Biogeosciences Discuss., 10, C1819–C1823, 2013
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10, C1819–C1823, 2013

Interactive Comment

Interactive comment on "Solute specific scaling of inorganic nitrogen and phosphorus uptake in streams" *by* R. O. Hall Jr. et al.

Anonymous Referee #1

Received and published: 13 May 2013

This paper is an interesting analysis of scaling of nutrient uptake in streams. The analysis is generally sound and the paper is nicely written and easy to follow. I think this paper is well suited to Biogeosciences and it has the potential to be quite important in helping move the field from the reach scale at which nutrient uptake studies are conducted to predictive, larger scale modelling efforts. I do see several areas where the analysis needs to be strengthened and reconsidered. Below I highlight a few 'major' issues followed by other miscellaneous comments.

Scaling relationships: You have decent relationships for NH4 and SRP (r2 = 0.57), but the relationship for NO3, while statistically significant, is extremely weak (r2 = 0.13). You mention there was more variation for it than for the others, but it pretty much gets swept under the rug in the discussion. I'm not convinced there is much of a

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relationship here considering there is such a poor correlation between Sw and Q/w. At a minimum you need to consider why the relationship was so weak for NO3 and what the implications are.

Reference vs. Altered systems: This is an interesting comparison, but there are several problems in this analysis. The r2 values are extremely low for nitrate and SRP-altered making it questionable as to whether there is even a relationship worth pursuing. In addition, comparing the regression of reference and altered P is misleading because you have a much larger range in Q/w for reference (∼0.02-10) vs. altered (∼0.5-10). If you are going to compare the two for SRP you should use only the overlapping ranges of Q/w (you should probably do the same for NH4 as well).

Uptake/metabolism: The analysis and integration of metabolism and uptake is not particularly compelling. In the introduction you hypothesized that N and P would be decoupled because P is subject to abiotic sorption and N is more tightly tied to metabolism. That seems fairly reasonable except for the tendency for NH4 to adsorb. In the results you state carbon demand drove inorganic N uptake. You can show that N uptake was correlated with metabolism, but you can't show causation. More importantly, while the correlation was statistically significant for nitrate, the r2 value (0.04) was low to the point of being a largely irrelevant relationship. Ammonium was better (r2=0.27), but still not great. In the discussion, the metabolism argument focuses on NH4 and ignores NO3. Based on your initial hypothesis NO3 should be best predicted by metabolism because it's the least likely to be influenced by abiotic sorption, so why ignore it? Is it because the relationship is poor? In the end, the metabolism issue is a rather muddled mix of weak analysis of the data and what appear to be pretty weak relationships. I wonder if part of the issue is that you're stacking uptake velocity (uptake efficiency) against metabolic rate (basically O2/carbon flux per unit area and time). Presumably the relationship would be better as uptake rate vs. metabolic rate as they are measuring the same thing – nutrient demand to support metabolic demand. A second issue may be the integration of ER which may partially result in nutrient demand (e.g. for bacterial/fungal growth) but is perhaps more coupled to nutrient mineralization rather than uptake. GPP should be more directly linked to nutrient uptake rate as they both build biomass.

Coherence in the introduction and discussion: To some extent the discussion didn't really follow well from the introduction. In the discussion you set up the isometric vs. allometric scaling models, but there is nothing about this in the introduction to set it up. I think the paper would read better if you set this up better in the introduction. Perhaps you could set this up better when you talk about Wollheim's constancy assumption.

Nutrient concentration: A sleeper in all this is nutrient concentration which you pretty much disregard. On p. 6676 l. 10 you note the assumption of constant biological demand (U? Vf?) relative to concentration is needed for isometric scaling. Was concentration related to specific discharge in your data? Did it have any role in what's going on? You note the issue in just one sentence in the discussion (p. 6682 l. 15) but that's it.

Miscellaneous comments: p. 6674 l. 21 – 'removing' is a term that gets (mis)used all the time especially in regards to uptake vs. net retention and/or true removal (e.g. the arguments over Cardinale's recent Nature paper). Would it be better so simply stick with uptake here and elsewhere since that's technically what your data are?

p. 6676 l. 2 and 6 – vf probably measures abiotic as well as biotic demand in most spiralling studies

p. 6676 l. 14 – how did you test for differences in slope and intercept? Perhaps a bit more detail.

p. 6676 l. 24 – note the range in Q/w here

p. 6676 l. 26 – start new paragraph for metabolism

p 6679 l. 11 – contrasting N and P uptake – You compared NH4 and NO3 independently to P to contrast N and P uptake. Did you try combining NH4 and NO3 to make this simpler? You could calculate Sw-N by combining Sw of NH4 and NO3 proportionally to their concentrations. Considering both forms of N can/will be used alongside P demand during uptake it makes sense to combine them. Also, you again ignore the r2 values on these which are really weak (Is the r2 = 0.082 for the top panel correct?). I think the real story here is that there simply is very little relationship between uptake of the two forms of N and uptake of P. That has been shown before.

p. 6679 last paragraph – I don't see how you are pinning the variation in m on variation in b. You included variation in several components of eqn 10, including error in the slopes (a) from your SMA's. In the discussion (p. 6683 l. 8) you argue variation in m was solely due to variation in hydraulic geometry. Doesn't it also include the variation in the slopes of you SMA's and therefore include uncertainty in the average value of m? Perhaps you can tease out how strong the effect of the variation in 'a' was in calculating m.

Discussion paragraph 2 – this analysis of the human influence seems jammed in here out of place

p. 6682 l. 1 – why wasn't biological demand related to nitrate (you have previously shown it is for some streams. . .).

p. 6682 l. 6 – differences in scaling relationships between SRP and DIN? Nitrate was the same as P (at least for the slope estimate, if not for the actual strength of the relationship) and you then note this a few sentences later. . .? It seems you need to do a better job separating out NH4 vs. NO3 in the discussion.

p. 6682 l. 20 – Here you spend a paragraph arguing that your data suggest large streams and rivers are important sites for nutrient uptake based on your scaling relationships. Two paragraphs later (p. 6684 l. 10) you explicitly note that larger systems are likely to be fundamentally different and that scaling relationships in smaller systems will not hold in larger systems. This needs to be better written to avoid the obvious contradictions.

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