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## ***Interactive comment on “The effect of a reciprocal peat transplant between two contrasting Central European sites on C cycling” by M. Novak et al.***

### **Anonymous Referee #1**

Received and published: 5 November 2009

Review of Manuscript bg-2009-182 by Martin Novak et al.: The effect of a reciprocal peat transplant between two contrasting central European sites on C cycling

This manuscript describes a well performed transplant experiment of peat cores from contrasting sphagnum bog sites and reveals a very interesting result: The transplanted peat cores adjusted in their characteristics to the host site during an 18 months in-situ incubation. Furthermore, the study clearly showed that the site receiving higher sulfur deposition had higher methane production despite partly contrary literature data available. Even more interestingly, a peat core from the low pollution site transplanted to the polluted site showed increasing methane production. These findings merit rapid publication and will be interesting for a number of (not only) peatland scientists. The methods are well described and good to understand, the experimental design is statis-

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tically validated.

However, there is one general problem I have with this study. From my point of view, available studies are not well incorporated in the introduction and discussion of this work. There is much more articles available dealing with transplant experiments and especially interpreting isotope profiles in peat. E.g. in the introduction in hypothesis no. 1, the authors mention isotope budgeting in soils and a selective removal of light isotopes through methane emission. Within this concept, I miss adequate references. This phenomenon/concept has already been described for lake sediments (e.g. Gu et al. 2004; extreme C-13 enrichment in a shallow hypereutrophic lake: Implications for carbon cycling; L+O 49, 1152-1159) and for peatlands (e.g. a very early study of Lansdown et al. 1992: CH<sub>4</sub> production via CO<sub>2</sub> reduction in a temperate bog: A source of <sup>13</sup>C depleted CH<sub>4</sub>, GCA 56, 3493-3503; a very recent study Knorr et al. 2008: Fluxes and <sup>13</sup>C isotopic composition of dissolved carbon and pathways of methanogenesis in a fen soil exposed to experimental drought. BG5, 1457 -1473; Clymo 2008: Diffusion and mass flow of dissolved carbon dioxide, methane and dissolved organic carbon in a 7 m deep raised peat bog. GCA 72, 2048-2066; and multiple studies from Hornibrook et al.). The results of these other studies would especially be interesting within this respect, as the authors did not measure d<sup>13</sup>C of CO<sub>2</sub> and CH<sub>4</sub> within the soil. Within this issue, it may also be good to introduce the different methanogenic pathways early in the manuscript before the data is being discussed, as they yield considerably different isotopic signatures of the methane produced. Concerning the delta values of the emitted CO<sub>2</sub> and CH<sub>4</sub>, it may also be helpful to cite more work on the isotopic composition of these gases, as the differences observed are relatively small when compared to variability reported in other studies. The reader may not always know about reported ranges of d<sup>13</sup>C in bulk peat and CH<sub>4</sub> and CO<sub>2</sub>. Differences in the bulk isotopic composition of the peats under study here should also be compared to more data, also from other authors and sites if the authors want to interpret and discuss the depth profiles.

This brings me to a second point. As the title points out the transplant experiment, I

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find the discussion of this aspect rather short. Introducing more available literature on the named issues may improve this part.

A third general point is a general statement to the references of this study. The article has 26 references, of which 8 share the same first author, and another study where the first author of this work is at the “senior author” position. Contrarily, as stated above, I think that there are quite some relevant references missing. I would like the authors to critically go through the references, leave out maybe 1 or 2 of the articles from the own group, but include work from other groups and sites. This work is certainly relevant for a lot of sites all over the peatland regions of the world and thus more work from such sites needs to be included.

I recommend a thorough revision of the manuscript to include more references and to link the data better to existing data. Nevertheless, the study is, undoubtful, very well done and interesting for a broad community. The study ideally matches the scope of Biogeosciences and should be published soon.

Specific comments:

Abstract

Page 10008, L12-13: You sometimes mix up methane production and methane emission in this paper. This needs clear separation and discussion

Page 10008, L20: The Pb 210 dating used in this study is recently being discussed. This is not mentioned in this study. Due to the increasing discussion of altered Pb profiles in peat, I would recommend to introduce some comment on this issue.

Page 10009, L11ff: There is a quite important study on global CH<sub>4</sub> sources by Mikaloff Fletcher et al. 2004: CH<sub>4</sub> sources estimated from atmospheric observations of CH<sub>4</sub> and its C<sub>13</sub>/C<sub>12</sub> isotopic ratios, GBC, 18. GB 4004

Page 10009, L 19ff: As stated in the general comments, there is more literature available on this isotope mass balancing. Please include this here and in the discussion.

