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Interactive comment on "Comment on "Solute specific scaling of inorganic nitrogen and phosphorus uptake in streams" by Hall et al. (2013)" *by* R. González-Pinzón et al.

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Discussion of the comment on "Solute specific Scaling of inorganic nitrogen and phosphorus uptake in streams" by Hall et al. submitted by Gonzalez-Pinzon, Mortenson, and Van Horn.

I have several critiques of the comments made by Gonzalez et al in reference to the Hall et al. 2013 paper.

(1) I don't believe the assessment of spurious correlation presented by Gonzalez is correct in this presentation. Gonzalez-Pinzon and coauthors argue that relationships between SW and Q/w are colinear (Benson case 11 for spurious correlations) because

water velocity is in both denominators. In study of Hall et al., that is not the case. In the studies collated and presented in Hall et al., SW is effectively an empirical measurement that is independent (in the statistical sense) of any of the hydrological or chemical variables used to transform SW into metrics like uptake velocity (VF) and uptake rates (U). SW in these studies were estimated by plateau style additions of nutrients (many enrichments which have their own issues) using the longitudinal decline in added solute relative to a conservative tracer to estimate the per meter loss rate of nutrient (KL in m-1) using the equation: Fx = F0e-kLx KL is estimated by fitting this model to the empirical measurements. SW is derived simply by taking the inverse of KL. Velocity never enters the calculation as argued by Gonzalez et al. because the observed loss rate is not estimated in units of time as indicated by Gonzalez and therefore the spurious relationship argument collapses over the entirey of their critique.

Had Hall et al. or others used chambers or certain transport models (e.g. OTIS) to estimate a temporal nutrient loss rate (referred to as KC in the Gonzalez-Pinzon comment) and then used velocity to estimate SW using equation 3 presented by Gonzalez-Pinzon, then their critique would be spot on. However, that's not how SW was derived in this study and as a result their critique is off base and distracts from what is a very useful and insightful scaling exercise.

(2) I also have concern about the Gonzalez-Pinzon comment regarding the transport model used in this approach. Technically, there isn't a transport model used in this approach to estimate SW for the same reasons argued above. It is a stream specific empirical measurement based on the longitudinal loss of nutrient rather than a modeled parameter. Also, estimating SW by adding nutrients at a constant rate and waiting for a plateau concentration to be established incorporates uptake activity along all flowpaths for which the plateau condition is sensitive. That may not capture the very longest flowpaths traveled by a small percentage of water parcels and doesn't reflect activity along flowpaths that leave and never return to the channel in the study reach (sensu Payn et al), but it certainly represents more than the activity of a square channel with

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a single advective flowpath and makes me wonder whether these authors understand the field methods used for estimating SW in the Hall et al. paper. Runkel and others have suggested other ways of estimating SW using transport based models to better integrate channel versus transient storage uptake activity but that is a separate issue and unrelated to the scaling exercise conducted by Hall et al.

(3) In the end, Hall et al. Test the idea that uptake efficiency is a constant across river segments by seeing if it scales with specific discharge with a coefficient of unity. That basically asks whether the biological demand and concentration effects on uptake rates are effectively constant and variance in SW only derives from changing water depth and velocity which is a very nice null model for comparison sake.

Interactive comment on Biogeosciences Discuss., 12, 5991, 2015.