Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Dear Editor,

On behalf of all co-authors, I hereby submit a letter in reply to the review comments on the manuscript *'Aquatic greenhouse gas emissions unaffected by forest harvesting'*, by Marcus Klaus, Erik Geibrink, Anders Jonsson, Ann-Kristin Bergström, David Bastviken, Hjalmar Laudon, Jonatan Klaminder, Jan Karlsson, intended as a *full paper* contribution to *Biogeosciences*. Below we give a detailed response to all comments from the reviewers. We have addressed all requested changes, have included a response to each of the comments and listed the intended changes to the manuscript as appropriate (blue text).

Thank you again for considering this manuscript for publication in the Journal of *Biogeosciences*. We would also like to thank the reviewers for taking the time to review this manuscript and providing constructive feedback that will help improve the manuscript. If you require additional information or clarification, please do not hesitate to contact me at marcus.klaus@posteo.net.

Sincerely,

Marcus Klaus

Marcus Klaus

Reviewer 1 (RC1)

General comments:

Comment 1.0 This paper discusses the impact of forest harvesting on greenhouse gas emissions of boreal inland waters. This is done by analyzing four catchment sites, two of which were affected by forest clear cutting. Overall, the approach of the "Before-After/Control-Impact"-analysis is sound and, in general, the methodological approach is described adequately. However, in some cases more detailed information is necessary as pointed out below ('specific comments'). The study shows the impact of forestry activity on groundwater GHG concentrations and reveals the importance of the role of the riparian buffer zone-stream continuum although no clear conclusion on the mechanistic role can be drawn.

Reply: Thank you. We do not draw any clear conclusion on the mechanism that acts to buffer the increase in groundwater CO_2 and CH_4 concentrations, because at this stage, we regard different mechanisms (e.g. in-stream processing, riparian processing) to be equally likely. More detailed studies targeting these mechanisms are needed as we point out in the discussion (p. 9, L. 35-37).

Change: We will add results on BACI effects on gas concentrations in streams and lakes which will help us to narrow down the discussion of the mechanisms (see our response to comment 1.19 and 2.0).

We will provide more detailed information as pointed out in our replies to specific comments below.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Specific comments:

Comment 1.1 P2, L30: specify the measurement period more precisely (Jun – September?)

Reply: We agree.

Change: We will specify the sampling months (June – September) and also delete "throughout the whole open water period" on page 3, L. 17 to avoid redundancy.

Comment 1.2 P2, L33: what is 'normal' precipitation? Better: close to the long-term average of xx mm

Reply: We agree.

Change: We will rephrase the text following the reviewer's suggestion.

Comment 1.3 P3, L16: 'water chemistry' is not the right term here. Maybe merge paragraph 2.3 and 2.4 under 'Water sampling and physicochemical analysis'.

Reply: We agree.

Change: We will restructure the text following the reviewer's suggestion.

Comment 1.4 P3, L17-18: '... and the deepest point of the lake (Fig. 2) as described in S2.' (Consider also to reorder this sentence so that the described sampling activities match with the description in the supplement because the next sentence refers to S1, while the following paragraph refers to S2 again.)

Reply: We agree.

Change: We will restructure the text following the reviewer's suggestion.

Comment 1.5 P3, L21: spatial variability in CO2 and CH4 concentrations within streams

Reply: We agree.

Change: We will rephrase the text following the reviewer's suggestion.

Comment 1.6 P4, L3: 'Filtered water samples' also from streams and groundwater wells? Maybe specify here again, since in the first sentence you write 'To characterize lake color and this could lead to the impression that you are talking about lake water samples only in the second sentence.

Reply: Thank you for pointing out this typo. Color was determined for lake and stream water.

Change: We will clearly point out what type of analysis was done in what type of system.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
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Comment 1.7 P4, L7: you measured TP but never mentioned in the results. Why?

