Biogeosciences Discussions, 2, S484–S486, 2005
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European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Profiles of C- and N-trace gas production in N-saturated forest soils" *by* K. Butterbach-Bahl et al.

Anonymous Referee #2

Received and published: 31 August 2005

General Comments

This paper aims primarily at the characterization of "the vertical distribution of C and N trace gas production and consumption" in forest soils in aerobic and anaerobic conditions. The data shown are certainly valuable and deserve to be published, but I am not convinced that the paper in its present form actually provides separate estimates of production and consumption. Rather, in the "aerobic" incubation conditions, a net flux resulting from the balance of both processes is measured, while in "anaerobic" conditions no information is given on consumption rates It may be misleading to write that production in anaeorbic conditions is two orders of magnitude greater than in aerobic conditions, without also writing that in natural anaerobic conditions (water-logged soils) the produced N2O or NO cannot escape to the atmosphere and is partially or totally consumed in situ. Further, one might question whether the soil may truly be considered

“anaerobic” and it may be preferable to use the terms “partially anaerobic” or “reduced O2”.

Specific Comments

Previous studies have attempted to quantify soil gas production and consumption using “compensation point” approaches, subjecting soil cores or samples to a range of gas concentrations. In such studies, the net flux F = P - k*C, where P is production, k the consumption rate and C the concentration in the headspace. On a scatter plot of the measured exchange fluxes versus the corresponding gas concentrations, one may estimate P as the zero intercept of the regression and the consumption rate k as the slope. In the present paper, both “aerobic” and “anaerobic” datasets are representative for only one gas concentration level each, namely ambient and 0, respectively, when in fact at least two datapoints are required in each situation to draw a regression. In both cases, several mixtures of O2/N2 and non-zero N2O, NO and CH4 concentrations should have been used in addition, simulating what happens in natural soils when produced gas accumulates and/or is consumed. In the “aerobic” case of this study, since production and consumption cannot be distinguished, the authors should not use the word “Production”. The words “Net exchange” might be more appropriate to describe what was actually measured. In the “anaerobic” case, the measured quantity can probably to a good enough approximation be assimilated to the absolute production term, although some of the produced gas may already have been consumed straight away and not reached the headspace. Be as it may, the “anaerobic” incubation fails to describe the existence and magnitude of the consumption processes, which is an equally important parameter for modelling purposes, and this point must be made clear in the discussion.

The authors report that soil water contents of soil samples were highest for forest floor($>$100%w/w) and decreased with depth in the mineral soil. It is not clear whether the samples were first humidified/dried to a common level of soil water content, or whether they were incubated without pre-treatment. In the latter case, what are the im-

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plications for the interpretation of results and the comparison between depths? In other cases or at other sites, it could well happen that the mineral soil is wetter than the forest floor. Likewise, it is not said if there were large differences in soil moisture content between sites and sampling dates, while this is a crucial parameter that determines which of nitrification/denitrification prevails.

To address the question of complete or partial anaerobia the authors might for example show that CO2 fluxes were negligible in the “anaerobic” incubations. If on the other hand CO2 was produced, then O2 must have been present still in soil microsites. The CO2 data would in any case represent a useful addition to this paper as an important indicator of microbial activity.

Technical comments

Did the authors observe cases when the gas concentration increase in the headspace was not linear? This does not seem unlikely since incubation could last several hours. If so, how did they calculate fluxes from non-linear time series?

Legend is missing in Figure 1

Interactive comment on Biogeosciences Discussions, 2, 1127, 2005.