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# *Interactive comment on* "Can the heterogeneity in stream dissolved organic carbon be explained by contributing landscape elements?" *by* A. M. Ågren et al.

**A. M. Ågren et al.**

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Response to Interactive comment on "Can the heterogeneity in stream dissolved organic carbon be explained by contributing landscape elements?" by A. M. Ågren et al. Anonymous Referee #2 Received and published: 24 December 2013

Overall comments: This paper makes use of an impressive series of measurements to explore controls on DOC dynamics in a small Swedish catchment. While I was initially quite excited to read the manuscript, my impression upon completing the paper is that it vacillates between two goals, without fully realizing either: namely (1) to use the measurements to model DOC concentrations throughout the Krycklan catchment,



and (2) to use the shortcomings of the model to understand landscape controls on DOC concentration. After reading the paper, I wonder if the authors initially aimed to accomplish goal (1), and then after realizing that the model is not fully capable of doing this, progressed to goal (2), without fully modifying the text to reflect this switch. The paper contains some very useful data, and potentially useful analyses. My suggestion would be to choose to pursue either goals (1) or (2) more fully, and edit the paper and strengthen the analyses/discussion to reflect this. For example, to fully realize goal (1), can the information gleaned from the residual analysis be used to solidify the predictive model? To fully realize goal (2), a more detailed analysis and discussion of where the model shortcomings lie, and the reasons for this, should be undertaken. The main conclusion of the paper seems to be that DOC acts relatively conservatively during high flows, but is non-conservative during baseflow conditions, with potential for significant contributions to streamflow from groundwater sources. I wasn't surprised by this finding, but do think that a more nuanced consideration of the model shortcomings, through a more complete residual analysis, or more detailed consideration of the results in the discussion, would be very useful.

We would still claim that we are working with two goals in mind. We have now modified the discussion and strengthened both goals, however, we have also more clearly focused the discussion towards the residual analysis, which we felt was the most interesting part. Goal 1: We argue that when and where the model performs well, it can be used for predictions. Regarding goal 2, "to use the shortcomings of the model to understand landscape controls on DOC concentration". We can see the point, as this paper in a way raises more questions than it answers by ending with the residual analysis and discussing probable causes for the model failure during baseflow. This was as far as we could get with this synoptic survey dataset, our understanding or rather lack of understanding, pointed out by the residual analysis in this study made us continue the line of thought and answer the questions asked here: how important are in-stream processing and changing flow-paths for the DOC concentrations. For this second study we used another dataset (long time series of a few of the sub-catchments). In the second study (Tiwari et al., 2014) we could show that the in-stream processing of DOC within the stream network was low (<1 mg L-1) but the changing flowpaths was an important control on the DOC concentrations. During baseflow 80% of the water draining the outlet of the stream originated from deeper groundwater flowpaths.

We have now included this information into this manuscript and rewritten the aims and discussion to focus it better.

Specific comments: Page 15925, line 28: "except for the February 2005 data" is repeated text.

Thank you, we have now fixed this.

Page 15926, lines 13-21: "the strength of this approach": Given that the main conclusion of the paper is that mixing models can only predict DOC concentrations accurately under certain, specific, circumstances, I urge caution with this statement and the text that follows, and would suggest re-wording significantly. The text here reads as if Fig. 1 is showing true in-stream concentrations, when in fact the evidence that follows indicates that modelling in this way will often create spurious results.

We now write: "This shows the strength of this approach during a time when the model performed well and could be used for prediction. With this approach DOC concentrations can be modelled throughout an entire stream network based on a few headwater observations."

Page 15926, lines 25-27: This is just as likely to reflect the fact that downstream processing is important during baseflow conditions; ie, that by including larger catchments (all measurements) in the model construction, you are more accurately capturing the in-transit degradation of DOC.

Yes, this has been acknowledged in the text: "indicating that the original construction dataset sites were not representative for this occasion."

Page 15927 and 15928 (Residual analysis, sub-models): A few things that could use



refinement: how, exactly, do you define 'significant variables with a high weight'? Is there a specific metric for significance that is being used? If so, is it the same for the high and low flow models?

This has now been clarified: "The models were refined to find the best predictor variables, this was based on the conditions that the variable coefficient should be significant (95% confidence interval) and the variable importance of the projection (VIP) should be high (>1)." This is also more clearly explained in the methods section 2.9.

(2) Once the significant variables have been selected, is the model being re-run to include only these variables? Please clarify.