Reply: Thank you for pointing out this inconsistency. We did not include TP in this manuscript because, phosphorous is typically less responsive to clearcutting relative to nitrogen and primary production in our lakes is nitrogen and not phosphorous limited. This is clearly described in the introduction (P. 2, L. 4-11).

Change: We will delete the methods description for TP as we don't show any TP data.

Comment 1.8 P4, L24-33 in Figure S3 you indicate that you also used bootstrapping when modelling the k600 for lakes, but you never mention this in the text where you describe how you obtained the gas transfer velocity

Reply: Thank you for pointing out this inconsistency.

Change: In the main text we will refer to the detailed description in Text S6 on how we accounted for uncertainties in k_{600} estimates. We will also move details on error propagation procedures for k_{600} estimates for streams to Text S6 to improve text flow and have all details on error propagation condensed in one place. Finally, we also noticed that the error propagation procedure for gas flux calculations (Eq. 1) were not properly introduced and will refer to Fig. S3 when introducing Eq. 1.

Comment 1.9 P5, L17-18: you use Equ. (1) also to calculate CH4 and N2O fluxes, right? So c should be the respective gas concentration (not CO2 concentration).

Reply: Thank you for spotting this mistake. Eq. 1 was indeed used for all three gases.

Change: We will make this clear by rephrasing the text.

Comment 1.10 P6, L13: why did you set the 'after' period to 2013-2015? Shouldn't it be 2013-2014 if you want to analyze the clear-cut effects only (without the influence from site preparation)? Did you look at any trends/effects in the individual years after the clear-cut?

Reply: Clear-cut effects can be expected to last for more than the first two years. By first contrasting 2013-2015 with 2012 we were able to test for the general response in the first 3 years after clearcutting. Our additional analysis that contrasts 2015 with 2012, was done to test whether effects started to be visible after site preparation. We did not test for any trends, but regard the analysis of contrasting 2015 vs. 2012 and 2013-2015 vs. 2012 as a means of testing whether effects started to be visible after site preparation which may be overlooked if all three years were lumped together.

Change: We will reason more thoroughly in the chapter on "Statistical analysis" how we defined the "after" periods and why we chose the time intervals.

Comment 1.11 P6, L6: 'paired difference' – did you do all the measurements at the different sites at exactly the same time? If not, did you account for that in the LME?

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Reply: Sampling lake-, stream- and groundwater in one catchment took a whole day for us. Hence sampling at exactly the same time point was logistically impossible for us as an individual research group. However, we tried to sample Control- and impact- lake pairs as close in time as possible, typically within 2-3 days, but never more than 7 days from each other. We did not account for this minor variation in sampling dates in the LME.

Change: We will point out more clearly in Chapter 2.3 that control and impact catchments were typically sampled within two or three days, but never more than seven days from each other.

Comment 1.12 P6, L10: what were the results of the pseudo-BACI?

Reply: We are grateful for pointing out this inconsistency between methods and results. We included the pseudo-BACI analysis in an earlier version of this manuscript, but after a round of revisions, decided to not include it to not overload the paper with details and to sharpen the focus. The pseudo-BACI revealed no significant BACI effects in any of the control catchments, which gives us more confidence to state that the BACI effects found in the clear-cut catchments were due to the clear-cut treatment.

Change: We will delete the method description on the pseudo-BACI as we do not show any related results.

We realized that the caption of Table S5 was misleading in this context. We will replace "in control and impact catchments" by "at control and impact sites in the impact catchments".

Comment 1.13 P6, Results: in general, when you present (mean?) values, indicate that those are (multi-?)seasonal means etc. For example, on P7, L4 you write 'Whole lake temperatures (ranging from 12.8-16.5 ˚C) ...' – but that's the range of the mean values and not of the entire measurements, right? (also check those numbers; different from Table 2)

Reply: Thank you for this comment!

Change: We will clearly state in the beginning of the results section that we refer to arithmetic mean values over each of the two time periods (before, after).

Comment 1.14 P7, L4-5: I think the wording here is confusing because temperature did not decrease but it actually increased only more so in the control. Any idea/explanation for that?

