

Interactive comment on “Riparian and in-stream controls on nutrient concentrations along a headwater forested stream” by S. Bernal et al.

Anonymous Referee #1

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This manuscript uses a large synoptic stream and riparian nutrient dataset to tease out relative contribution of riparian zone and in-stream biotic processes on downstream nutrient fluxes. Very little research has addressed this question, thus the authors describe a novel finding. Their approach is really clever and the authors have a great dataset to test these ideas. However I have a lot of questions and suggestions to guide a revision.

This paper was a bit tough for me to follow. Part of it is that the authors are putting together a large dataset to tell a big story; I commend them for this. But this approach means that they need some sort of road map or conceptual guide for the readers to follow through the manuscript. Ways to do this may include some sort of conceptual model (see e.g. the approach taken Payn et al.), a results section that better describes

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the findings and how the data support these findings. I note that the discussion was much clearer in reporting the results than was the results section. It may also be possible to leave out some parts that do not add to the story the authors want to tell. E.g. the hillslope data in the discussion were not well integrated in the rest of the story.

The authors used linear regression and GLM to examine change along the 15 sites of the reach. That probably works well to estimate if e.g. something doubled, but tests of significance will be hampered by the fact that these sites are not independent of each other. Processes at upstream sites determine those at downstream sites. In the case of nutrients it is likely the same molecules. Statistical test should consider this fact, and if they don't then the authors need to justify that point. I regret to not being an expert in spatial statistics, but something along the line of a generalized least squares regression may help. See, e.g., Ives and Zhu, *Ecol. Apps.* 16:2-32 for possible ideas on what, if anything, to do.

I am having trouble with the idea of only focusing net groundwater input when in fact there may be new groundwater entering a stream reach with concomitant losses. Thus net will be zero, but there could be a lot of new solute entering the stream via groundwater. See Payn et al *WRR VOL. 45, W11427*, doi:10.1029/2008WR007644

Specific comments

598-5 unclear what “which. . .” modifies

599-7 considerably important

25 See the now classic work of Lowrance et al. on riparian controls of watershed nutrient export

601-6 recast to downstream-most site. No comma after both.

602-3 two separate findings in one sentence

602-19 composed of

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602-22 wetted width

603-22 were used

604-4 If using the molybdate blue technique on filtered samples, then one analyzes soluble reactive P (SRP) of which phosphate is one component.

604-20. This method assumes no loss of water along the reach, i.e., the stream is solely gaining. Is this point true for this stream?

604-21. So divided by watershed area, correct? Unclear as written.

605-9. It seems like one could get a lot more information if the data were not averaged for each period but done separately using a multilevel approach.

606-4. But Table 3 reports a p-value from the ANOVA, which is not a goodness of fit (as I understand it). I am not sure why to report a P-value between the differences in these models anyway

606-10. Up to now all of the stats are based on parametric distribution. Why a switch to a non-parametric test here?

606-15. I usually think of a CI as a confidence interval. This common use of CI may be confusing when redefined as chemical index. Chemical index is by itself not very meaningful, so there may be a better phrase (and therefore acronym) to use.

608-2. What about segments that are gaining and losing at the same time, where the nutrient concentration in the groundwater inflow is different than the outflow, which we would assume is the same concentration as the streamwater.

608-18 SE estimated by averaging over what, time? Also, what is the frequency of Fsw not equal to 0 by chance using this technique? It seems to me that many would be not significant, but that would require a little explanation.

608-22. But there could be groundwater input in a losing segment.

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608-24. Putting the R code and data into an appendix will help readers to replicate this work in the future. This is a really valuable dataset.

609-3 Units for this area specific discharge simplify to units of length per time (say mm/d) which is common in hydrological literature and therefore less confusing for some to read.

609-15 The CI data and the Q data in this paragraph are not really well linked, i.e. I am not sure what collective finding they support.

609-20 What is the error in measuring Q via using dilution gaging? Given that there is always some measurement error, streams will always be gaining or losing when calculated as a strict difference. How much does a stream have to gain or lose to detect a difference above the measurement error?

610-23. I assume that these predictor variables do not covary among themselves?

610-27, ok so then why fit a straight line to the data?

612-9 Makes sense given the chemical index uses NH4 as part of its calculation

613-1 This paragraph is problematic in that it is repeating things from the introduction. Then it describes the significance of the findings before stating what the main findings are. I note that the results was a difficult place for me to understand the main findings—it was mostly a description of the data. In any case this paragraph can be safely deleted. Better, given the results section, would be to summarize the main findings.

614 8-12. This clear statement of the findings are restating results.

615-6 By saying .."where the N2 fixers are highest" implies that the N2 fixer may control NO3, but later in the paragraph we are reminded that there was no relationship with N2 fixers. I would recast to avoid creating confusion here and instead simply state the most plausible mechanism up front, and not one found to be not plausible.

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615-16 lowest part of the catchment

616-18 New results in the discussion?

617-20. Is that because NO₃ concentrations are higher than SRP and NH₄?

617-22. What is meant by “cycled more efficiently” Longer uptake length? Lower v_f ?

618-21 “if we are to understand”?

618-23. This sentence says that instream processing is important but manifests itself at a small spatial scale than riparian processes?

619-1. Ok a great way to end, but I could use bit more explanation here.

Table 1. Reporting an SE implies normally distributed data, yet there is a non-parametric test used. The equation for CI is not how it is described in the text.

Table 3. Likelihood is relative likelihood, correct?

Fig 2. The X-axis looks like it is plotted categorically vs. numerically as a function of distance. It seems to me that plotting numerically would be clearer because the sites are not equally spaced. Stream width and % sand should be on separate plots.

Fig. 5. A straight line seems to be a poorly fitting model for a U-shaped pattern of nitrate concentration and flux

Fig 7 legend, bottom “post-hoc”

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