Biogeosciences Discuss., 12, C5325–C5333, 2015
www.biogeosciences-discuss.net/12/C5325/2015/

Biogeosciences Discussions
Open Access

BGD
12, C5325–C5333, 2015

Interactive Comment

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 #3

Received and published: 14 September 2015

Review of the paper BG-2015-331 "Nitrogen export from a boreal stream network following forest harvesting: seasonal nitrate removal and conservative export of organic forms" by Schelker, J., Sponseller, R., Ring, E., Högbom, L., Löfgren, S., Laudon, H.

General Comments

This is an interesting paper focused on how forest disturbances impact on stream water chemistry in boreal regions. The topic is relevant and the study fits perfectly within the scope of Biogeosciences. In general, the paper reads well and the introduction is well framed. I miss some information in the Study Site section such as the areas of the experimental catchments which can be useful to the reader for doing some back of the envelope calculations. The Methods section needs some extra work. The results are supported by a quite large amount of field and satellite data, though there are some results that need to be worked further. Rather than reporting patterns exclusively, the authors have brought the paper to a higher level by adopting a quantitative approach, which I mostly like. The discussion includes some results than need to be moved earlier in the text. Overall, I have some concerns with the applied mixing model in its present form. There are few other major issues that the authors need to solve before the paper can be published.

The authors focus their study on nitrate because they argue that forest harvesting increase the mobilization of inorganic nitrogen, primarily nitrate. However, they indicate (in the discussion section) that the contribution of ammonium to the total inorganic pool in stream water is pretty high (from 20 to >50%). Therefore, by modeling only nitrate concentrations, the authors may be missing an important piece of information. I recommend showing more clearly nitrate and ammonium concentrations for the two periods of study (2004-2006 and 2007-2012). If changes in ammonium concentrations are small between the two periods, this would support the approach considered by the authors. Yet, if ammonium concentrations change substantially between the pre-harvest and post-harvest period, the authors should consider the possibility of calculating the mixing model for DIN rather than for nitrate to get a more complete picture of how forest disturbances translate downstream.

One of the major issues the authors need to deal with is the uncertainty associated with the mixing model calculations because the response to clear-cut differed tremendously between the CC4 and NO5 catchments. This issue cannot be overlooked by the authors and requires careful consideration. For instance, the concentration of the clear-cut end member (C_harvest) is characterized by averaging nitrate concentration for CC4 and NO5. Yet, results and conclusions could differ markedly from the ones presented here if authors would have used nitrate concentrations either from CC4 or NO5 alone. According to the authors, the distinct response between these two catch-

BGD

12, C5325–C5333, 2015

ments may relay on the fact that riparian strips were kept in NO5 but not in CC4. If "leaving small (5-10 m) buffer zones along headwater streams is common practice" (12066.4), then one would expect that, on average, the mean response of the whole harvested area would be closer to NO5 than to CC4, being the later a more extreme scenario (savage clear-cutting without protecting riparian areas). By using the average of the two clear-cut catchments, the authors may be magnifying the "forest derived nitrate" and consequently the nitrate removal efficiency (Er) that is potentially attributed to in-stream processing.

Another issue that the authors need to address is the implicit assumption that chemistry for the clear-cut and control end members is representative of the water draining through the whole harvested and uncut area within BA2 and BA1. Or in other words, that the chemical signature of groundwater entering to the stream outside the experimental catchments is similar to the stream water chemistry of the end members. I understand that this assumption is needed for applying the proposed mixing model, but the authors need to include this assumption explicitly in the paper and discuss the advantages and limitations of their approach.

The interpretation of the modelled results should be explained in the Methods section rather than in the captions of the Figures. For instance, explain how the differences between modeled vs measured concentrations were interpreted, or the reasoning of why Er and Q should be or should not be related to each other.

Be consistent with the presentation of Figures and add letters to identify the different panels. The second panel in Figure 2 is not referred anywhere and it is not clear what the author mean by estimated and measured Q. Figure 3a and Figure 2b are redundant. The results in Table 1 are not included in the results section.