Reply: We are aware of that temperatures in control lakes increased more than in impact lakes. We express this differential effect by using the formulation "decreased ... relative to control lakes". However, we agree that his might be confusing.

The increase in whole lake temperatures was likely due to the higher air-temperatures in the after period relative to the before period (Table S1). As we point out on P. 10, L. 2-4, the effect size of -0.4˚C was

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
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small (Cohen's D = -0.20) and had likely no ecological or biogeochemical effects. We therefore did not speculate further on this effect in the manuscript.

Change: We will rephrase the text according to the reviewer's suggestion. We do not know and will not attempt to speculate what could have caused the rather minor difference in temperatures.

Comment 1.15 P7, L25: 'medium effect size of +533 µM or +56%' – 533 µM is the slope of your LME, but since you included lake pair as random effect also on slopes, you should get two slopes!? Is this the mean? This also applies to all the results/tables where you present slopes/effect sizes. How do you get the 56%?

Reply: We indeed get two slopes and intercepts, one for each pair. We here present arithmetic mean slopes and intercepts. The relative effect size (here: 56%) is the effect size divided by the mean value in the impact system in the control year.

Change: We will explain more clearly how slope and intercept is calculated and add a definition of the relative effect size to the methods section.

We realized that the relative effect size was incorrect for groundwater CH_4 concentrations and will replace 845% by the correct number (822%). This will have no effect on the conclusions of this study. The relative effect size for groundwater CO_2 concentrations was correct.

Comment 1.16 P8, L29 ff: Discuss your results in the same order as you present the results.

Reply: We disagree with this suggestion. We think that it is most logical to start the results section with the background data (chemistry, hydrology) and slowly built up to finish off with GHG fluxes, but to start the discussion with the main finding (effects on GHG fluxes) and then relate this to findings on hydrology and chemistry.

Change: No change will be carried out.

Comment 1.17 P9, L4: enhanced organic matter degradation, but maybe also increased organic matter input due to forestry activity in the first place?

Reply: Enhanced organic matter degradation does not exclude the suggested mechanism of enhanced organic matter inputs from logging residues. However, we agree that logging residues should be specifically highlighted as a potential source. Logging resides indeed often increase nutrient and carbon decomposition and leaching, (e.g. Palviainen et al. 2004, Plant and Soil 263; Mäkiranta et al. 2012, Soil Biology and Biochemistry 48). Some CO2 and CH4 may be formed from degradation of logging residues in the soil and partially be emitted directly from soils to the atmosphere and partially contribute to groundwater CO2 and CH4 levels. However, the relative magnitudes of these fates are presently unclear.

Change: We will mention this alternative explanation along with other potential explanations of the observed groundwater concentrations.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Comment 1.18 P9, L5: actually, the explanation would be the reduced CH4 oxidation

Reply: We agree with the reviewer.

Change: We will use the more specific term "oxidation" instead of "net uptake".

Comment 1.19 P9, L21: info/effects on wind speed are summarized in table 2, not table 4. Not sure if you can draw any conclusions on additional forcing on air-water gas exchange velocities, since you actually didn't measure wind speed above the lake. Also considering this, it would be interesting to see the effects on lake water GHG concentrations. Did you check this? If there are no significant effects, maybe just mention this in the first sentence of paragraph 3.3 (i.e. 'Forest clear-cuts did not affect lake water GHG concentrations (data not shown).').

Reply: Thank you for pointing out the typo and suggesting to mention how GHG concentrations responded to the treatment! We indeed tested for BACI effects on greenhouse gas concentrations, but did not detect any significant effects, except for stream CO_2 concentrations (decrease). (see also reply to comment 2.0).

