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Biogeosciences Discussions

Interactive comment on "Riparian zone processes and soil water total organic carbon (TOC): implications for spatial variability, upscaling and carbon exports" *by* T. Grabs et al.

Anonymous Referee #2

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The authors describe the results of a study of the spatial variation of TOC concentrations and fluxes in subsurface waters of the riparian zone at the Kryklan catchment in northern Sweden. Overall, the paper is well organized, well written, and the methods and assumptions of the study appear to be reasonable. This is an excellent study of aspects of spatial variation of TOC in a well-studied riparian boreal catchment, and will make a nice addition to the literature on riparian biogeochemistry and relations to landscape geomorphology. The weakest part of the paper lies in a few places where overstatements are made and proper consideration and caution regarding the limitations of this data set and the modeling approach have not been made. But these are generally fairly minor concerns, and overall this is a strong paper.

My comments are organized by first describing those that are greater in scope, and ending with minor suggestions.

Use of the term export – I believe that term export has very specific meaning in the catchment biogeochemistry literature, and refers to export of solute mass via streamflow. Here, the authors use this term somewhat inappropriately to describe what is essentially a modeled flux of TOC across a defined cross-sectional area. So, I much prefer the term flux here, not just as a personal word choice, but because there are many spatial considerations and potential processes that might change this TOC flux before it is exported from the catchment. Additionally, there are transport pathways such as overland flow that are an important part of TOC export from riparian zones, yet were not considered in this study. Furthermore, biogeochemical processes in the hyporheic zone as well as in the stream channel were not considered, and might alter TOC export as well. Secondly, this study did not really explore systematic lateral changes in TOC with distance from the stream channel, another factor that could change the flux from what was observed at the static points sampled here prior to stream export. These concerns are relevant to Section 6.2 of the Discussion as well as in the Conclusion Section at line 19.

Assumption of only lateral flow in the riparian zone – The modeling approach applied here assumes all relevant flow is strictly lateral in the riparian zone. This is not necessarily a bad assumption, but there are many published studies that demonstrate upward flow of somewhat deeper groundwater, especially as one gets closer to the stream channel. And of course the classic conceptualization of groundwater – stream water interactions shows upward curving flow lines delivering water to the stream bed—-so called “Tothian” flow if I remember correctly from hydrogeology class. It is important to remember that there are different combinations of mixing that could yield the concentrations and export of TOC measured in streams. Some combination of deeper groundwater with low TOC, shallow groundwater with high TOC as measured in this study, and finally overland flow from saturated areas that might have even greater TOC

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concentrations. Again, it is important to acknowledge here that you have a specific conceptualization of lateral shallow flow as being most important for transporting TOC to streams, and you have in this study ignored the upwelling into the stream of deeper groundwater, which may be part of a more complete understanding of stream TOC export.

The limits of a two-dimensional conceptualization – I agree that this work clearly shows much heterogeneity of TOC profiles with depth in subsurface water. The implications of this work are that models based on lumped representations of the riparian zone may be inaccurate as the authors' state. But it is important to remember that this study did not systematically explore the third dimension going from upland areas across the riparian zone towards the stream. In soils with abundant shallow organic matter as here, and lateral flow towards the stream, one would expect TOC to increase during transit. So, even though the work reported here is a great improvement in riparian spatial conceptualization, this other dimension towards the stream was not studied and may be another important consideration when modeling TOC in subsurface waters of riparian zones.

Minor points—

o Page 6, line 4 – not familiar with term "paludification" of soils here and throughout the text. Is this a well-known term for journal audience?

o Page 12, line 26 – "Spearman" should be capitalized.

o Page 13, line 11 – sometimes minor predictor variables can be significant in multivariate regressions, even if these variables are not major stand alone variables in bivariate regressions.

o Page 17, line 14 – need comma between scatter and points.

o Page 21, line 9 – change "to" to "of" so would read vicinity "of" streams.

o Page 21, line 10 – slight changes might affect hyporheic dynamics short of the greater

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changes that might be expected with a full reversal of flow direction.

o Page 24, line 6 – lumped representations of riparian zones at catchment scale are probably inadequate, seems to be true at Kryklan, but can this statement be made as generalization? Supporting evidence from other studies?

o Page 25, line 6 – should capitalize Minnesota.

o Page 25, line 15 – should capitalize Svartberget.

o Page 27, line 6 – should capitalize Fennoscandian.

o Page 30, line 10 – should capitalize Wisconsin.

o Page 30, line 15 – should capitalize “S”.

o Page 30, line 23 – should capitalize Swedish.

Interactive comment on Biogeosciences Discuss., 9, 3031, 2012.