Biogeosciences Discuss., 9, C3282–C3292, 2012
www.biogeosciences-discuss.net/9/C3282/2012/

Biogeosciences Discussions

Interactive comment on "Nitrate leaching from short-hydroperiod floodplain soils" *by* B. Huber et al.

B. Huber et al.

elisabeth.pannatier@wsl.ch

Received and published: 16 August 2012

Many thanks for this thorough review of the manuscript.

Referee 2: The manuscript considers several closely interrelated variables related to soil hydrology - soil texture, water holding capacity, volumetric soil moisture - that only address one part of the N cycle. Distinguishing between causal factors and surrogates for those factors is difficult and, at a minimum, the statistical analysis doesn't convince me that it has done so.

Authors: We didn't do any statistical tests between nitrate concentration and soil texture because soil texture was only available for the top 10 cm. Despites this, we used soil texture several times as one of the main driving factor explaining the nitrate con-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

centration. We did this because it is well established that the FC is driven by soil texture. However, to avoid confusion and to focus on our findings we removed the term "soil texture" when explaining the driving factors and used FC only. We remind that field capacity (crosses in Figure 4) were determined with the measurements of the matric potential and soil moisture from the relevant depth (10, 50 and 100 cm). We agree, when speaking about the field capacity, the term "volumetric water content" at FC might be confusing and not needed. In the method section it is already clearly stated that the FC is expressed in terms of VWC (see page 5667 line 11-12).

Therefore, we suggest to change the following sentences accordingly:

page 5660, line 15-16: ...main factor explaining the differences in nitrate concentrations was the field capacity (FC). in subsoils with high FCs and VWC near 100% FC, ...

page 5673, line 12-13: ...nitrate concentrations in our soil solutions were explained by the FC ($p <= 0.03$),...

page 5673, line 16: ...could be related to differences in FC (Figs. 4 and 5).

page 5675, line 12-14: This suggests that the FC and the depth of groundwater might be more important factors than the succession stage...

page 5678, line 7-8: In soils with high FCs, low C/N ratios and VWCs nearby 100% FC,...

page 5679, line 19: ...variability of FC, which is...

Referee 2: The authors state that they log-transformed non-normally distributed data, but don't state which variables, or mention any formal tests for normality of the log-transformed data.

Authors: We could change it in a revised manuscript: (see page 5668 line 14-15): "The nitrate data were log-transformed to improve the data distribution".

Comment: We used histograms and Q-Q plots to visualize the data distribution. Only

BGD

9, C3282–C3292, 2012

nitrate data were not normally distributed and were therefore log transformed. It was clearly visible from the histogram and Q-Q plot that the log transformation made the nitrate data distribution closer to normal distribution.

Referee 2:It is impossible from the graphs presented to get an idea of the extent to which non-normality, or highly-leveraged outliers could be influencing the results.

Authors: The log-transformation was only done for nitrate. As it can be seen in Fig 4., some plots (e.g. G2, WB1 and MF1 100 cm) were highly skewed. Data points that were greater than $\pm$3SD were considered as outliers and not shown in the figure.

Referee 2: A correlation between soil solution nitrate and volumetric water content is promoted as an important finding. There is no way to assess the importance of this relationship that just barely satisfied (p=0.03) the criterion for statistical significance despite a fairly large number of data points.

Authors: As stated on in the result section (page 5670, line 22-23) we clearly stated that nitrate concentrations were not significantly correlated to VWC. However, we agree that in the discussion section (page 5673, line 12-13) this sentence can lead to misunderstanding. Therefore, we can change the sentence to . . .Despite these multiple factors, the nitrate concentrations in our soil solutions were explained by FCs ($p \leq 0.03$),. . .

Referee 2: The data would be better served by simple least squares regression than the maximum likelihood analysis used.

Authors: We have also tried the least square regression. However, it does not change our findings, i.e. nitrate concentrations and VWC are significantly correlated to FC, whereas nitrate concentrations were not significantly correlated to VWC. If asked, we can replace the statistical method section with least square regression in a revised manuscript.