*We agree that wind measured above the mires may differ from wind above the lakes. Although the wind may not be exactly same in **absolute** terms, the weather stations were installed at mire locations with wind conditions as similar as possible to the lake and we are confident that our data adequately reflect the **relative** differences between lakes and years. In the clear-cut catchments, wind was measured on open mires right next to the lakes. The mires had about the same size as the lakes and were surrounded by similar vegetation. The forest buffer zone left around the mires was similar to the forest buffer zone left around the lakes. In Lillsjölidtjärnen, the mire-buffer zone was slightly wider, while in Struptjärn, the mire-buffer zone was slightly more narrow (Fig. 2).*

Change: We will refer to Table 2 instead of Table 4.

We will add a brief note to the methods section on how representative our wind speed measurements on mires were for lake conditions. In the discussion, we will acknowledge the uncertainties in wind speed estimates and tone down our interpretation that forest buffer zones effectively buffered greenhouse gas emissions from clear-cuts.

We will add a note on the treatment effects on GHG concentrations in the results section as suggested by the reviewer, include the associated BACI results in the supplementary material and refer to these results in the discussion. The decrease in stream CO_2 concentrations in response to clear-cutting will give us further support for our hypotheses that riparian or in-stream processes buffered clear-cut effects.

Comment 1.20 P9, L27: however, this does not explain the results for CH4?

Reply: Indeed, this would only explain results for CO_2. Enhanced in-stream methane oxidation in the sediments is likely primarily an effect of the commonly found substrate limitation of methane oxidation (e.g. Bastviken 2009; Duc et al 2010; Segers 1997), i.e. methane oxidizer communities have a higher capacity than commonly expressed and will oxidize more CH4 when concentrations increase.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Change: We will clarify this possible explanation for CH_4 in the revised text.

Comment 1.21 P9, L38-39: 'The relative pH decrease of 0.5 units...' – but the Effect size (slope) of pH in Table 2 is 0.00.

Reply: As indicated by "" and in the footnote, the model parameter estimates are based on log-transformed data (to follow best practices in calculating statistics for pH and accounting for the fact that pH is -log_{10}(activity of H^+)). Due to rounding, the slope appears to be 0.00, but is in fact -0.0000383 (Lake Epi) and -0.000077 (Stream).*

Change: We will increase the number of decimals shown for pH to make the data appear correctly.

Comment 1.22 P18 ff: check all your tables for consistency (i.e. compare with the numbers you write in your results).

Reply: Thank you for this reminder.

Change: We will confirm consistency for all numbers in the tables and text. We will correct minor mistakes in numbers given on page 8, L. 18, 19, 26 and 28. These corrections have no effect on the discussion or conclusion.

Comment 1.23 P19, Table 2ff: p-value: maybe highlight significant effects

Reply: We agree.

Change: We will highlight significant effects by bold p-values.

Comment 1.24 P22, Figure 1: A)-C) not really clear what is shown in the pictures. Is A) and B) the same lake but picture taken from different angles? Is B) also before the clear-cut?

Reply: We apologize for not being clear enough.

Change: In the figure caption, we will add "(A, D)" after "before" and "(B, C, E, F)" after "after". We will also add y-axis labels "Lillsjölidtjärnen" and "Struptjärn" to the figure to clarify which catchments the pictures refer to.

Comment 1.25 There is no dashed line in C)? Why are there pictures of only two of the four field sites?

Reply: Thanks for pointing out the sub-optimal explanation of the dashed line.

We show only pictures of the clear-cut catchments here, because we want to highlight changes before and after clear-cutting. We did not include pictures from the control catchments to not overload the figure.

Change: We will explain the dashed line more clearly in the figure caption.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Comment 1.26 Figure 2: Nice. Maybe exchange C) and D) to have the lakes in the same order as in Table 1

Reply: Thank you.

Change: Change will be adopted. We will also modify the scale bars in all panels to improve readability of numbers.

Comment 1.27 P23, Figure 3: Boxplots instead of bars; also for Figure 6 and 7.

