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

Anonymous Referee #1

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The study presents the CO2 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 CO2 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:

Major comments/suggestions

1. Finland has been leading country in terms of RCG cultivation for bioenergy produc-

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tion 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.

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.

âĂć 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.

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 har-

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vest. 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.

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.

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.

Minor comments:

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

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

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cutaway peatlands? Here, TER in this paper is much higher compared to Shurpalil 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.

Page 2, Life 26: Change quantity 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.

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?

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

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.

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

Page 11, Line 6: in the end of the sentence add '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.

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

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of having such large photosynthesis and respiration in winter in Finland.

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

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.

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 dipper layers to cope drought stress. This is contradictory claim as the short rooted crops can be highly affected by drought.

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