

Interactive comment on “Nitrogen export from a boreal stream network following forest harvesting: seasonal nitrate removal and conservative export of organic forms” by J. Schelker et al.

Anonymous Referee #2

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GENERAL COMMENTS:

This study evaluates nitrate and dissolved organic nitrogen removal along the river network of a boreal catchment in Sweden that has been altered by forest harvests. This is an important scientific question given current forestry practices in boreal regions and predictions of increased forest use in the near future.

The manuscript is generally well written and within the scope of the journal Biogeosciences. However, there are some issues that could be addressed to improve the manuscript.

- The model used to estimate N removal would benefit from estimates of uncertainty.

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The general assumptions made are considerable. Some apparently smaller assumptions like using the average concentration of CC-4 and NO-5 to calculate Charvest seem dangerous without considering measurements of uncertainty or running different scenarios. Moreover, the model could be better explained to the reader. A figure may be helpful in this sense. For instance, it is unclear how dilution is accounted for. It seems that nitrate removal efficiency should be calculated with the flux rather than with the concentration. The area of stream network used to calculate U should be reported.

- Given the availability of nitrate, nitrite and ammonium data and the fact that ammonium seems to be almost as important as the other forms, I suggest that the authors redo their calculations to estimate the dynamics and removal of dissolved inorganic nitrogen (DIN) rather than nitrate. In this way, the manuscript would cover all dissolved nitrogen forms (inorganic=DIN and organic=DON), which according to the manuscript also represent most exported nitrogen because particulate nitrogen seems very low in these streams. Moreover, there will be no need of speculation on the processes that convert nitrate to ammonium or vice versa.

- The study design seems not justified well enough for the objectives of the manuscript. For instance, it is unclear why those catchments were chosen and why the catchments were differently harvested. The authors should explain it more clearly.

- The discussion seems too speculative in some parts, especially when it refers to processes and mechanisms that have not been measured in this study to explain some of the observed patterns. The authors could tone down some sentences.

- The use of some terms is confusing. The authors should clearly define the terms chosen and then use them consistently throughout the manuscript. For instance, the authors should clearly define what they consider the “stream network”, and then use terms like “in-stream”, “riparian”, “landscape” consistently. Another confusing use of terms occurs when using words like “uptake”, “removal”, “retention”, etc.

- The title could be improved to reflect more clearly the contents of the paper. It seems

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too long and confusing.

SPECIFIC COMMENTS:

P12062 L17: "Landscape" here means river network or the whole catchment (including terrestrial ecosystems). Please clarify. L21: "Net removal" within the river network? Please specify. L22-25: Unclear sentence. Especially the part that says "capacity and limitation of N-limited..." Please rephrase.

P12063 L17: Some studies have. You could cite here Bernhardt et al. 2003, Riscassi and Scanlon 2009, etc. L18: I suggest adding "stream" or "river" before "network."

P12065 L4: I think that the N limitation issue could be mentioned earlier in the introduction. Moreover, its consequences for this particular study should be explained. A hypothesis may emerge from here. L3-18: I miss some hypotheses and predictions here.

P12066 L1: Why was the riparian buffer left intact in this catchment and not in the other? L7: It seems quite strange that the samples were analyzed unfiltered. Why? Did you make some tests to see the influence of not filtering on your DIN and DON estimates?

P12068 L24: I understand that the efficiency can be set to zero but a negative value may also mean in-stream release of NO₃ (i.e. negative U values).

P12069 L16-19: It would be nice to see these different seasons depicted on the figures. This would allow the reader to follow results more easily.

P12070 L9-17: The scale of the figure does not allow seeing most of the described patterns.

P12071 L3-6: Why are U values negative? Net uptake values are usually positive if there is net uptake and negative if there is net release. I suggest changing it.

P12072 L8-16: Confusing paragraph. The supplementary figure is quite unclear and

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there is no figure legend or number. Unclear what is meant by upstream and downstream here and what the purpose of this paragraph is.

P12073 L11: Change to "zero or near-zero". L17: Did you try correlations with variables other than discharge? L29: It would be interesting to see and integrated U for the whole year (in kgN) that could be compared to other variables in Table 1.

P12075 L10-15: The effect of DNRA seems quite irrelevant here. I suggest removing these lines.

P12076 L14-19: These conclusions are ok, but they do not refer to consequences on stream network (in-stream) N removal.

Fig. 2: In the first panel it is not possible to see the temporal trends of the sites other than CC-4. Maybe you could try to use a log scale or to add a new panel/figure. What does "estimated Q" in the second panel mean? Please explain.

Fig.5: Strange to see U values as negative values. Also, I do not see the pairs of letters mentioned in the figure legend.

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