RCG.

It is true that the yields during the following years would be similar with the second harvest. In multi-year study carried out in Finland, the spring harvested yields were increasing significantly from first to second harvest after which the yields remained fairly constant for the next six years (Saijonkari-Pahkala, 2001, Non-wood plants as raw material for pulp and paper, PhD Thesis). What is interesting in the present study is that the root biomass was low. We are expecting an increase in the root biomass as the crop ages. In theory, more developed root system should enhance the viability of the crop and, therefore, lead to higher aboveground biomass yields.

Page 20, Line 19-20. Previously it has been mentioned RCG has very shallow roots mainly concentrated on 0-15 cm. Here it is written that the plants can take water from deeper layers to cope drought stress. This is contradictory claim as the short rooted crops can be highly affected by drought.

The shallow root system where 95 % of the roots were concentrated on the 0-15 cm layer was reported for the comparison site on organic soil. In the present study, 70-80 % of the root was in 0-10 layer. In the present study, it is possible that the roots reach deeper layers in the soil. Also the water movement and availability is different between the soil types: water is easier available to the plants in mineral soils. This is one of the strong points of this comparison study showcasing the differences in the two study sites.