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Biogeosciences Discussions

Interactive comment on "Effects of flooding cycles in the Pantanal on the turnover of soil nitrogen pools and emission of N_2O" *by* L. Liengaard et al.

Anonymous Referee #2

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General comments

This paper by Liengaard et al. presents data of soil N2O and CO2 fluxes in the Pantanal, South America's largest wetland, together with soil ammonium and nitrate, O2 and pH values, as well as soil temperature and soil moisture data from a weather station, which did not cover the whole study period, though. Additionally, laboratory incubation experiments with soil slurry for the estimation of N2O formation potential were conducted. Three field campaigns of different duration were performed at different times of the year in 2008, 2009 and 2010, covering approx. seven, three-and-a-half and two weeks, respectively. At three different sites, soil gas measurements and sampling was done along transects of 10-25 m length, at three different height levels, i.e. the lowest level with the longest inundation period, the highest level with the shortest

inundation period, and an intermediate level. In 2010, ten sites were sampled once each, which was called “screening” by the authors.

In general, the study reports very interesting data, especially very high N2O emission rates, from a poorly studied, but ecologically and biogeochemically very significant part of the world, i.e. the tropical wetlands. Some parameters that are relevant for N2O emissions, such as inorganic nitrogen content of the soil, pH and O2 content, have been determined. However, this study suffers from several weaknesses. First of all, “long-term” measurement (if you want to call three to seven weeks long-term) have been conducted only at two sites each in 2008 and 2009, one of which (site A) was the only one that was sampled in both years, and the second site (site B in 2008 and site C in 2009) were only sampled within that one year. Moreover, site C was completely water-logged during the whole field campaign 2009, therefore the conditions there were not comparable to the other two sites and the other year. The “screening” in 2010 was only a short-term campaign. The other obstacle is that not all parameters were determined at all sampling locations: soil pore water pH, and ammonium, nitrate and phosphate concentrations were determined at all sites at all three levels; total soil C and N content were determined at site A and B for all three levels, but only in 2008; the O2 concentration profile was measured in 2009 at site A and C, but only at level 2; N2O and CO2 fluxes were quantified at sites A, B and C, but only at levels 1 and 2 etc. No flux data for inundated areas are presented, although they represent are major area of the Pantanal during the flooding period. Especially in view of the fact that the authors try to come up with an N2O flux estimate for the whole Pantanal, this is a major drawback. The same is true for the fact that the authors did not measure N2O (and CO2) fluxes at level 3, which might also represent a significant part of the total area of the Pantanal.

With respect to methodology the significance of the soil slurry incubation experiments was not clear, as the conditions applied do not correspond to natural conditions, namely the complete destruction of the soil structure and hence the total alteration of nutrient

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availability and diffusivities of dissolved substances and gases. Therefore, the explanatory power of these experiments can be questioned. The mere fact that N2O is first formed under anaerobic conditions and then further reduced to N2 with depleting nitrate pools is not new. Also the estimation of NO and N2 losses is founded on a very weak basis.

In general the paper is quite well written, although especially the Materials and Methods section is sometimes imprecise. The number of figures should be perhaps reduced.

Although the general information, i.e. repeatedly very high N2O emissions from a natural, unfertilized wetland, which are as high as the highest N2O emission rates from fertilized agricultural land, is very interesting and highly valuable, I have doubts whether this paper reaches the high standards a scientific journal like Biogeosciences should try to maintain. This is, inter alia, due to the poor temporal coverage of measurements, the lack of sufficient replication (only two sites in 2008 and 2009 with one transect each, and ten "screening" sites in 2010), that was aggravated by the fact that the measurement in the different years were performed in different seasons (with falling and rising water level, respectively), and the fact that there was no clear concept visible in determining the different parameters, i.e. several parameters were not determined in all years or at all three sites or at all three locations of the transects. If the authors could come up with more data for more sites and/or more locations at each site, I could recommend publication of this paper.

For specific comments see below.

Specific comments

p. 5994, l. 1-3: The hole-in-the-pipe model also refers to nitrification as one of the two sources of nitrous oxide.

p. 5995, l. 23: It would be good to learn about the period of each field campaign here to be able to put the calculation of cumulative fluxes and other generalizations into the

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right perspective.

p. 5995, 26-28: Unfortunately only one replicate was sampled in the years 2008 and 2009. Given the vast extension of the Pantanal, this appears to be too little replication for a representative description of the study region.

p. 5996, l. 1-2: What is the difference between “field campaign” (2008 and 2009) and “screening” (2010). Was it only the difference in length of the measurement periods, or were different parameters measured?

p. 5996, l. 2-4: This is the major drawback of this study: The data from the sampling in 2008 and 2009 are not comparable to the 2010 data, as in 2008/2009 sampling was done while the water was retreating, i.e. after a longer period of inundation, when soil nitrate pools are known to be exhausted due to continued denitrification activity, whereas in 2010 screening was done with rising water, i.e. when pulse emissions of NO and N2O are to be expected after N accumulation during the drained phase.