Specific Comments

Introduction

12064.24 clarify what you mean by “these relationships”

12065.13-18. The “questions” proposed by the authors are somehow interrelated because questions (i) and (ii) are focused on patterns, while (iii) refers to the involved processes and mechanism which lead to those observed patterns. Thus, I suggest some rewording for improving the strength of this final introductory paragraph.

Methods

12066.20. Include some more quantitative information about the areas that were harvested within the different studied catchments.

12066.24 Include for which catchments C_modelled was calculated.

12065.24. Include drainage area for the 4 experimental catchments.

12066.6. Indicate that water samples were also analyzed for chloride and silica (hydrological tracers) and that results on that were reported in a previous study (see later comment).

12067.3-10. This info could be partially moved to the Study Site section; focus this section on the description of the mixing model.

12067.17 According to eq. 1 “percentage” should be “fraction” and units would be “over 1” rather than in “%”. Otherwise the factor 100 should be included in eq. 1

12067.25. The response to clear-cut differed tremendously between the CC4 and NO5 catchments. Thus, there is a substantial uncertainty associated to these calculations. There are several possibilities to deal with this problem. For instance, the author could consider either an upper and lower limit for C_modelled or different harvest scenarios (with and without keeping riparian strips).

12067.26. “. . . each scaled to 100% harvest using a scaling equation” Why the authors expect that C_harvest will increase linearly with increasing the harvest area (eq 2)? And by how much the results obtained would change if another ecosystem re-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

sponse (e.g. asymptotic) would be considered? The reasons behind this assumption are not clear, especially when reading later in the text that Q_harvest may not change substantially between a catchment harvested 88% or 100% (12068.12).

12068.3 not clear what the authors mean by "reciprocal".

12068.4. Similar to C_harvested, the authors should consider some sort of confidence interval when characterizing the concentration of the control end member (C_control).

12068.19. According to the results presented Er was calculated the other way around: (modeled – measured)/modeled.

12068.23. Values of Er < 0 could be indicating either in-stream nitrate release and/or groundwater inputs with higher nitrate concentrations than stream water. This information could be useful for discussing some of the obtained results. I recommend further considering this variable when working on the revised version of the mp.

12068.25. From here on, this info does not relate to the "Mixing Model". Add a new subsection.

12069.14-16. By doing so, the authors are also assuming that stream water chemistry for the clear-cut and harvest end members is representative of the water draining through the whole harvested and uncut area within BA2 and BA1. Or in other words, that there may be no longitudinal changes in groundwater chemistry entering to the stream. Is this assumption reasonable? Do the authors have some additional data throughout the basin area to support this assumption? Could changes in groundwater inputs along the stream partially explain the observed patterns?

Results

12069.20 To improve the flow of this section, results could be divided in two subsections, one describing measured concentrations and fluxes; the other with the model results.

BGD
12, C5325–C5333, 2015

12070.15 This temporal pattern was also exhibited by CC4 but not for RS3 (as far as I can distinguish from the graph). You could reorganize these results in two paragraphs: the first focused on changes in concentration between 2004-2006 and 2007-2012 and the second focused on seasonal patterns.

12071.3-6. If U is the difference between modeled and measured fluxes, in-stream net areal uptake rates should be positive throughout the text. This would have more sense, since stream ecologists usually considered U> 0 when there is actually net nutrient uptake by stream biota.

12071.3-6. Were the Us obtained for BA1 similar to those for BA2? And if not, why the bioreactive capacity of this stream may change along the longitudinal axis? The discussion of the paper would benefit if showing these results more clearly.

Discussion

12071.8-17. The authors are right in that the marked response in CC4 was not observed downstream. Yet, it will be interesting to highlight the differential response exhibited by the two harvested catchments, especially because if riparian areas are usually protected against clear-cut, the response observed for CC4 may not be widespread.

12071.13 The results contained in Table 1 should have been introduced in the earlier section.

12071.18-24. These are results and should be moved to the earlier section.

12071.23. Clarify to which season you refer when saying 54 and 46%.

1207120-24. The contribution of NH4 to the total inorganic N pool is quite substantial, and thus, the authors could be underestimating the potential of in-stream processes to retain and transform DIN in these catchments. I wonder how different the results would be if considering DIN rather than nitrate alone. Could the authors provide some insights on that?

