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Biogeosciences Discussions

Interactive comment on "Nitrate retention and removal in Mediterranean streams with contrasting land uses: a ^{15}N tracer study" *by* D. von Schiller et al.

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General comments

I found this paper to be reasonably well-written and a useful contribution to our understanding of nitrogen spiraling in streams. In particular, the parallel consideration of assimilatory and dissimilatory nitrogen processing is very interesting. However, I did find the Methods chapter too abbreviated, leaving the reader with several questions. E.g., it remains unclear how hydrodynamic parameters have been modeled with OTIS without using an upstream boundary condition. My largest concern with this paper is the absence of reliable reaeration estimates (via gas tracer additions or night-time oxy-

gen regressions; however, the authors have the data to calculate the latter) which may potentially introduce errors into both denitrification and oxygen metabolism estimates. Overall, I support publication of the manuscript. However, there are a number of issues that the authors should address before the manuscript is ready for publication.

Specific comments

1. Reaeration estimates

Reaeration rates of O2, N2, and N2O have not been measured in this study via gas tracer additions or night-time oxygen regressions, but been modelled using one of numerous empirical models available (e.g., see models reviewed by Genereux and Hemond 1992). According to my experience, reaeration rates obtained from different models can differ substantially and there is no way of knowing whether the model chosen by the authors delivers realistic results; particularly as the authors do not present any of the obtained reaeration coefficients. This is a crucial point for this study, because uncertainty in ecosystem metabolism (especially R) has been demonstrated to critically depend on the magnitude and precision of the reaeration coefficient (McCutchan et al. 1998). Likewise, uncertainty in denitrification rate should be strongly influenced by the magnitude and precision of the reaeration coefficient. Thus, I suggest that the authors attempt to estimate reaeration using the night-time oxygen regression method (Young and Huryn 1996). At least for the agricultural stream which exhibited sufficient GPP this should be possible. Alternatively, all conclusions related to ecosystem metabolism and denitrification should be toned down.

2. Metabolism estimates

Errors in the calibration of DO electrodes can result in substantial error of R in streams with high surface reaeration, such as the studied streams (McCutchan et al. 1998). Thus, the authors should add a few lines as to how and when the DO electrode was calibrated. As Clarke electrodes are subjected to drift due to biofilm development on

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the membrane and electrolyte consumption, it would be interesting to know whether the electrode was corrected for drift. Moreover, a graph on diel DO metabolism - such as fig. 2 in Mulholland et al. 2001 or fig. 4 in Gücker et al. 2008 - would be helpful. Finally, the respiration rate presented for the agricultural stream (table 1) is amongst the highest reported in the literature, but there appears to be no organic matter source or environmental condition (tables 1 and 2) explaining this huge difference in R between the agricultural stream and both other streams.

3. Hydrodynamic parameters

This may be a misunderstanding due to the too abbreviated Methods chapter, but with the experimental design described in the Methods (P3312 L21-23 and P3317 L1-6) hydrodynamic parameters can hardly be estimated. The estimation of hydrodynamic parameters from tracer additions using OTIS or any other 1-d transport model requires (1) an injection point about 100 times the mean stream width upstream of the first sampling position in order to allow for full lateral mixing of the tracer, (2) a first sampling position (used as the upstream boundary condition in the inverse modelling procedure) at which full lateral mixing of the tracer has occurred, and (3) at least one downstream sampling position against whose breakthrough curve the model parameters are fitted using least-squares etc.. The injection of the conservative tracer (and the labelled nitrate!) directly into the investigated reach without ensuring lateral mixing and the absence of an upstream boundary condition appears inappropriate.

4. Terminology

In my opinion, the definitions of N retention and removal as given and used in the paper (P3308 L5; retention=assimilation, removal=denitrification) are a bit too strict and dogmatic. E.g., N assimilated by benthic compartments is frequently exported to the floodplain during flood events leading to a permanent removal of assimilated N from the stream channel. In my opinion, referring to total nitrate uptake and different

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uptake processes (e.g., see Mulholland et al. 2004) is more appropriate.

On P3308 L9, L15, and throughout the text the authors refer to a spatial gradient ranging from forested over urban to agricultural using phrases such as *the concentration of ... increased from ... over ... to ...*. I do not think that this is appropriate with n=1 for each land use category. The authors may want to rephrase this throughout the text to read *... was higher/lower in ... than in ...*. I am also not convinced that the investigated streams are typical examples of the land use categories they have been assigned to by the authors. E.g., the agricultural stream does not exhibit typical agricultural stressors such as riparian clearcutting, higher water temperature and PAR, and increased SRP concentration and the urban stream has surprisingly low ammonium, SRP, and DOC concentrations. Instead, the difference in altitude between the forested stream and both other streams (about 900 m) and the 7- to 10-fold difference in discharge between the agricultural stream and both other streams appear to be more important differences than land use. I suggest avoiding generalization related to effects of land use in a revised manuscript.

Technical corrections

P3309, L2
Especially biogeochemists have traditionally appreciated reactive transport, whereas rather modellers may have (a long time ago) viewed running waters as inactive conduits. Give references for this statement or consider reformulating.

P3309, L12-13
This statement refers to ammonium, not nitrate.

P3311, L5
This appears to be an important argument in your paper (e.g., see title), but what is the ecological/biogeochemical significance of Mediterranean climate for N cycling? You may want to discuss this in greater detail. According to Gasith and Resh 1999, *streams*

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in Mediterranean-climate regions are physically, chemically, and biologically shaped by sequential, predictable, seasonal events of flooding and drying over an annual cycle.

P3316, L27
Spell CV out.

P3324, L4
Replace *taken* with *taking*.

P3324, L6-7
The argumentation that Vf should be used for intersite comparisons, because it corrects for stream size is commonly used, albeit U corrects for stream size in the same way. I personally think both Vf and U should be used for such comparisons, because U is not corrected for C, and thus includes interesting quantitative information on total uptake.

P3330, L27
This is a very speculative conclusion, given that it is based on n=1 and that the differences between the agricultural and both other streams were only about 3 mg/L DO. The difference in DO may partially be due to the lower discharge and current velocity of the agricultural stream and differences in reaeration associated with that.

References

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