p. 5996, l. 18: It is not clear which part of the transects the location of the weather station is representative for, especially with respect to soil temperature and soil moisture. Is it more the upper, the middle or the lower part?

p. 5997, l. 5: At which sites and down to which depth were the soil cores sampled?

p. 5997, l. 13-15: At which sites was soil pore water sampled? Furthermore, it is not fully clear to me how the Rhizon filters were inserted: With the soil core left in its place in the soil just with digging away the surrounding soil? Taking out the soil core and inserting the filters? Placing the soil core back to its original location and then sampling the pore water? Or leaving the soil core out of the soil during pore water sampling?

p. 5998, l. 3-4: How did you calculate the average? Just the arithmetic mean? Or did you use a weighted average, i.e. attributing a certain part of the soil core to each of the three values?

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p. 5999, l. 2f.: When were the soil cores sampled? Which diameter and depth? Moreover, the significance of these experiments is not clear to me. According to my understanding, converting soil samples to slurry does not reflect the natural conditions of inundation, where the soil stratification stays more or less intact, at least at the low water flow velocities in the Pantanal. This has implications for nutrient availability as well as diffusion coefficients for nutrients and oxygen. The authors should comment on this.

p. 5999, l. 17-18: Why were N2O and CO2 fluxes not measured at Level 3? Depending on the areal representativeness of each level chosen in this field study (highest, intermediate, lowest soil moisture) for the whole Pantanal you could have missed an important part of the N2O source, or the areal estimate could have been also much lower due to lower N2O emissions at Level 3.

p. 6002, l. 3-4: You do not mention any phosphate data for sites A, B and C for the years 2008 and 2009 in the text.

p. 6002, l. 6f.: It is unfortunate that there are no data for total C and N neither for site C nor for the ten screening sites in 2010.

p. 6003, l. 18-25: The relevance of the CO2 flux data for this paper is not clear to me, as there is neither a correlation analysis with soil parameters nor a comparison with N2O fluxes.

p. 6004, l. 2: "...from drained soil...": Does this mean that data from site C were excluded, as this site was permanently water-logged during the field campaign in 2009?

p. 6004, l. 5-7: At least for site A you should have some data, as the weather station was located there, recording soil temperature and soil moisture data.

p. 6004, l. 9f.: "...detailed study of the nitrogen cycle...": For a detailed study of the nitrogen cycle I would expect at least some numbers on (at the input side) nitrogen fixation, DIN, DON and particulate organic nitrogen import with flood water, and (on the

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output side) NO and N2 losses, as well as nitrate and DON losses with the retreating water, furthermore data on plant and microbial N uptake and release. As only data on soil ammonium, nitrate and total nitrogen as well as N2O losses are available I would refrain from talking of a “detailed study of the nitrogen cycle”.

p. 6005, l. 3: What are the long-term monitoring sites? There is only one site that was monitored within two years (both 2008 and 2009, site A), the other two sites (B and C) were only sampled in one year (2008 and 2009, respectively). I would not speak of “long-term monitoring sites”.

p. 6006, l. 14f.: Again, how comparable are these slurry incubation studies with natural conditions?

p. 6006, l. 15: How do you know that denitrification was constant¿

p. 6006, l. 17: Did you measure nitrate and nitrite concentrations in the slurries?

p. 6007, l. 23f.: Here a critical discussion of the phosphorus status of the studied soils is missing. A P deficiency in Pantanal soil together with high natural N fixation rates could explain the observed very high N2O emission rates.

p. 6009, l. 5-7: How did you calculate the magnitude of N2O pulses from rain-wetted soils? Did you use one default value, which lasted for one day?

p. 6009, l. 23-25: For this extrapolation you would need a good estimate of the spatial and temporal distribution of drained and wetted soils.

p. 6010, l. 1-5: This is an assumption on weak grounds, totally neglecting any other dynamics like e.g. plant N uptake, thereby fostering plant–microbe competition for nitrogen.

p. 6010, l. 18-20: Do you have any estimate of the contribution of inundated soil to total N2O emissions? Given the vast flooded area of the Pantanal this could be large. Or have you assumed that there is no N2O emission from flooded soil, but only total

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reduction to N2? Then I would say this should have been proven.

p. 6011, l. 16-28: Again, what is the contribution of the period of inundation? Do you think there is no nitrogen loss during that time? Any estimate of a nitrogen balance has to include this period.

p. 6012, l. 10-11: These plants will for sure not take up all of their nitrogen directly from N-fixing microorganisms, but only a smaller, albeit significant part. Thus, the large difference in estimated annual N fixation and total N content of the plant material is not surprising.

Technical corrections

p. 6006, l. 7: Replace “gleisoil’s” with “gley soils”

p. 6007, l. 5: omit “be” between “can” and “promote”

Interactive comment on Biogeosciences Discuss., 8, 5991, 2011.