Comments on the paper by Lohila et al., bg-2011-175.

General:

The article discusses the GHG exchange from a forestry drained peatland in Finland. This study is relevant to climate change as such datasets are rare indeed. Despite a large Finnish area under such ecosystems, whole ecosystem scale eddy covariance measurements of CO2 exchange have not yet been reported. This study is an attempt to fill that gap. The study is well organized; proper data collection systems are employed. The paper is concisely written. In my opinion, there are some issues that still need to be addressed in the paper. Those specific issues are discussed below;

Abstract

The first line of abstract sounds vague.

I was rather surprised to see the usage of the phrase "likely to change " when several papers published by these authors have clearly shown that the GHG biogeochemcistry does change after the drainage of natural peatlands.

Line 17: change 'loose' to 'lose'.

Line 18: Please qualify the ecosystem by the major forest/vegetation type .

Lines 21 -22: Relatively little change in WT level – compared to what? Compared to the natural peatland that existed there? What was the basis for comparison?

Lines 22 – 24. The range in NEE – what does the range refer to? Please clarify.

Line 30-32: While stating the novelty of this work, the authors fail to recognize the fact that there are other studies in Finland that have reported the drained peatlands to be sinks for C. The novelty of this work is that a forestry drained peatland has been shown to be a C sink with EC for the first time. Please qualify the ecosystem studied appropriately.

Also the last sentence of this section is incomplete and therefore, not clearly readable.

Introduction

This study refers to a paper (Pihlatie et al 2010) that has reported data collected from the same site after the time period which the present paper refers to. There is nothing wrong with that. But a question does arise in readers' minds. If more data exist, why then the specific limited time period has been selected for inclusion in this paper? As the authors duly realize, C exchange measurements vary from one season to the other. Inclusion of data from other years would be scientifically more rewarding.

Materials and Methods

Line 86: The sentence about the drainage does not agree with your own statement in the abstract.

The authors do not say anything about the energy balance closure at the site although they have all data needed to assess this aspect of EC measurements.

Line 185 : Modelled values – how were the values modeled? This appears for the first time in the paper. The reader is left wondering about modeling until the reader reaches the gas filling section discussed in the results section. I would therefore, suggest the authors to move the gap filling section to the M&M section.

How was the storage flux calculated? Reference to a paper that describes the method?

Line 177: Why 70% was chosen as the threshold level for representativeness as estimated by the footprint model? Why a more stringent limit has not been set?

Results

Line 224: Meteorology, in my opinion, is not a proper title for this section.

Line 308-310: Please specify the 365-day periods.

Lines 311-312: Please note that the NEE is positive during the growing season only after the peak NEE.

Lines 395-397: Without any detailed hydrological characterizations, these statements about water movement within the peatland appear to be speculative and should be indicated as such.

Line 404: The sign convention for C flow due to leaching reversed here? Any flow out of this ecosystem is assumed to positive.

Lines 456 – 474: As the relationship between NEE and VPD could be confounded by TER, could the authors look at the relationship between GPP and VPD?

In conclusion, I suggest that the paper be accepted for publication after the authors address the above comments.