Reply: We disagree. We argue that the data visualization should reflect the statistical analysis. We are interested in treatment effects on the arithmetic means in greenhouse gas fluxes. This is what our BACI analysis tests for. To reflect this, we present bar charts of arithmetic mean fluxes (±standard error). Boxplots would be misleading as they would imply that we tested for differences in the distribution of the data. Boxplots would also not be suitable to express the uncertainties in mean values that we obtain from our error propagation procedure and show in Fig. 3, 6 and 7.

Change: To prepare the reader better for the type of graphs we will show, we will point out more clearly in the introduction and methods section that we are interested in and tested for changes in the arithmetic mean gas fluxes.

Comment 1.28 P24, L5 (Figure 4): what is 'minimum ice extent'?

Reply: We agree that this term might cause confusion. Our ice-in and ice-out dates were based on field observations. As we did not visit the lakes every day, this estimate is associated with uncertainties. We express these uncertainties by showing the maximum and minimum ice cover duration based on the earliest and latest possible ice-in and ice-off dates.

Change: We will rephrase the figure caption to improve clarity.

Comment 1.29 P26, L14 (Figure 7): 'summarized as arithmetic means over ten bootstrap runs that take between-chamber variability into account (see Fig. S3)'. In Figure S3, bootstrapping is only indicated for the BACI statistics. From the Figure and the text it is not really obvious how you used bootstrapping and how you take between-chamber variability into account.

Reply: Thank you for pointing out this lack of clearness.

Change: We will modify Figure S3, indicating how errors were propagated for the area-weighted depth-zone specific averaging. In the figure caption, We will refer to Text S6 for details on the error propagation procedure.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Comment 1.30 Supplement, P1, L34: how did you account for the much higher measurement height of the wind speed at Stortjärn?

Reply: Thank you for pointing out this missing piece of information. We corrected wind speed from mast height to 10 m assuming a logarithmic wind profile following Crusius and Wanninkhof (2003, Limnology and Oceanography 48).

Change: We will clarify this in Ch. 2.6.

Comment 1.31 Technical corrections:

In general, use passive voice ('atmospheric fluxes were quantified' instead of 'we quantified atmospheric fluxes'.

Reply: We agree to use passive voice wherever suitable.

Change: We will use passive voice throughout the manuscript unless active voice is needed to highlight our own thoughts or actions and distinguish from other thoughts or actions cited in the context of a sentence (e.g. on page 9, L. 25-37).

Introduce abbreviations the first time the respective spelled-out word is used and use abbreviations throughout the rest of the manuscript (i.e. for carbon (C), greenhouse gas (GHG),...).

Reply: We agree.

Change: Change will be adopted throughout the manuscript. We will also properly introduce the abbreviation for root mean square error (rmse) and remove the abbreviation for gas chromatographer (GC) and use the full word instead.

P1, L10: 'greenhouse gas (GHG)'; use abbreviations throughout the rest of the manuscript.

Reply: We agree.

Change: Change will be adopted throughout the manuscript.

P1, L23: 'carbon (C) and nitrogen (N)'; use abbreviations throughout the rest of the manuscript.

Reply: We agree.

Change: Change will be adopted throughout the manuscript.

P2, L10: 'oxygen (O2)'; use abbreviations throughout the rest of the manuscript.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Reply: We agree.

Change: Change will be adopted throughout the manuscript.

P2, L25: 'site preparation' (be consistent with the use of hyphen)

Reply: We agree.

Change: Change will be adopted.

P2, L26: 'CO_2, CH_4 and N_2'

Reply: We agree.

Change: Change will be adopted.

P2, L32: '1-3 °C'

Reply: We agree.

Change: Change will be adopted throughout the manuscript.

P3, L32-33: 'At the deepest point of each lake, at the stream master site and at the groundwater wells...'

Reply: We agree.

Change: Change will be adopted.

P4, L24: For both lakes and streams gas transfer velocities (k), the water column depth that equilibrates with the atmosphere per unit time, were obtained as described in the following. (Use passive voice, no comma after "streams", no hyphen in "gas transfer")

Reply: We agree.