Yes, we now write: "The models were then rerun on the selected variables to create two refined models (Fig. 8A & B)."

(3) Please provide more detail on what R2Y, R2X and Q2 represent, for readers unfamiliar with this specific model type. In particular, the difference between R2X and R2Y should be clarified beyond the information provided in lines 27-1.

We now write: "R2Y and R2X are goodness of fit measures. That means that 57% of the variability in X was used to explain 35% of the variability in Y. Q2 is an estimate of the predictability of the model. It is calculated by cross-validation and resembles R2 in regression models where 0 is poor and 1 indicates optimal predictability."

Pate 15930, lines 4-7: I agree that this is a good way to take this paper. However, if this is indeed the focus, then the paper should be reworded throughout to clarify this. Until I reached this point, I was under the impression that the purpose was to (1) try to predict-in stream DOC, and (2) use the residuals to help to refine the model. In addition, if the main purpose is to use residuals as a diagnostic, then more should be done with the residual results in the discussion component of the paper.

Yes, we have now more clearly focused the discussion towards the residual analysis, which we felt was the most interesting part.

Page 15930, lines 15-22: Arguably, DOC is never truly conservative, but will mimic conservative behavior under high flow conditions when in-catchment and in-stream transit time is short enough that degradation is sufficiently minimized. Therefore, it's likely that the relatively good model results that you get at high flow are caused both by the hydrologic connectivity at this time (as described), and a more 'conservative-like' behavior of the DOC.

Yes, we have now clarified this in the discussion. We now write "It is also likely that the relatively good model results during high flow are caused by a more 'conservative-like' behavior of the DOC due to the shorter in-channel residence times of the stream network (0.5 days) at high flow (Tiwari et al., 2014). "

Page 15932, lines 15-18: I don't think you can be so definitive in this conclusion, given that the analysis in Fig 8 explains only a fraction of the variability in the low flow residuals.

We now support that statement citing a companion article (Tiwari et al. 2014) that shows that in-stream processing is not a major control of DOC in this catchment. This information has now been inserted into the discussion.

Page 15932, lines 22-24: Differences in specific discharge spatially throughout the catchment? How does this affect your assertion on page 15918 that it's reasonable to assume that specific discharge does not vary throughout the catchment for the purposes of the model?

We have changed the wording in the methods section (p. 15918) to: "We make the simplifying assumption that the specific discharge is the same throughout the catchment" and we state that that assumption introduces uncertainty, especially during low flow. We then show that the assumption does indeed fall short of representing reality, and that it is important to understand changing flowpaths and the seasonal dynamics when modelling DOC in meso-scale catchments. This is now more clearly stated in the discussion.



Tables 1 and 2: could the true discharge also be provided, for comparison purposes?

We are not sure what the reviewer means by true discharge. The specific discharge is given in table 1, we believe this is a better measure than giving it in L s-1, as that measure depends even more on where in the catchment you measure (small or a large catchment).

At site 7 (where Q was measured for this study) the min discharge 0.06 mm day-1 equals 0.37 L s-1, while the same discharge 0.06 mm day-1 at the outlet equals 279 L s-1.

Figure 3: The text in the caption could be refined slightly for clarity. I find "variability of the different soil coverage" to be unclear. Perhaps "percent coverage of each landcover type in the construction and validation datasets", or similar?

We now write; "Fig. 3. Boxplots showing the percent coverage of each landscape type in the construction (n=15) and validation datasets (n=100). "

Figures 4 and 8: I find the contrast between these two figures to be interesting, and think this could use some discussion in the text. For the low flow models, it's clear to me that the regression model is always underestimating DOC in peat-rich catchments (hollow circles), and the PLS residual analysis reflects this. In the high-flow models, there does not seems to be a strong estimation bias for these same peat-rich catchments, which scatter fairly well around the 1:1 line in Fig. 4. This lack of strong importance of peat for explaining residual variation seems to play out in joint model from Fig. 7. Could some description of the reason for peat becoming a strong explanatory variable for the residuals of the separated model in Fig. 8a be explained briefly in the text?

We now discuss this in the first two sections of the discussion.

That concludes our answer to the interactive comment.

Anneli Ågren and coauthors

References Cited

Tiwari, T., Laudon, H., Beven, K., and Ågren, A. M.: Downstream changes in DOC: Inferring contributions in the face of model uncertainties, Water Resour. Res., n/a-n/a, 10.1002/2013wr014275, 2014.

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Interactive comment on Biogeosciences Discuss., 10, 15913, 2013.