

Interactive comment on “Riparian zone controls on base cation concentrations in boreal streams” by J. L. J. Ledesma et al.

Anonymous Referee #1

Received and published: 12 March 2013

General Comments

Overall, the manuscript addresses base cation concentrations in forested catchments, including selected riparian zones and streams. Identifying catchment contributing areas, such as upslope forests and riparian zones, is important to assess the roles of forest harvesting and climate change on streamflow and water quality in boreal and temperate biomes. In this regard, data contained in the manuscript may be worthy of publication; but not in its current format. For example, I am not convinced the adequate statistical design was used in the study. Granted, you have a partially nested catchment study, but I am left to wonder what your experimental units were and how they were stratified across sub-catchments. There are at least four levels of site cited in the text, including riparian zone, stream, wetness, and soil; with no mention of ex-

C285

perimental units. If each riparian zone is a ‘site’ then the study is not replicated. It also seemed like comparisons among riparian zones, streams, wetness, and soil types were all trying to be haphazardly attempted with a limited dataset. I believe testable hypotheses, with the appropriate statistical design needs to be clearly stated, around which the paper should focus. This includes consistency in statistical terminology.

I also believe the methods require more in-depth description because I was confused as to the focus of the paper. For example, were you trying to calibrate RIM for base cations in riparian zones or using it as a guide to understand riparian zones and identify uncertainties for future studies? Given that this was the first application of base cations using RIM, I wonder why (1) few data were presented that compare modelled versus measured concentrations and fluxes, (2) only a pedestrian view of the model description was presented, and (3) statistical and conceptual bases of the assumptions were not thoroughly described and tested? These are particularly important given that discharge estimates from one stream were used to calculate flow-weighted base cation concentrations for all riparian zones and streams.

Finally, the manuscript requires major restructuring. For example, I suggest an overall objective with hypothesised conditions and how they were addressed in the study to be clearly articulated in the Introduction. The Discussion is primarily a revisiting of the Results without any discussions of model calibration/corroboration with field data or applying results to intensified forest harvesting or climate change, although these were important reasons justifying the study in the Introduction. There are also a number of occasions in the Discussion where a concept is mentioned once then dropped; I was left to wonder what point was being made.

I also provide specific comments that the authors may find useful.

Specific Comments

Abstract

C286

Page 740 Lines 9-22: why not use “organic” versus “mineral” deposits as the riparian types to compare and then use the number of each type as your plots?

Line 13-16: if Mg/Ca ratio provides a distinct upslope mineralogical signal, why is this not part of the study. Also, in the Results and Discussion, you talk about stream Mg/Ca being similar to soil solution in the riparian zones. Which was it, or is it both and upslope and riparian contributions cannot be detected based on the current study design? Were Mg/Ca stable throughout the sampling period? Define “good”. This is a subjective term, where one person’s interpretation may differ from another’s. What data support the predictability of the model?

Lines 20-24: do these data mean more riparian influence at larger streamflow? How is this differentiated from upslope contribution? Could it be that near-surface soil in the upslope areas contribute to chemical dilution during rapid runoff? Also, what about surface flow through riparian zones?

Lines 22-24: I am not convinced that results as presented justify this conclusion.

You may want to synthesise lines 13-20 into a hypothesis.

Introduction

Page 741 Line 4: define “good”

Lines 6-8: do you mean more upslope contributing area?

Line 10: what does the riparian zone buffer?

Line 15-17: I am not sure what you mean by a third paradox. Please explain what this means.

Lines 19-25: So what? Is this going to be the riparian process studied? If so, make this transition to the above paragraph.

This page needs to be re-structured. You may want to point out that riparian zones

C287

provide multiple ecosystem services, such as (1) pollutant adsorption (inorganic and organic), (2) nitrogen retention, (3) acidity buffering, and (4) organic matter transfer to streams.

Page 742 Lines 1-13: Olsson et al. (1993) was a field study, was it not?

Line 9: what is the “transmissivity feedback mechanism”?

Lines 14-21: is RIM a conceptual or mathematical model? Why study this? What is the importance of base cations and their potential depletion? I suggest framing questions in terms of concerns about base cation depletion and coupling (integrating) upslope and riparian zones as contributing source areas.

Line 26: why is this first mention of Si? Why is it different from base cations?

Page 743: Lines 1-5: if this is the first attempt to model base cations, why is model performance not included? I think that should be the first objective.

An overarching objective is needed, with your hypothesised conditions, and how your study addressed hypotheses.

Study area

Page 743 Lines 20-21: “. . .forest (87%), wetlands (9%), and lakes (1%) are not land uses, but site types or cover types.

Lines 23-26: is straightening channels a limited impact?

Page 744 Lines 2-4: any information on annual timber removals?