Change: Change will be adopted.

P4, L26: 'wind speed'

Reply: We agree.

Change: Change will be adopted.

P4, L37: delete 'respectively'

Reply: We agree.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Change: Change will be adopted.

P5, L2: 'sub-reach'

Reply: We agree.

Change: Change will be adopted.

P5, L20-21: 'Atmospheric CO2 and N2O concentrations were 425 ppm and 350 ppb (median of biweekly in-situ measurements), respectively, and atmospheric

Reply: We agree.

Change: Change will be adopted.

P5, L40: '...were the arithmetic mean flux of all chambers located at the respective depth.'

Reply: We agree.

Change: Change will be adopted.

P6, L3: 'site preparation'

Reply: We agree.

Change: Change will be adopted.

P6, L9: 'soil sampling' – before you just talking about groundwater sampling so try to be consistent with the wording. See also P7, L7.

Reply: We agree.

Change: We will be consistent with the wording throughout the manuscript.

P6, L12: (Pinheiro et al., 2015) is the citation for the R package so put it after "'lme' function"; also give citation for the program R and mention which version you used.

Reply: We agree.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Change: Change will be adopted. We will introduce in Chapter 2.3 that "All data analysis described in the following were done using the statistical program R 3.2.2 (R Development Core Team, 2015), if not declared otherwise."

P7, L4: '16.5 ˚C'

Reply: We agree.

Change: Change will be adopted.

P7, L8: delete 'Here'.

Reply: We agree.

Change: Change will be adopted.

P8, L1: the symbol for mole is 'mol' not 'M', i.e. 99 mmol m-2 d-1. See also L4, L5, L10.

Reply: We agree.

Change: Change will be adopted.

P8, L6: delete 'clear' (it's double)

Reply: We agree.

Change: Change will be adopted.

P8, L14: 'mmol m-2 d-1'

Reply: We agree.

Change: Change will be adopted throughout the whole manuscript.

P8, L16: 'varied from 1.2 to 1.3 mmol m-2 d-1 in the control stream and from 0.07 to 0.18 mmol m-2 d-1 in the impact streams'

Reply: We agree.

Change: Change will be adopted.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

P8, L22: delete 'linear mixed-effects models' or just use abbreviation

Reply: We agree.

Change: Change will be adopted.

P8, L26 and L28: '_mol m-2 d-1'

Reply: We agree.

Change: Change will be adopted.

P9, L2: 'However, aquatic GHG emissions are also fueled by direct catchment inputs of the respective dissolved gases'

Reply: We agree.

Change: Change will be adopted.

P9, L8: replace 'in average' with 'on average'

Reply: We agree.

Change: Change will be adopted.

P20, Table 3: 'Effect size of forest clear-cutting on DIC and CH4 concentrations (_M) in groundwater in the impact catchments.'

Reply: We agree.

Change: Change will be adopted.

P25, L4 (Figure 5): replace 'lakes' with 'streams'

Reply: We agree.

Change: Change will be adopted.

P25 f Figure 6 and Figure 8: delete 'dissolved'

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Reply: We agree.

Change: Change will be adopted.

Supplement, P2, L17: 'dissolved inorganic carbon (DIC)' – you already use the abbreviation before (e.g. in L8 and in the main text)

Reply: We agree.

Change: Change will be adopted.

Supplement, P14, Table S4: check numbers! "Before" should have the same values as in Table 2, right?

Reply: The mean values reported in Table 2 and Table S4 are indeed not exactly the same for some variables (e.g. lux). For these variables, we report bootstrapped mean values that take uncertainty due to gap filling into account. Hence, slight differences in reported mean values can occur, but note that these values are similar in a statistical sense (within the limits of their standard error).

Change: No change will be carried out.

