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Biogeosciences Discussions

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

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The study presents the a one year greenhouse gas balance of a drained, fertile peatland forest in Southwestern Sweden, which has been afforested 60 years previously on agricultural land. Based on two different measurement methodologies for NEE, measured N2O and CH4 fluxes and estimated biomass and litter carbon balances, the site was estimated as a net greenhouse gas sink or source, depending on the NEE methodology chosen. The study fills a gap in a so far understudied land-use type on peat soils, which covers a significant fraction of European peatlands. The study is very carefully made and provides an unusually robust assessment of methodological uncertainties. It shows the first full greenhouse gas balance of an afforested coniferous forest on drained fertile peat soil. The study fits well in the scope of Biogeosciences.

The manuscript is very clear and well presented. All tables and figures are clear and needed.

There are a few minor issues, which should be revised.

General comments

General 1: The uncertainty of soil respiration measurements (interpolation to annual flux) and N2O emissions should be further analysed and discussed. The measurements are only made fortnightly, so that important N2O emission events may have been missed, or extrapolated over too long periods. Ignoring the temperature response of Rsom could also produce bias in annual flux estimates.

General 2: Discuss the relation of CO2 and greenhouse gas fluxes with water table in more depth, since water table could explain to a large degree why soil emissions were so high.

General 3: Discuss the implications of the findings that the soil is losing carbon while the biomass is accumulating carbon. However, the biomass will be harvested and used so that this carbon returns to the atmosphere as CO2 at some time in the future. What really counts for climate mitigation is the long-term sink in the soil. However, the studied site represents a large CO2 source, much larger than the Finnish forest sites published so far. This is an important finding of the study, which should be highlighted more. These aspects should also be included in a revised Conclusion section.

General 4: The introduction highlights the emission factors used in the Swedish national inventory and the IPCC Guidelines. This aspect should also be taken up again in the Discussion section.

Specific comments:

Page 5109 line 4: Neither Smith and Conen 2004 nor Davidson and Janssens 2006 focus on organic soils; I suggest to delete these reference.

BGD

10, C897–C901, 2013

Page 5109 lines 5-17 and page 5110 lines 15-18: there are several misunderstandings and wrong statements regarding the national inventories under the UNFCCC and the IPCC Guidelines. First, within the LULUCF sector, emissions from drained organic soils (CO_2, N_2O except agricultural soils, CH_4 optional) are explicitly reported and calculated in each land-use category (e.g. forest land, land converted to forest,. . .) as part of the soil pool, under organic soils. Reporting for CO_2 and N_2O from drained forest organic soils is mandatory (IPCC GPG 2003 and IPCC 2006 Guidelines, chapters on forest lands). Default emission factors for N_2O from drained organic soils under forest are given in these chapters, not in GPG 2003 Appendix 3a.2, which only refers to organic soils, which have are not forest, cropland or grassland. The default emission factor, however, is much lower than the emission presented in this study. This is worth mentioning. The default values, however, are in the process of being updated by the IPCC Wetlands Supplement, which will be published in autumn 2013.

Page 5113 line 25: The soil moisture values seem not to be used in this study. Or did I miss this? There is very little information in the literature about TDR calibration in peat soils. It would be interesting to include the site-specific TDR calibration in the presented results.

Page 5114 line 4: add reference to Forster et al. 2007 to show which GWP values have been used.

Page 5115 lines 9-15: The way in which Rsom is simplified requires Rsom measurement by trenching and litter exclusion, or similar considerations. This is described later, but should be made clear here that typically, Rh is measured by soil respiration measurements, and Rsom is measured (or tried to be measured) here.

Page 5115, line 26: add the canopy height and how many metres the CO_2 measurements have been performed above the canopy.

Page 5118 soil respiration: describe measurement set-up, number of replicates and frequency of measurements.

BGD

10, C897–C901, 2013

Page 5119: there are a lot of estimates and model assumptions needed to convert the measured soil fluxes to Rsom. An alternative approach could be to measure Rh minus litter input (or litter decay) minus root turnover (as assumed in this study). I suggest to compare this approach with the data from the trenching to better quantify and assess the methodological uncertainty of soil respiration measurements.

Page 5120 lines 14-18: Litter input is not constant since afforestation so I am not convinced that the approach used here is valid. An approach of dividing litter mass by the time since canopy closure or similar, or of taking total litter production LL since afforestation divided by time since afforestation, might be more appropriate.

Page 5120 lines 23-27: chamber size, number of replicates and description of "station 1, 2, 3" are missing. Page 5121 lines 5-12: please describe station 1, 2, 3. I do not understand the sentence "the same procedure was applied to the 2- to 3- week periods when respiration was measured". Which procedure? Averaging the measured values? Averaging is only allowed when there are no gaps or uneven time intervals. Soil respiration typically shows a strong correlation with temperature and should be interpolated to annual fluxes by temperature response functions, unless measurements are performed at high frequency and representatively across the diurnal temperature cycle (if present).

Page 5122 line 22 and page 5123 lines 1-3: please quantify the uncertainties.

Page 5127 line 1: the unit should be 0.25 t C ha-1 yr-1

Page 5130 line 16: typing error: "Yamulki"

Page 5132 line 20: the methodology 2 has resulted in a greenhouse gas source, so you should be consistent with your findings and say that the findings were ambiguous within uncertainties.

Table 4: It appears more logical to me to present the results in C equivalents rather than in CO2 equivalents.

BGD
10, C897–C901, 2013

Interactive comment on Biogeosciences Discuss., 10, 5107, 2013.