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Interactive comment on "Responses of carbon dioxide flux and plant biomass to drought in a treed peatland in northern Alberta: a climate change perspective" *by* T. M. Munir et al.

Anonymous Referee #2

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manuscript by Munir et al.

General comments

This manuscript presents a case study where the impact of drought – or lowered water level – on CO2 fluxes and plant biomass is examined. The arrangement is interesting in that sense that the control and drained plots are situated very close to each other, having the same climate. In addition to comparison of the drained and control plots, a comparison is also made between two climatically different years. The authors conclude that 10 yrs after the drainage both the biomass and CO2 balance has radically changed. Extrapolating the results to warmer and drier climate, as done in the paper in a qualitative way, would lead to dramatic loss of CO_2-C from Canadian treed bogs. In fact, the loss of C reminds me of that in the drained peatlands taken into agricultural use. The fact that the fluxes shown in this paper only represent the growing season, makes the annual C loss even greater. This makes me to suspect the quality of the CO_2 flux measurements, which I see is the most vulnerable and uncertain part of this paper.

First of all, I do not find any description of how the respiration has been measured. The chapter 2.2 only describes the measurements done on light conditions. However, I assume that respiration has been measured either by doing the measurements in night time, or by darkening the acrylic chamber by a dark cover during daytime. I ask the authors to add a detailed description of the respiration measurements.

Secondly, assuming that the respiration really was measured, I think the flux calculation method used here is problematic, and now I'm meaning mainly the respiration measurements. Recently, it has been reported that the gas flux measurements in the porous peat soils - such as bogs with sphagnum-covered hummocks and/or lowered water level – are prone to big errors due to the disturbance of the gas gradient within the peat soil caused by the fans in the chamber (Lai et al. 2012, BG 9, 3305–3322; Koskinen et al. 2013, BGD 10, 14195-14238). This causes excess flushing of CO_2 out from the soil in the beginning of the measurement which can misleadingly seem to follow the exponential saturation in the concentration increase reported by many authors, and which has increasingly lead to use of exponential fit when calculating the fluxes. Lai et al. concluded in their study that the most correct flux values are obtained when calculating the flux ca. 13 minutes after chamber closure. Koskinen et al. on the other hand used data closer to the start of the measurement, but they still removed the first 2 minutes of the concentration data. What comes to the present study, in which the fluxes were calculated using the exponential function, it is possible that this overestimation is the main reason behind the huge CO_2 emissions from the drained site with thicker aerobic, dry and porous peat layer as compared to the control site. The problem is further exacerbated by the use of two fans in a 30 cm tall chamber.

All this makes me to suspect that the high fluxes result from the release of stored CO2 rather than being a real biological phenomenon. This is critical for the conclusions of the whole paper, as the idea of the paper is to compare fluxes on drained and non-drained peatlands; the phenomena is more severe on more drained and porous peat soils. Therefore I suggest that, for the respiration data, the authors check the linearity of their concentration data, and in the case of non-linearity recalculate the fluxes using as late and short fitting time as reasonable. I'm aware that their short measurement time of only 1.75 min may not necessarily allow the inspection of this problem. In that case, the authors could make some kind of sensitivity analysis (i.e., calculating the balances by applying some typical values of underestimation, taken from the above-mentioned papers, to their data).

To conclude, it is hard for me to believe that such a great seasonal CO2 emission from their drained site can be possible. I might be wrong, but I would like to see this tested. Related to this issue, I also would like to see some comparison to earlier studies regarding the CO2 balances, now the quantitative literature review on CO2 fluxes is totally missing, which would put this result in a better perspective.

The biomass part is more convincing I think, and makes a strong case on the impact of drainage on the biomass. In general, the paper is well written and relatively clearly presented, and the subject is definitely topical and within the scope of the journal. More detailed comments follow.

Specific comments

p. 15003 lines 17-20: the sentence is strange, please reformulate

15005, 8: 60 cm x 60 cm x 30 cm

15006, 7-8: since they have an important role in the paper, please specify what exactly are GEPmax and NEEmax? How they are calculated? Are they related to one

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measurement occasion or do they represent longer period or average?

15006, 11-12: please reformulate the sentence. It is not clear, if the parameterization was done separately for different years or not, and if not, why? Do you think the parameters should stay the same during different years, so that all differences are only arising from meteorology?

15006, 15: remove comma after “where”

Chapter 2.2.2: which data was used for validation?

15008, 25: define H and W, 25 cm x 25 cm

Chapter 2.3, last paragraph: would be useful to see the results of tree C uptake somewhere

15010, 11: Fig 1 should be Fig. 2

15010, 14: can you really say that 2012 was warmer? The difference was so small (0.02 deg) that I don’t think it’s significant.

15010, 17: “trenched” should probably be “drained”

15013, 1: can you really say that the difference between 40 and 13 g C m-2 is significant? Would be good to have uncertainties for these numbers.

15013, 8: related to an earlier comment, may be you cannot say 2012 was warmer

15013, 14: Aurela et al. (2004), perhaps you should refer to Aurela et al. (2007)?

– Table 1: What is “SEE”? Please give number of observations (n=?). Again, please indicate if both years are included in the parameters.

Table 4: please give the uncertainties for site NEE’s, to help to see if the differences are significant or not

Figure 1: You could mention also here that the measured data in this fig was not used

for the model parameterization, and how was this data selected (may be it's enough to give the latter in the text)

Figure 2: add "2011" and "2012" in the figure below the x-axis

Figure 4: Perhaps because I have problems understanding the definition of GEPmax and NEEmax, and what time periods they represent, I don't understand why you are not showing the seasonal fluxes here, and what is the reason for comparing instantaneous values (and have they been measured at the same time, e.g. GEPmax and Rr?). Again, explain H and W, the figures should work independently from the text.

Interactive comment on Biogeosciences Discuss., 10, 14999, 2013.

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