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Interactive comment on "Partial coupling and differential regulation of biologically and photo-chemically labile dissolved organic carbon across boreal aquatic networks" *by* J.-F. Lapierre and P. A. del Giorgio

Anonymous Referee #2

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The authors have not addressed my initial concerns in their response regarding the manuscript's layout, experimental setup and methodology used to derive their conclusions.

The organisation of the manuscript is confusing with the sections of discussion appearing in introduction and results. For a more reader-friendly layout, I suggest moving most of the discussion points to the relevant section or combining the results with the discussion and restricting the contents to the main points of interest.

I suggest adding a more detailed analysis of large-scale spatial trends of OM; at the

moment, from provided Figures and Tables, only the classification aspect is clear (wetland, lake, river). How do environmental conditions and OM properties change within given classes? It is not clear how environmental conditions appear to play a role in determining the DOC quantity and quality. How the terrestrial gradient was quantified/assessed? The PCA figure suggests that the discrimination between classes is not perfect. Why and how is this important?

The authors state that: 'We are aware that there is some fluorescence lying at wavelengths < 270 nm. It is well known, however, that most fluorescence components identiïňĄed by the PARAFAC modeling process have two peaks in excitation, and at least one of the peaks will always be measured at wavelengths > 270 nm.' The double excitation maxima peaks for Tryptohan- and Tyrosine-like fluorescence have been shown in the literature to behave differently in terms of their reactivity, correlations with nutrients and water quality parameters. Large quantities of FDOM exc <270 nm are both bio- and photo-degradable thus crucial to understanding the links between the two OM fractions.

The only reason that authors excluded this region in their analysis is 'Fluorescence was measured below those wavelengths but the rather high signal to noise ratio affected the performance of the PARAFAC model, hence we removed them.' – if the model does not cope well with the data, perhaps the authors should consider using another model? At the moment, the model's limitations make the analysis partial. This serious limitation of the current approach should be addressed or at least acknowledged in the paper. To assess the uncertainty of the approach the simplest procedure is to quantify the amount of protein-like signal in the lower excitation wavelengths (not analysed in the study) and compare with the protein-like signal in the higher excitation wavelengths (analysed in the study).

The methodology - one can argue indeed that referring to the authors own words 'PARAFAC modeling, moving window regressions and model comparison are state of the art analyses techniques in empirical studies of DOM dynamics'. However, these

techniques have to be applied correctly and their limitations should be acknowledged. Principal components analysis, multiple linear regression analysis and coefficient of determination R2 do not prove causal links between variables. They only indicate some relationship between variables which is also not conclusive for the low R2 values reported in the study (0.35 to 0.70). The authors say that 'r2 ranging from 0.35 to 0.70 may appear low to researchers used to work in smaller and more homogenous sets of systems, but again the value of these relationships cannot be judged on the r2 alone'. If they cannot be judged alone, perhaps the authors could provide additional evidence? At the moment, the arguments based on these low R2 correlations are mere speculation. The potential colinearity in the dataset should also be assessed.

Interactive comment on Biogeosciences Discuss., 11, 6673, 2014.