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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.

Anonymous Referee #2

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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 sug-

gestion 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.

Specific comments:

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

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.

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.

Page 15927 and 15928 (Residual analysis, sub-models): A few things that could use refinement: (1) 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? (2) Once the significant variables have been selected,

is the model being re-run to include only these variables? Please clarify. (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.

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.

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.

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.

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?

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

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?

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?

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