Lines 9-10: where are these located? Scots pine, Norway spruce, and birch make up 100% by your designation, so how can there be peatlands?

Lines 13-15: If rivers and incised, should this not be relative to stream water height?

What do you mean by “Riparian Observatory”?

C288

Line 25: OK, so you have nested catchments, what was the design for your study? Why are there no characteristics of the riparian zones?

Lines 26-28: why was sampling only conducted twice during spring?

Page 745 Study design: what are your hypotheses and statistical design – i.e., experimental units, random or fixed effects, balanced or unbalanced?

What are the upslope/riparian zone proportions in the catchments?

Lines 15-19: why not use groundwater if that contributes mostly to streamflow?

Lines 23-26: why annual if spring runoff differences occurred between years? How does RIM calculate flow-weighted averages? Why not use volume-weighted from the volume of water in the lysimeters?

Page 746: Lines 1-5: I am not sure what is meant here. Why were fluxes not calculated and what about including mass balances?

Lines 10-12: why only stream C7 and I am not convinced your assumption is valid.

Lines 13-18: where were uncertainties estimated from and how were they transferrable to your study? Combined, the total error may be large. Have the referenced studies used discharge measurements for one sub-catchment to apply to others?

Lines 20-23: how was specific discharge measured? What were the experimental units?

Page 747 Lines 210: again, define experimental units. Why scale and not just code as missing values? What is the certainty of the scaling factor?

Lines 13-17: what does 'binned' mean? A justification for using discharge from one stream to scale all riparian zones and streams is needed.

Lines 22-24: if you are calling riparian zones sites, then there is no replication in the study.

C289

Page 748 Lines 1-11: Justifications for the assumptions are needed.

Lines 13-24: How do the model numbers compare to those collected from the field?

Page 749 Line 13: what are 1.96 standard deviations?

Line 15: why are riparian zones and streams both referred to as site?

Page 750 Lines 5-15: what was the experimental design for riparian zones? Do they not consist of different soil characteristics? There are no replicates as presented. Maybe consider 'organic' versus 'till'?

Results

Lines 19-23: These should be your 'treatments'. Perhaps soil type as main effect and then dry, humid, and wet as co-variables. Also, this is the third mention of site in the paper. I have become confused as to what you are trying to compare.

Page 751 Lines 4-15: you may want to present these data by soil type and then, if applicable, how moisture regime influenced within soil types.

Lines 10-11: does no clear pattern mean they behaved the same way?

Lines 23-28: why not look at sampling depth separately as opposed to lumped per riparian zone?

Line 28: what was K higher then?

Page 752 Lines 1-3: how were statistics conducted? If comparing riparian zones, then no replication exists and you cannot conduct ANOVA.

Lines 5-8: I am not sure what is meant here. If similar, how can they differ?

Lines 13-15: How were soils sampled for mineralogy? Is there any other soil chemical and physical characteristic data available?

Lines 20-23: what is mean by "The dilution was small in forested subcatchments. . .?"

C290

Page 753 You should present data for RIM-calculated flow-weighted concentrations at different depths and compare to field measurements.

Lines 1-28: too much detail is presented and I suggest presenting data from soil type.

Lines 18-28: this information belongs in Discussion

Page 754 Lines 13-15: what about the headwaters from where your streamflow measurements were taken?

Line 27: what was the source of silt; glacio-fluvial or till? If deposited by till, they would be the same Quaternary deposit.

Page 755 Lines 1-8: You don't have all the required data to calculate mass balances because you have only measured riparian soil solution and stream water. There are no inputs (mineral weathering and atmospheric deposition) or flux from uplands into riparian zones.

Lines 10-11: what do you mean by "too high" and "too low"?

Lines 12-23: too much detail is presented and results are not necessarily justified by the experimental design. For example, if you are calling riparian zone "sites" then there is no replication to test for differences.

Discussion Page 756 Lines 4-9: why are dry, humid, etc. barely discussed? These types of data should be presented in the Results.

Lines 13-14: Highest concentrations of what?

Lines 15-20: So what? How does this relate to forest harvesting or climate change effects in headwater vs. lowland catchment locations?

Lines 19-20: I don't understand the meaning of this sentence.

Page 757 Lines 1-3: What is meant by "remarkable"? Most of the Discussion is a very generic interpretation of the results relative to your site and other studies conducted in

C291

boreal regions.

Lines 4-7: what about mineral weathering vs. decomposition for K?

Lines 20-22: One could argue weathering rates vary across riparian zones.

Page 758 Lines 1-5: you should be able to come up with relative measures by comparing Mg/Ca ratio to that in the literature.