Reviewer 2 (RC2)

Comment 2.0 Klaus et al. studied greenhouse gas emissions (CO2, CH4, and N2O) from lakes and streams in catchments that underwent forest harvesting. Using a BACI design in four boreal catchments, they found very little change in greenhouse gas emissions after harvesting. The study was well designed and well executed. The manuscript is well written. I have some minor comments and suggestions for improvements. The only major comment I have is that as far as I can tell the authors don't report the differences in CO2, CH4, and N2O concentrations in surface water in lakes and streams, they just report the fluxes. The only significant difference they found is in concentrations of the greenhouse gases in ground water, but what about concentrations in surface water? If there is a lack of difference in concentrations, that might help reduce the number of potential explanations for the lack of responses in fluxes. If there were no differences in concentrations, the authors should state that.

Reply: Thank you for suggesting to mention how GHG concentrations responded to the treatment! We indeed tested for clear-cutting and site preparation effects on greenhouse gas concentrations, but did not detect any significant effects, except for stream CO_2 concentrations (decrease) (see also reply to comment 1.19).

Change: We will briefly mention the treatment effects on greenhouse gas concentrations in streams and lakes in the results section, add associated BACI statistics to the suppl. material and refer to these results in the discussion. The decrease in stream CO_2 concentrations in response to clear-cutting will give us further support for our hypotheses that riparian or in-stream processes buffered clear-cut effects.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Below I provide specific comments.

Comment 2.1 Page 1, lines 11-14- I would separate into two sentences after the word Catchments. It is a very long sentence!

Reply: We agree.

Change: We will separate the sentence into two.

Comment 2.2 Page 5, line 4- seems like low agreement between k600 measurements and estimates. Is this common in the literature?

Reply: Our k_{600} estimates indeed show relatively poor agreement between methods. However, low agreement across different methods is common in running waters (Lorke et al. 2015, Biogeosciences 12; Hall and Madinger 2018, Biogeosciences 15) just as it is in lakes (Gålfalk et al. 2013; Journal of Geophysical Research 118), mainly because there can be extensive high-resolution variability in k in both time and space so differences also between nearby locations and over short distances are to be expected.

Change: No change will be carried out.

Comment 2.3 Page 5, line 25- add: after modifications

Reply: We agree.

Change: Change will be adopted.

Comment 2.4 Page 7 line 35- why are concentrations of CO2, CH4, and N2O in lake and stream water not reported?

Reply: Thank you for this question! We realized (also per comment 2.6) that reporting concentrations of greenhouse gases in lakes and streams would provide valuable context to the flux estimates given, and facilitate comparisons to results given for groundwater concentrations.

Change: We will report greenhouse gas concentrations (see response to comment 2.0) and set changes in groundwater into the context of stream and lake water concentrations in the discussion.

Comment 2.5 Page 8, line 41- N2O does not result from bacterial decomposition of inorganic N. It results from incomplete denitrification and nitrification. I would reword this sentence.

Reply: Thanks for pointing out this inaccuracy!

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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Change: We will reword the sentence as suggested and also modify the following sentence and add "DIN" in addition to DOC derived from catchment soils as a potential driver of aquatic greenhouse gas fluxes.

Comment 2.6 Page 9 lines 9-12- I don't follow the percent increase in CO2 and CH4 calculations. Is the 8.45 fold increase, the equivalent of an 845% increase? Also, I am a little confused because these are calculations for changes of concentrations, but you never provide the concentrations changes for lake and stream water, just the fluxes.

Reply: The relative effect size (here: 56%) is the effect size divided by the mean value in the impact system in the control year (see also our reply to comment 1.15).

Change: We will explain more clearly in the methods section how slope and intercept is calculated. We will also add a definition of the relative effect size to the methods section.

We realized that the relative effect size was incorrect for groundwater CH4 concentrations: we will replace 845% by 822%. This had no effect on the conclusions of this study. The relative effect size for groundwater CO2 concentrations was correct.

Comment 2.7 Page 9 line 20- I think the word "remain" should be changed to "retain"

Reply: Thank you for spotting this typo.

Change: Change will be adopted.