12072.4. In this case, it may be clearer to refer to the years comprising the two periods than to "pre-treatment vs treatment".

12072.8-10. These are results; moved them to the earlier section.

12072.11-16. What about NR7 and NO5? Did they show similar seasonal patterns than BA1, BA2? And if so, could one still say that "enhanced upstream inputs of nitrate in headwaters are translated downstream during the dormant season"?

12072.21-22. I recommend to briefly comment on that already in the methods section. The good match between measured and modeled concentrations for hydrological tracers would give consistency to the mixing model. Note, however, that the fact that the model works well for chloride but not for nitrate, does not necessarily imply in-stream nitrate retention because groundwater entering downstream the experimental catchments could have similar chloride concentrations but different nitrate/ammonia concentration than groundwater upstream.

12073.11-13. or that the concentration of nitrate was higher in downstream groundwater inputs.

12073.16-23. Avoid repeating results or adding new results in the discussion section.

12073.21-23. The authors should be cautious and take into consideration the uncertainties associated to these calculations before claiming that ca.70% of the nitrate inputs were removed. I suspect this figure is far too big, likely because the actual approach magnifies the effect of CC4.

12073.24-29. According to Table1, the decrease in nitrate loads between BA2 and BA1 was <30%, which is a much lower number than the 70% proposed. Thus, and assuming that all nitrate retention was occurring within the stream channel and that there were no differences in groundwater inputs between BA2 and BA1, U values for this stream reach would be several times lower than 6 microg N/m2/min. How would the authors explain this shift in the in-stream bioreactive capacity along the stream?

BGD

12, C5325–C5333, 2015

12074.11-26. These changes induced by forest harvest may be occurring only within the CC4 that (i) occupies a relatively small area of the BA2 and BA1 catchment and (ii) showed tremendous increases in nitrate concentrations. Thus I don't see how this explanation applies for patterns in BA2 and BA1.

12075.10-15. Too speculative. A more systematic analysis of the ammonium time data series will provide a clearer picture of whether seasonal changes in ammonium are terrestrially or stream derived. A table including ammonium and nitrate concentrations for the two periods could be useful.

12076.11. Not clear to which "two mentioned measures" the authors refer.

Figures and Tables

Figure 2. The second panel is not referred within the main text. Based on the Methods section it is not clear what the authors mean by estimated vs measured Q.

Figure 3. Differences between modelled and measured concentrations could indicate biogeochemical retention of the solute during transport downstream but also hydrological mixing with sources with different chemical signature. I recommend including the interpretation of the results in the Methods section where the authors can link the expected patterns to the assumptions underlying the model.

Figure 4. Panel (A) is not introduced in the main text and it is redundant with Figure 2. If Er >0 means nitrate retention, then the differences is between modeled and measured concentrations nitrate concentration. Why did the authors explore the dependency of Er on Q? This should be explained in the Methods section.

Figure 5. Positivize U values. To avoid any confusion to the reader, highlight in the caption that this is a potential maximum value for in-stream uptake. The letters for statistical significance are not included in the figure. Show data for BA1; differences in U values between BA2 and BA1 can enrich the discussion by supporting (or not) the explanations given for in-stream nitrogen processing.

BGD

12, C5325–C5333, 2015

Interactive Comment

Table 1. This table and results therein should be included in the results section.

Figure SS1. Please include the caption of this figure. Include data for the snowmelt period to be consistent with the data analysis throughout the mp.

Technical Corrections

12064.18. “photoautotrophic” rather than “autotrophic”

12067.7 or 2001-2011? (as in caption figure 1)

12068.26 Change “net uptake rates” by “net areal uptake rates” throughout the mp.

12069.7. change “treatment” by “clear-cut”.

12069.15. Change “loss” by “export”.

12069.24. Delete “buffer”

12070.1 and 4. Which treatment? Clarify.

12070.23. Change “nearly exclusively” by “usually”.

12072.4. Delete the “-“ sign.

Interactive comment on Biogeosciences Discuss., 12, 12061, 2015.

BGD

12, C5325–C5333, 2015

Interactive Comment