Lines 5-13: Why and what is "transmissivity feedback concept"? Your sample size is not 1,424; that is the number of water samples you analyzed or predicted. You have not defined a statistical design to test for power. The sample size would be based on the number of experimental units (i.e., plots), which have not been identified.

Lines 22-25: what are the ranges and potential causes of uncertainty?

Lines 26-27: why? No statistical justification for this has been made. How did your C7 streamflow estimates compare with those from the referenced studies? C7 is a headwater; where were the other studies located? C7 may not represent all headwaters and certainly not lowland catchments.

Page 759 Lines 1-2: why is using flow-weighted concentrations for a number of riparian zones based on streamflow from one stream better than volume-weighted concentrations from the individual tension lysimeters? What are the associated errors with your method?

Lines 8-9: this is important enough to provide basic statistics in a table.

Lines 14-22: how did measured vs. modelled concentrations compare? What were the fluxes and how did they compare to modelled results?

Lines 23-28: these are predicted values, which may not be consistent with field studies and based on the data presented, there is no way to compare. Would more variation in R7 be important in forest catchment management? Why can underestimation be attributed to Zmod, Gw?

C292

Page 760 Line 5-7: I think many assumptions have been violated in your study.

Lines 8-17: how does this relate to previous field studies?

Line 15: this could be stored water that is mobilised during rain events.

Lines 25-28: what is good? You presented no measured base cation flux from field to compare modelled results. Riparian zones are not always near saturation, especially in headwater catchments. Constant concentration in shallow soil assumption is not valid because base cation concentrations in surface soil horizons are quite variable spatially and temporally.

Page 761: Line 1: why would this occur and what is its importance?

Lines 2-5: no calibration or validation of RIM was reported; therefore you cannot validly test uncertainty.

Lines 7-12: those are not mass balances, but only a model predicted concentration. Because no field and model data comparisons were made, nothing has been demonstrated.

Lines 13-15: if Mg/Ca signal is indicative of mineral dominated soils, it could be from groundwater originating upslope. This cannot be separated from riparian zones based on the data presented in the paper. Also, do all riparian zones transport water and base cations to streams?

Lines 15-19: no comparisons were made to evaluate model performance in estimating flow-weighted concentrations. Was the flux higher because of larger depth of runoff? What about snowmelt. Large precipitation events following dry periods? What about possible base cation depletion?

Lines 19-27: what is the importance to intensified forest harvesting and climate change?

Page 762 Lines 1-8: your comparison used model predictions and was not calibrated or

C293

compared to field-based measurements. Seeing this is the first attempt to incorporate base cations into RIM, you may want to focus the paper on either model calibration or its use to identify key ecosystem uncertainties requiring additional research. In either case, I have issues with how the flow-weighted concentrations were calculated and not compared to volume-weighted base cation concentrations or their fluxes.

Lines 9-13: is water transport from riparian zones diffusive or advective? I think nothing has really been supported because no field data are presented to compare model flow-weighted Mg/Ca ratio or upslope contributing area.

Page 763 Lines 13-14: I disagree because riparian zone contribution to streamflow and chemistry has been studied since the 1970s. The problem has been lack of acceptable methods to partition catchment source areas of streams.

Lines 23-24: you have measured flow for only one stream, which was in the headwaters and may not be applicable to larger order streams.

Lines 26-28: I am not sure what you are trying to say here.

Page 764: Lines 1-2: what about weathering rates? Temperature, water content, and topography affects base cations too.

Lines 3-8: I would argue the non flow-weighted concentrations are not as dangerous to use as those produced from a non calibrated model.

Lines 10-14: what about other areas of the boreal region?

Lines 15-24: although concentrations may be lower in surface soil solution, fluxes may be higher because of more runoff.

Lines 27-28: why would boreal catchments be vulnerable to forest management? What are some of the possible forest management influences?

Tables

C294

Table 1: what is the cut-off for peat proportion of catchment that does not influence base cations? Why are not riparian zone characteristics presented?

Figures

Figure 1: it is doubtful that discharge from C7 can be assumed to resemble that in higher order catchments. Why not stick to the sub-catchments in the insert of Fig. 1? Measured streamflow from C7 may be easier to swallow for estimating flow-weighted concentrations in the riparian zones and streams.

Figure 2: why are there inconsistencies between measured and modelled groundwater tables during winter and spring (and sometimes into summer)? This may be important to your conclusions.

Figure 4: you may want to provide summary of 'mineral' versus 'organic' riparian soils. Were soil characteristics at R11 and R14 different from the other riparian zones?

Figure 7: are these measured or modelled concentrations? I thought discharge was measured at only one stream (C7)?

Figure 8: why different scales on the x-axis? Comparisons would be easier to make if the scales were the same.

Interactive comment on Biogeosciences Discuss., 10, 739, 2013.