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Biogeosciences Discussions

Interactive comment on "Long cold winters give higher stream water dissolved organic carbon (DOC) concentrations during snowmelt" *by* A. Ågren et al.

Anonymous Referee #2

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Agren and colleagues focused their study on dissolved organic carbon (DOC) dynamics during snow melt in a boreal catchment. They describe interesting results obtained from a long term hidro-biogeochemical monitoring coupled to a pluri-annual riparian soil experiment. Undoubtedly, manuscript is of high scientific quality and the manuscript structure (text, tables and figures, cited literature) is excellent and, it constitutes a new contribution to the prolific scientific literature focused on DOC dynamic in northern regions. My concern is about the scientific relevance. The manuscript is similar to that published recently by some of the authors in another journal (Haei, M., et al. 2010). The similarity among the two works is palpable. Title, objectives, hypothesis, study site, data analysis and finally, conclusions are analogous. Therefore, it is my obli-

gation to use all information from Haei paper to understand better your manuscript and to limit, as possible, redundancy among papers. Under this perspective, I understand that the present manuscript should be a step forward with respect to the Haei paper. Therefore authors should use their published manuscript as starting point to reinforce (or refute, why not!) the relationship between winter severity and DOC concentration in stream/riparian strip, providing an alternative/complementary date analysis.

In consequence, the hypothesis that drives the manuscript cannot be the same to that of Haei paper. A future potential reader of this paper will already know the answer.

Authors manage a huge quantity of DOC data (15 year record, with samples collected, at least, at weekly frequency. . ..that means a >750 samples!!). Nevertheless, they extracted, from the data base, only two samples obtained in two specific hydro-biogeochemical instants (DOCM and DOCF). Therefore, their entire statistical analysis is based on a set of 15 DOCM + 15 DOCF samples (<4% of the entire data set. . ..do I am wrong?). Obviously these samples are essentials to understand their catchment hydro-biogeochemical functioning. However, I believe that authors lost the opportunity to perform a more global analysis. For instance, it is excellent the initial analysis of the monthly variability of the logDOC-logQ slopes. It would be really interesting to explore the inter-annual slopes variability. . .. . .These slope values might be an additional "Y variable" for PLS analysis. . .. . ..Perhaps, the logDOC-logQ slopes from data collected during snowmelts proceed by a "severe winters" are higher (i.e. severe flushing) than that collected after a "soft winters". . .. .Analysis and discussion of these results would be a substantial step forward with respect to the Haei paper.

With respect to the PLS, in this manuscript the predictors are more or less the same to that used by Haei (31 variables vs. 29). In consonance with my previous comment, in this manuscript the analysis should star by using at the beginning the significant 12 "X-variables" found by Haei. Perhaps these significant X-variables might help to identify new and more specific predictors. For instance: variable "Number of days with below-zero air temperature in the preceding meteorological winter" is (after subtracting

hydrology) the most significant predictor in Haei paper (and in the manuscript as well). With this previous information is it possible to attempt to dissect this predictor: For instance identifying the “Number of consecutive days with below-zero air temperature” (this is simply an example. . ..is possible to identify predictors ad infinitum. . ...).

Below I inserted some additional and more specific comments.

Pag. 4861, line 9. The study site coordinates are different from that Haei et al.. check it!

Authors selected two “Y variables” (DOCM and DOCF) obtained during two keys moments. Nevertheless, authors should explain and justify in more details reasons that motivate their selection. In addition they should reveal if DOCM and DOCF covaried or not (from fig 4a I suppose that the relationship is not really consistent. . ...but it should be described and discussed). Furthermore, in Fig. 4b it appears (more or less..) that DOCF residuals are typically negative, while that of DOCM are typically positive. . . Why?.....Is it coherent this tendency?

Pag. 4894 eq. 2: In my opinion the formula is wrong and it should be rewritten. The model is the derivation of a simply exponential decay (dC/dy=-f C0) and its analytic solution is: Cy= C0eˆ(-f y)

where “y” is the depth and C0 is the solute concentration at y=0.

Pag 4867 line 14: It is difficult to individuate clearly the variables “length of the spring flood (days >1mm) and the "amounts of discharge (mm) during the rising limb” in table 1: “amounts of discharge (mm) during the rising limb”=” Discharge from the onset of snowmelt until the peak-flow (mm)” ? “length of the spring flood (days >1mm)” = ?? What does “days >1mm” mean?

Pag. 4867 line 25 (and table 1) the term “metrological” sound to me unusual. What does mean? In any case, the variables “Length of the metrological winter” and “Number of days with below-zero air temperature in the preceding meteorological winter” appear

describe a similar variable. . ...do they covaried? Due to the relevance of length and severity of winter in this study it should be explained in detail difference and/or similarity among the two variables. Furthermore, the facts that the “delayed soil frost thaw” is the third most important variable appear obvious because is a consequence of a long and severe winter (i.e. “Number of days with below-zero air temperature. . .”).

Fig. 5. In the PLS weight plot the label “w*c[1] appears on both axes. Is it correct? The data cross perfectly a hypothetical 1:1 line. . .. . ..therefore a x-y plot is unnecessary and it should replaced by a histogram or a table. On the other hand, I strongly miss a plot illustrating the relationship between long severe winters and DOMM (or DOMF) concentrations (or residual after subtracting the RIM output).

Table 1: what is the difference between “Start soil frost thaw” and “start data of snowmelt”?

Reference

Haei, M., M. G. Oquist, I. Buffam, A. angstrom gren, P. Blomkvist, K. Bishop, M. Ottosson Lofvenius, and H. Laudon (2010), Cold winter soils enhance dissolved organic carbon concentrations in soil and st ream water, Geophys. Res. Lett., 37, L08501, doi: 10.1029/2010GL042821

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