Biogeosciences Discuss., 9, C3870–C3873, 2012
www.biogeosciences-discuss.net/9/C3870/2012/

Biogeosciences Discussions

Interactive comment on "Soil CO_2, CH_4, and N_2O fluxes from an afforested lowland raised peatbog in Scotland: implications for drainage and restoration" *by* S. Yamulki et al.

Anonymous Referee #1

Received and published: 10 September 2012

This paper described measurements of soil GHG fluxes from drained and afforested peatland plots, and compares with neighbouring undrained and unplanted plots, as well as a nearby near-pristine site. The work is unusual, being a long-term experiment and well replicated. The paper is well-written and fairly concise, and addresses a pertinent topic.

The major weakness of the study is that the comparison does not include any direct measurement of uptake of CO2 by the plants, either tree or understorey, yet this is a major term in the budget. There is some discussion as to what this term might be, based on measurements at other peatland sites and forest yield tables, but this is fairly

speculative, and not clearly explained how this was combined with the measurements that were made. I don't follow how the direct measurements say that n-pris has a higer net GHG flux than DP, yet conclude it might have the half the GHG flux when Ps uptake is accounted for. Do the authors assume a steady state where efflux = influx? This would make little sense. Large CO2 effluxes probably correlate with influxes, but quantifying the imbalance brought about about by changing water table etc is necessary to answer the questions posed. To be harsh, I don't think we can actually draw any conclusions about the net effect of drainage or restoration on the GHG balance from this work - we still don't know if it is a good thing or a bad thing. This is reflected in section 5.4, which doesn't actually say what the implications are. So, the paper could focus on just CH4 + N2O, as this is a simple comparison, with the CO2 effluxes discussed but made very clear that their interpretation is by no means straightforward. Or, the details of the calculation by which the net GHG balance, including photosynthetic uptake, needs to be much more clearly explained, probably tabulated.

Other work has derived estimates of the effect of drainage/restoration expressed as kg CH4 m-2 y-1 per cm change in water table. Could this be calculated for comparison? How do the Fch4 rates compare with other UK studies on peat? Include some quantitative comparison with existing UK syntheses: Baird AJ, Holden J, Chapman P (2009) A Literature Review of Evidence on Emissions of Methane in Peatlands. Defra Project SP0574. pp Page, University of Leeds. Bussell J, Jones DL, Healey JR, Pullin AS (2010) How do draining and re-wetting affect carbon stores and greenhouse gas fluxes in peatland soils? CEE review 08-012 (SR49). Collaboration for Environmental Evidence: http:\\www.environmentalevidence.org/SR49.html. Collaboration for Environmental Evidence Systematic Review. Levy PE, Burden A, Cooper MDC et al. (2012) Methane emissions from soils: synthesis and analysis of a large UK data set. Global Change Biology, 18, 1657-1669. doi: 10.1111/j.1365-2486.2011.02616.x.

The statistical analysis is applied to the median of the replicates in a block because of some high values. However, this merits some discussion - are these high values real (explicable) or not? Was a threshold value used to exclude unbelievable values? This could be presented as a sensitivity analysis - how do results differ when the analysis is applied to the raw data, block medians or block means etc?

The static chamber method is rather error prone, especially when using only three time points. A linear increase was assumed, but particularly in the case of CO2, the response is often nonlinear. Can some more evidence of quality control be providwed? eg. what were the r2 on the regressions, do nonlinear fits change the results? There are many papers on the topic and the appropriate analyses should be done. See for example: Kroon PS, Hensen A, Bulk WCM, Jongejan PaC, Vermeulen AT (2008) The importance of reducing the systematic error due to non-linearity in N2O flux measurements by static chambers. Nutrient Cycling in Agroecosystems, 82, 175-186. doi: 10.1007/s10705-008-9179-x. Pedersen AR, Petersen SO, Schelde K (2010) A comprehensive approach to soil-atmosphere trace-gas flux estimation with static chambers. European Journal of Soil Science, 61, 888-902.

GWP applies to the mass of an emitted gas (the radiative forcing relative to CO2 over some time span), not to a site. This should be renamed net GHG flux (kg CO2-eq m-2 y-1).

The recent Fluxnet CH4 workshop agreed that units of nmol CH4 m-2 s-1 should be the standard unit, as it conforms to SI and is unambiguous. Neither tonnes nor hectares are SI units. Fluxes are expressed here as per day and per year, yet the integration from the measurements up to this level is not described.

Section 3.3: "temperature/treatment interaction" may be confusing to non-statisticians - probably better called "treatment-specific temperature coeficients".

Does Fig 5 show raw data or block medians?

Fig 6 would be better as x-y scatter of Fch4 vs WT, with different symbols for treatments, normalised to T=10.

Fig 7 y-axis units are missing from the axis label.

Interactive comment on Biogeosciences Discuss., 9, 7313, 2012.