Biogeosciences Discuss., 10, C1543–C1547, 2013
www.biogeosciences-discuss.net/10/C1543/2013/

Biogeosciences Discussions

BGD

10, C1543–C1547, 2013

Interactive Comment

Interactive comment on "A fertile peatland forest does not constitute a major greenhouse gas sink" *by* A. Meyer et al.

Anonymous Referee #1

Received and published: 6 May 2013

This paper presents an annual balance of CO2, CH4 and N2O exchange of an afforested peatland in Sweden. Two independent estimates are produced for CO2 by employing a wide range of measurements of different components of the balance, including micrometeorological, chamber and biomass measurements. The paper is very suitable for the scope of BG. It presents a full GHG balance for a drained fertile peatland, which has not been determined earlier. An important conclusion that such an ecosystem does not constitute a significant GHG sink was reached. The presentation is mostly clear and fluent. I would recommend publication after a revision that should carefully consider the following points.

General comments

(1) Two different approaches are used to estimate the CO2 balance, which in prin-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

ciple can be considered a strength of this study. However, the way the estimates are presently interpreted actually constitutes a major weakness. While it is obviously useful if the magnitude of different component fluxes can be determined, the interpretation of the results appears problematic, as the overall balance obtained this way significantly differs from that measured with micrometeorological techniques. In this respect, the authors should formulate the discussion of the results and the final conclusions more carefully. The uncertainty estimates (and how they are defined) play a key role here. For example, in the beginning of Section of 4.2 it is stated that the approaches "gave different results", but due to measurement uncertainties the results are "actually not different". This sound very vague, as does the discussion on the nature of the error propagation scheme that follows.

(2) Related to the previous comment, the uncertainty estimation procedures should be made more explicit. It is not clear how the standard errors are calculated for Eq. (13) and what "the respective components" (p.5121, l.14) are. Based on the text and the data presented in Table 4, I assume that the error propagation principle is applied with standard deviations and that these uncertainty estimates only represent the variation between the three stations. The statistical rationale/test adopted for comparing the mean +/- s.d. values obtained in this way should be presented.

(3) The geographical context of the study should be defined more exactly. The introduction is very much focused on Sweden, while Finnish studies are used for comparison. In Section 4.2. some other studies are mentioned but not considered comparable because "site conditions differ considerably". The authors should define the conditions that their results represent and outline the region within which comparable organic soils can be found.

Specific comments

p.5109, l.11: Which drained organic soils, those in Sweden?

p.5109, l.24-25: These ranges are based on a limited set of studies, cf. Maljanen et al.

BGD

10, C1543–C1547, 2013

Interactive Comment

(2010).

p.5111, l.4: Lohila et al. measured N2O and CH4 fluxes with chambers rather than using micrometeorological methods.

p.5111, l.10 and elsewhere: Which GHG compounds?

p.5111, l.10: Minkkinen et al. (1999) is probably not an appropriate reference here.

p.5111, l.10: "Klemedtsson et al. (2005) found ... (Klemedtsson et al., 2005...)" sounds awkward; please rephrase. What is the geographical coverage of these data?

Section 2.1: Please indicate the forest height.

p.5112, l.23: 'Please explain 'BA' (defined on p.5118).

p.5116, l.8: Which fluxes?

p.5116, l.12: Lindroth et al. do not provide a full description of the data post-processing procedures. More details should be presented (e.g. WPL term, compensation for high-frequency attenuation).

p.5116, l.16: Unclear what is meant by 'spikes' here.

p.5516, l.17: How large was the storage term as compared to the eddy flux?

p.5516, l.18: Is this "biotic flux" the same thing as NEE?

p.5516, l.20: Unclear what is meant by "eddy covariance criteria"

p.5517, l.13: The Kljun et al. model (parameterisation fit) also works in stable conditions, at least up to z/L<1. The Kormann and Meixner model employs stability correction functions that in principle are not valid beyond this limit either.

p.5518, l.25: Please describe the chamber measurements.

p.5519, l.2: Why is root litter decomposition (R_LR) excluded from Eq. (10)?

BGD

10, C1543–C1547, 2013

p.5119, l.25: Eq. (11) is confusing. The constant 0.997 actually represents the decay time (in years) and corresponds to the measurement period of Ngao et al. Please clarify.

p.5121, l.13: NEEcalc has not been defined.

p.5121, l.21: Please indicate the measurement height (1.5/22m?).

p.5122, l.10: The discussion of source areas is too qualitative and vague ("in general", "occasionally", "prevailing wind direction"). The conclusion that the measured fluxes were "certainly" dominated by the forest stand needs more justification.

p.5126, l.9: A value of 8.2 is not within a range of -0.8 to 6.7.

p.5126, l.17: The estimate of random error requires a reference.

p.5127, l.1-: The uncertainty estimate depends on the number and length of gaps, as well as on the gap-filling technique adopted. While most estimates reported by Moffat et al. are within the range cited, a more thorough discussion should be presented, as no proper uncertainty calculation is presented in the paper for the EC data. Moreover, the value of 15% adopted may not represent a conservative estimate, as only a few error sources are considered.

p.5127, l.5-: This kind of discussion is too general to be really useful. A quantitative estimate could be calculated for the source area effect by using the footprint model described previously.

p.5127, l.14: It would be trivial to check the relationship between NEE and wind direction from the existing data.

p.5131, l.1: It is illogical to state that "the site is either a GHG sink or source, depending on the approach". Whether a site is a sink or a source obviously does not depend on measurement methods.

Table 3: The tree growth rate at Kalevansuo is incorrect. The NEE value for Alkkia is

BGD

10, C1543–C1547, 2013

Interactive Comment

from Lohila et al. (2007), not Lohila et al. (2004). The first SOM respiration value (-0.8) for Alkkia should be removed. It seems to be taken from Lohila et al. (2004), but that paper deals with an agricultural peat soil growing barley and grass.

Table 4: If the CH4 flux is -4.4 kg CH4 ha-1 yr-1, then wouldn't this correspond -0.1 tCO2eq ha-1 yr-1?

Table A1: What is 'flux integration'?

Fig. 2: It would be useful to indicate if longer periods of NEE data are based on gap filling. Do these NEE data include the storage term?

Technical corrections

p.5110, l.23: Should be Maljanen et al. 2003b?

p.5111, l.27: 'Christansen' should read 'Christiansen'

p.5112, l.5: '2011' should read '2012'

p.5113, l.3: Incorrect grammar

p.5113, l.5: 'water table' should read 'water table depth'

p.5516, l.23: A wrong unit for friction velocity

p.5517, l.12: A wrong Kljun et al. paper in the reference list

p.5122, l.19: 'August' should read 'July'?

p.5132, l.10: Incorrect grammar.

Table 1: 'Minkkinnen' should read 'Minkkinen'

Fig. 2: 'maj'?

Fig. 3: The error estimates of R_SOM and E_CO2 are interchanged.