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Page 10009/10010: You three objectives are only loosely connected. I think you could make more out of this. The isotope mass balance idea is good but needs to be linked to the transplant study. You have shifts in the isotopes when looking at the different treatments, no? The transplant experiment is also an interesting tool to study the impact of sulfur deposition history versus other site specific environmental factors and litter chemistry on methanogenesis, no?

## Materials and Methods

This section is really very well done!

## Results

Page 10014, L 22ff: "...from CB to VVJ...", even though their home site CB had similar C concentrations along the entire profile. The CB to VJJ carbon concentration profiles were indistinguishable ..., i.e. from their host site." I got lost in this construction. Please rewrite.

Page 10015, L15-16: "CB to CB had the lowest gas production rates, VVJ to VVJ had the highest gas production rates": This is an interesting side aspect that probably merits some discussion later on. Maybe the disturbance effect of coring had a different impact on the two peats? If so, why?

Page 10015, L19-22: "A striking feature of every graph in Fig. 6 is that all four..." This is indeed a very surprising result. Nevertheless, I would recommend to avoid "striking feature". And: There is a lot of studies available dealing with the C-sources for respiration and methanogenesis in peats. It is generally described, that the C used for methanogenesis and other respiratory pathways is younger than the surrounding soil and that methanogenic activity is very much related to photosynthetic activity. Could it be possible that DOC entering your cores from aside and freshly formed plant exsudates were responsible for this convergence? I recommend adding a short comment and references regarding this issue in the discussion (e.g. Chasar et al. 2000: Ra-

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diocarbon and stable carbon isotopic evidence for transport and transformation of dissolved organic carbon, dissolved inorganic carbon and CH<sub>4</sub> in a northern Minnesota peatland; GBC 14, 1095-1108; Crow and Wieder 2005: Sources of CO<sub>2</sub> emission from a northern peatland: Root respiration, exudation, and decomposition; Ecology 86, 1825-1834; Fenner et al. 2004: Peatland carbon afflux partitioning reveals that sphagnum photosynthetate contributes to the DOC pool. Plant and Soil 259, 345-354; just to name a few).

Page 10016, L5-6: “The peat cores producing more CO<sub>2</sub> and methane at their home location (i.e. VVJ) always had isotopically lighter carbon”: I find this an interesting point (see also Fig. 6) which is unfortunately not discussed. Why do you think is this? More methane production would me make to expect more loss of light isotopes in the respective cores. But neither the CO<sub>2</sub> nor the bulk phase shows this. IOs this a question of organic matter quality? Or pathway/microbial community?

Discussion Page 10016, L20-22: “A number of records of recent peat slow...” must it be “show” instead of “slow”? There is something wrong in this sentence.

Page 10017, L 2-4: “Thus higher availability of nutrients at VVJ may have contributed...” You should give a reference for this idea. E.g. Aerts et al. 2003 did not find an impact of nutrient addition on litter decomposition rates (Ecology 84, 3198-3208)

Page 10017, L13 ff: There is more literature available on d13C profiles in peat. E.g. from the group of Hornibrook there are several profiles available.

Page 10017, L14 ff.: You need to introduce the different pathways of methanogenesis and their respective d13C ranges here, probably also the effect of methanotrophy should be introduced here as well. Here again, as stated in the general comments: There is more literature available on this residual isotope enrichment issue.

Page 10018, L1: How much are these changes in CO<sub>2</sub> isotopic signature compared

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to observed ranges in soil CH<sub>4</sub> and CO<sub>2</sub> (being at least partly the precursor for CH<sub>4</sub>)? This should be added here for the readers that are not so familiar with the observed ranges.

Page 10018, L 10-11: There is also literature available on the d13C of plants under different climatic conditions. At least one reference should be given here to support this idea.

Page 10018, L16-18: This increase of d13C downcore, reflecting substrate degradation: Please make the link here again to methanogenesis (removal of light isotopes) if you mean this here. This would be easier to read and strengthen this point.

Page 10018, L26: As you observe zero methane emission at some points of time from some of your cores and there is certainly not zero production, you should write “Net methane production. . .” instead of “Methane production..” only.

Page 10019, L1: “. . .that some explanations are ruled out” This statement is followed by only one possible explanation as far as I see (vascular plants).

Page 10019, L10-21: I find this discussion of the transplant on the emissions a bit too short. What about these “environmental parameters” that control the C mineralization? What do you suggest? You could dwell a bit on this (DOC, young carbon from plants, soil solution chemistry (I suppose the exchange of soil solution was not hindered in this experimental design).

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