Referee 2: Another problem lies with measurement/estmation of soil properties. Impor-

BGD

9, C3282–C3292, 2012

tant soil hydraulic properties appear to be extrapolated to 0-50 and 0-100 cm depths from measurements made over the 0-10 cm depth. This can be a substantial source of error in young alluvial soils, which commonly form thin, fine textured caps over coarse bed sediments. Although soil depths were reported, it is unclear how soil depth was defined or measured.

Authors: We agree that the soil sampling is not fully described in the manuscript. We wanted to avoid long and cumbersome explanations for the reader. However, we do know that the soil properties are homogeneous with depth at our study site.

First, the so-called "soil" compartment in our manuscript corresponds to the fine textured material overlying the coarse gravel. The soil depth (limit between fine textured material and coarse gravel) given in Table 1 was determined by visual inspection of the drilling cores collected during the installation of 29 piezometers from the gravel bar to the alluvial forest (transect B in Schneider et al. 2011). This information can be added in a revised manuscript.

The soil properties were determined in two steps. In a first step, two 1m long soil cores were taken with a drill corer from two or three plots of each FPZ (GRASS, WILLOW BUSH and FOREST zones) at the study site. The soil cores were only 40 cm or 80 cm long in the GRASS zone because of the smaller soil thickness (Table 1). The presence of coarse gravel prevented the use of the equipment. Each core was split into 20 cm long segments where a number of parameters (soil texture, total organic carbon and total nitrogen) were measured. The results are published in the paper of Samaritani et al. (2011). The authors explained that the soil texture, TOC and TN contents did not vary much with depth in any of the FPZs. A principal component analysis demonstrated a generally larger lateral than vertical variation of the soil properties.

In a second step, we collected topsoil cores (0-10 cm) at the 8 plots described in the manuscript, close to the lysimeters used to collect soil solution. Based on the results of Samaritani et al. (2011), we assumed that the soil properties did not vary much with

BGD

9, C3282–C3292, 2012

depth. We deliberately did not collect 1 m long cores close to our instruments to avoid any disturbance.

In a revised manuscript, the soil sampling conducted in the study of Samaritani et al. (2011) and the main findings could be briefly described in the method section.

Referee 2: Finally, it is unclear whether soil bulk density was measured. Soil chemical properties are reported in g/kg where areal measurements would be more appropriate, and these depend critically on bulk density, which can decrease by 50% or more as young soils undergo dilation.

Authors: We did not observe any dilation of the soils. The bulk density was measured at two depths (5-15 cm and 45-55 cm) in each FPZ with metallic cylinders (1000 cm3). The bulk density slightly increases with depth (from 1.1 to 1.3 g/cm3).

Referee 2: One of the main conclusions is that nitrifications rates, limited by soil moisture, was a rate limiting step on nitrate accumulation and leaching. I think this inference goes well beyond the data presented.

Authors: We did not formulate this conclusion in such a direct way, because we did not measure net and gross nitrification rates in this study. As you suggested, we could combine the Implications and Conclusions sections in a revised manuscript and focus on the results rather than on inferences.

In the discussion, we will refer to the study of Shrestha et al. (Soil nitrogen dynamics in a restored and channelized floodplain section, submitted to Journal of Environmental Quality). They measured gross N transformation rates (mineralization and nitrification) and potential rates (nitrification and denitrification) in different FPZs at the same study site.

Referee 2: The Discussion section is overlong and should be condensed some. The Implications and Conclusions sections should be combined into a concise statement of Conclusions.

BGD

9, C3282–C3292, 2012

Authors: Actually the discussion was extended in this manuscript, because you requested more information on the errors related to the NO3 fluxes. We suggest to condense the next paragraphs in the discussion:

From page 5673 (line 9) to page 5674 (line 20): "The nitrate concentrations measured in the soil solutions are the net result from nitrate-producing and consuming processes (e.g. nitrification, denitrification, microbial immobilization, plant uptake) and from mixing with groundwater and river water." Despite these multiple factors, the nitrate concentrations in our soil solutions were explained by FC ($p \leq 0.03$), underpinning the importance of soil properties on soil nitrogen processes. For example, in GRASS the replicate plots G1 and G2 showed large differences in nitrate concentrations, which clearly could be related to differences in FC (Fig. 4 and Fig. 5). In a parallel study, Shrestha et al. (2012) showed that the potential for nitrification was high in the three FPZs but that the activity of nitrifiers could be limited by low soil moisture. Lower nitrification rates could therefore explain the lower nitrate concentrations in the sandy soils like in G2 (Table 1). In willow bush (WB1 and WB2), nitrate concentrations were very low and even lower than those at G2 (Fig. 4). We assume that the high demand of N by the still young (7 years) and rapidly growing Salix viminalis (Rytter, 2001) led to either high nitrate uptake, or competition for ammonium between the plants and microbes, leading to limited substrate availability for the nitrification process. Considering the preference of ammonium over nitrate by these trees (Burger Chakraborty and Sägesser, 2010), the latter is the more likely reason. Similarly, we conclude that the increasing nitrate concentration with depth in MIXED FOREST can be attributed to a maximum plant uptake in the topsoil. Flooding affects both nitrate concentration and soil moisture significantly (Fig. 5) and it is not completely clear whether mixing with river water and/or denitrification is the main driver for the observed decline in nitrate concentration during inundation. However, due to the short flood durations, the longest in GRASS lasting between 1 and 3 day at G1, we assume that this inundation time is often too short to reach redox potentials sufficiently low to favor denitrification. This assumption is also supported by the study of Pinay et al., (2000) where the flood

BGD

9, C3282–C3292, 2012

frequency was too short to observe any significant influence by soil denitrification."

Page 5676: remove lines 3 to 9 (comparison with NO3 fluxes in European forests) Page 5678-5680: remove section 4.5 Implications for river restorations and integrate some parts in the conclusion.

Specific comments:

Referee 2: Throughout the typeset manuscript, _mol L-1 is written as _moll-1. Is that the journal style? While capitalizing the L might not be mandatory, joining the "l" to "mol" seems to create a new unit.

Authors: We will use everywhere mol L-1.

Referee 2: 5661 and throughout mss. The emphasis on nitrification is misguided. Large nitrate pools are expected in soils with high mineralization rates relative to biological demand, and nitrification was not directly measured.

Authors: We do not agree that we put too much emphasis on nitrification. The objectives of the paper are clearly stated in the introduction (page 5662, lines 5-8) and do not mention any measurement of nitrification rates. Nitrogen transformation rates (gross mineralisation, gross nitrification and potential nitrification) were measured in another study (Shrestha et al. submitted to Journal of Environmental Quality). We suggest to discuss more our results in the light of Shrestha's findings.

Referee 2: 5666, section 2.6. You need to say how moisture sensors were installed and might add what precautions were taken to avoid altering hydraulic properties.

Authors: In fine textured soils, the soil moisture sensors were installed from the soil surface to avoid disturbance of the surrounding experimental plot. In coarse soils, the sensors were installed horizontally from the edge of a soil profile at the desired depth. Care was taken not to disturb the native bulk density.

Referee 2: 5667. Lines 3-12. Explanation of how matric potential was used in the

BGD

9, C3282–C3292, 2012

study (and why results were not reported) is pretty convoluted. Needs to be stated more succinctly.

Authors: We suggest to summarize the matric potential measurements in the following way:

"The soil matric potential (Ψ) was measured with tensiometers built of ceramic cups (highflow porous ceramic cups, Soil moisture Equipment Cop. Santa Barbara, USA) between saturation and -900 hPa and with MPS1 Decagon sensors between -100 hPa to -5000 hPa for each plot and depth. The measurements of VWC and Ψ were used to validate the soil water retention curve used for the modeling of the water fluxes. Field capacity (FC) was defined as the VWC for Ψ between -100 hPa and -60 hPa based on tensiometer readings. "

Referee 2: 5667. Section 2.7. Am I right in understanding that soil hydraulic conductivity and VWCfor the 0-50 or 0-100 cm soil depths were extrapolated from calcluations applied to soil samples collected from 0-10 cm depth? This could introduce large errors since fining upwards patterns are the rule in alluvial soils, and tend to be especially pronounced in young, high energy environments.

Authors: No, we used the soil properties measured until a depth of 100 cm in the forest and 50 cm in the grass and willow bush zones. See response to the second comment.

