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Biogeosciences Discussions

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

Anonymous Referee #2

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This manuscript describes a study combining field nitrate measurements with modeled hydrological fluxes to construct a mass N balance in a river floodplain, adding to an already extensive literature on riparian N fluxes. While many studies have documented the capacity of riparian zones to serve as sinks for excess nitrogen from agricultural lands, this study found more complex patterns of N retention in the youngest soils while more mature floodplain soils were net N sources to rivers. The study is also useful in extending consideration to short hydroperiod riparian soils, which have been relatively overlooked in the literature.

The manuscript has been revised somewhat from the original submission. and several of my original concerns have been addressed. However, there are still issues that need to be addressed before it is suitable for publication

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

Another problem lies with measurement/estmation of soil properties. Important 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. 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.

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.

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

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Specific comments:

Throughout the typeset manuscript, μmol L-1 is written as μmoll-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.

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.

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.

5667. Lines 3-12. Explanation of how matric potential was used in the study (and why results were not reported) is pretty convoluted. Needs to be stated more succinctly.

5667. Section 2.7. Am I right in understanding that soil hydraulic conductivity and VWC for 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.

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).

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

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

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

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

5669. line 24. Table 4 should come after Table 3.

5673, lines 9-12. Awkward sentence.Rephrase.

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.

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.

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

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