Comment 2.8 Table 2- why do the Control and Impacts have such different discharges (27-40 L/s versus 3-4 L/s).

Reply: The streams included in our study are all representative for headwater streams in the Swedish Boreal forested landscape. The control stream had higher discharge than the impact streams because of its larger catchment area (catchment-area specific discharge was similar in all catchments (1.0, 1.4 and 1.5 mm d^{-1} in Struptjärn, Lillsjölidtjärnen and Övre Björntjärn in June-September 2012) and well within the range of what has been measured previously in our study region (c.f. Lyon et al. 2012, Water Resources Research 48). Our study focusing on greenhouse gas emissions from streams and lakes is only one of many that are about to result from this project (see e.g. Deininger et al., accepted in "Ecological Applications", focusing on clear-cut effects on in-lake basal productivity). The different interests in this experiment made it particularly challenging to find control and impact catchments that were similar in all variables of interest.

Change: We will add specific discharge data as background information to the study site description.

Comment 2.9 Figure 3- why 37.5-42.5 and then 5-105cm depth? It seems strange to have a shallow and then the whole soil column together? Why not separate shallow vs deep?

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

Umeå, 2018-08-07

Reply: We agree that this figure legend was confusing and suggest a clearer description of the soil water sampling.

Depth specific groundwater sampling was done to target depths that are hydrologically most strongly connected to stream water (Leith et al. 2015). Depth integrated sampling was done to characterize the whole soil profile.

Change: We will alter the figure legend and instead write "Concentrations of DIC and dissolved CH_4 in groundwater at depth specific locations (37.5-42.5 cm; panel A-B) and depth integrated locations (5-105 cm; C-D)…". We will introduce the terms "depth-specific" and "depth integrated" in Chapter 2.3 and clarify why we sampled at a specific depth and integrated across the whole soil profile. This will imply moving some of the details given in Text S2 to the main text.

We will also be more consistent throughout the manuscript and only speak of groundwater instead of soil water as done partly in the previous version of the manuscript. We will also specify more clearly in the captions of Table 2 and S4 that groundwater refers to the depth-integrated locations.

Comment 2.10 Figure 5- it would make easier to compare across sites if all panels had the same scale on the y-axes.

Reply: We agree.

Change: We will modify Figure 5 so the y-axis scale is the same in all panels that show CO2 fluxes in the three study streams.

Further changes that will be done

We noticed slight inaccuracies and missing details in the methods description of the gas transfer velocity measurements (Text S4). We will correct these mistakes and add details.

We realized that the first paragraph of the introduction focused on carbon cycling only while our manuscript also includes nitrogen cycling (N_2O). To make this consistent, we will also refer to nitrogen cycling and include two additional references (Sponseller et al. 2016, Seitzinger and Kroeze 1998), replacing one of the carbon-related references (Jonsson et al. 2007).

We will add a brief clarification in chapter 2.2 that *"treated catchments are referred to as 'impact' catchments and untreated catchments as 'control' catchments"* in the remainder of the manuscript.

We realized that we overlooked a statistically significant treatment effect on stream DIN concentrations. We will mention this effect in the results and discussion section.

In the submitted manuscript, we cited Deininger et al. (unpublished data). This data is now accepted for publication in the Journal *Ecological Applications*. We will refer to this reference instead.

Marcus Klaus
Department of Ecology and Environmental Science
Umeå University
SE-901 87 Umeå, Sweden
E-mail: marcus.klaus@posteo.net

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In the submitted manuscript, we used the term "emission" in some occastions. Since streams and lakes are not necessarily consistently oversaturated in greenhouse gases relative to the atmosphere (which would imply emission), we will consistently use the term "air-water fluxes" throughout the whole manuscript.

We noticed that we refer to two different references by Schelker et al. (2013) but did not distinguish in the main text which of the two we refer to. We will clearly indicate by letters "a" and "b" which reference we refer to.

The name of Lillsjölidtjärnen was misspelled in Fig. S2. We will correct this.