Referee 2: 5667. Bulk density measurements for fresh sediments were mentioned in Methods, but not for soils. How was bulk density measured? Bulk density tends to decrease a lot during early soil development - C and N should be reported, and statistical comparisons, should be on areal basis (kg/ha/10 cm depth, not g/kg).

Authors: In page 5664 (lines 3-4), it is briefly described how bulk density was measured. We can add in a revised manuscript that it was measured at two depths (5-15 cm and 45-55 cm) in each FPZ with metallic cylinders (1000 cm3). The bulk density slightly increases with depth (from 1.1 to 1.3 g/cm3). C and N stocks are reported in

BGD

9, C3282–C3292, 2012

Samaritani et al. (2011). They reported the values in g kg-1. Since the manuscript is already quite long, we will refer to the paper of Samaritani et al. 2011 for further description of soil properties in the revised manuscript.

Referee 2: "Meteorological station" is better than "meteo-station". Need some sort of reference to the specific meteorological station used.

Authors: Yes, we agree. This can be corrected in a revised manuscript. The station is described in Pasquale et al. 2011. The citation will be moved directly after the meteorological station.

Referee 2: 5676, line 25 "between 1 and 11%" of what?

Authors: of nitrate in the aquifer

Referee 2: 5669, sec. 3.2. Soil thickness isn't even mentioned in the Methods sections. Deserves more than footnote in Table 1.

Authors: In a revised manuscript (Method section, soil sampling), we can add a small paragraph:

"The soil compartment corresponds to the fine textured material overlying the coarse gravel of the aquifer. The soil depth (limit between fine textured material and coarse gravel) given in Table 1 was determined by visual inspection of the drilling cores collected during the installation of 29 piezometers from the gravel bar to the alluvial forest (transect B in Schneider et al. 2011). "

Referee 2: 5669. line 10-14. Long, contorted sentence (concentrations of what?) needs to be straightened out.

Authors: We suggest to improve this sentence in a revised manuscript: "The mean chemical composition of the water samples (river, side channel, bulk deposition and soil solution) is presented in Table 2. Only the soil solution data from the lower soil compartment (i.e. at 50 cm of depth in the grass and willow bush zones and at 100cm

BGD

9, C3282–C3292, 2012

in the mixed forest) are shown."

Referee 2: 5669. line 24. Table 4 should come after Table 3.

Authors: This can be corrected in a revised manuscript.

Referee 2: 5673, lines 9-12. Awkward sentence.Rephrase.

Authors: We suggest to improve this sentence in a revised manuscript: "The nitrate concentrations measured in the soil solutions are the net result from nitrate-producing and consuming processes (e.g. nitrification, denitrification, microbial immobilization, plant uptake) and from mixing with groundwater and river water."

Referee 2: 5673, Section 4.2 You don't really have the data to talk in a detailed way about nitrification rates. I wouldn't expect nitrification rates to be the limiting step on standing nitrate pools or N leaching. If nitrification were a rate limiting step, you should be able to measure an inverse correlation with ammonium concentrations.

Authors: We suggest to condense the paragraphs from page 5673 (line 9) to page 5674 (line 20). We could also discuss briefly our results in the light of the findings of Shrestha et al. (2012, submitted to JEQ).

Referee 2: 5677, lines 18-27. Gilles Pinay relates denitrification rates to texture for a number of floodplains soils in one of his papers. Might be a useful reference here.

Authors: Thank you for this remark. Indeed Pinay et al. (Biogeochemsitry 50, 163-182, 2000) found a significant relationship between denitrification rates in floodplain soils and their texture at alluvial sites along a stretch of the Garonne River in France. However, in a later publication (Pinay et al. 2007, Freshwater Biology 52, 252-256), they found no relationship at the European scale. We prefer to skip the discussion about the role of texture for denitrification. It would make the discussion even longer.

Referee 2: The Conclusion section reads more like a summary. Suggest it be combined with Implications section into a concise statement of conclusions

BGD

9, C3282–C3292, 2012

Authors: This is a good suggestion.

Interactive comment on Biogeosciences Discuss., 9, 5659, 2012.