The manuscript deals with the C balance and CO2 and CH4 emissions of drained peatlands, which is indeed an important topic and suits well for publication in Biogeosciences. The manuscript is generally well-written and the methods are explained with sufficient detail. The comparison of two methods is important as the uncertainty of each method is otherwise difficult to assess.

The finding that the average C loss since drainage is considerably lower than the current C loss is interesting and possible explanations are thoroughly discussed. Unfortunately, it does not become clear, if this difference could be simply due to bias in either method.

This is for three reasons:

1) For the CO2 chamber based method, only the resulting annual C budget is reported and its components (see eq. 6) are not shown anywhere. This prevents the reader from evaluating the reasonability of these values. Moreover, it seems that only CO2 and CH4 measurements are included in error propagation. Also, there is no explanation on how the values for C input and output (manure, fertilizer, seed, harvest) where obtained and what is their uncertainty.
2) The error estimate of the secondary subsidence is based only on the spatial variation between the four cores. It is for example stated that the use of deeper horizon as reference may cause a considerable bias, but no numerical estimate of that is given. There must be some published results on stratigraphy of this type of peatlands that can be used to roughly estimate the magnitude of the potential bias. Also, would not the biomass and fertilizer inputs and outputs affect the ash content, can this be ruled out as insignificant?
3) The C loss due to secondary subsidence (eqns. 3-5) is calculated in rather a strange way. First, the thickness the "original" peat layer is calculated comparing the (volumetric?) ash concentrations, which I think is okay. But then the C loss is calculated assuming the original peat layer loosing thickness with C density of the reference layer. Would not the "right" and most straightforward way to calculate the change in the C storage be to calculate the difference between the "original" C storage (sum i = 1 to n $[C_r/T_r*T_{0i}]$) and the current C storage (sum i = 1 to n $[C_i]$)? On page 12350, some motivation for the method is presented, but it is quite fuzzy. If this method is suggested for general use as a good, unbiased method, a more precise description of how the equations were derived would be useful.

After a revision considering these issues, this manuscript can be a good contribution.

Some specific comments:

Manuscript

p 12349 r 1-5: You have V_{0i}, V_i and V_0. Are they three different things? Only V_0 is explained!

p 12349 r 13 What is the unit of ash concentration? Is it volumetric or gravimetric? Shouldn't it be volumetric in this kind of calculation?

p 12350 r 17: "during May 2007 and April 2012" Only those two months or rather from May 2007 to April 2012?

p 12351 Equation 6: what does the summing over i = 1 to n stand for? I see no mention in the body text.

p 12355 r 23- There are published values of DIC and DOC export. Why not to include those in equation 6 or at least make a quick calculation here in discussion to evaluate how much they could affect the chamber based C budget?

p 12357 r 2 & r 17 Why should the increase be “non-linear”. What does that stand for here?

p 12358 r 8 onwards: Couldn’t you use also t/ha units in this section as you are doing in all the previous section? Would make comparison easier.

Supplement

Fig. S1. Please include a scale bar into the picture. The notion of the original scale does not help very much when the map is rescaled to the computer display.

Fig. S2. Why use kg m-2 here, when t ha-1 is used elsewhere in the manuscript?

Fig. S3. Please clarify, if “CO2 losses” refers to measured respiration or NEE or C balance or what?

r. 88-95 which temperature (2 cm, 5 cm and 10 cm soil depth or air temperature in 20 cm height) did you use for calculating annual respiration flux?