

Interactive
Comment

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

Received and published: 12 June 2014

The paper aims at evaluating the basin-scale drivers of biodegradable and photochemically reactive DOM pools.

However, the paper promises much more than it delivers. The dataset comprises a large number of environmental samples covering a vast geographical area and thus gradient in DOC quantity and quality. However, the statistical analysis of this robust dataset is fairly basic and by any means not exhaustive. The paper contains several speculations about the causal links between different variables that are not supported by the statistical methods used. The authors tend to oversimplify and over interpret

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



complex relationships between the variables.

The focus of the paper is confusing. The authors claim that the main focus is on large-scale patterns in bd and pd-DOC not on the estimation of both fractions. However, the former aspect of the study is not evaluated critically enough to support such a claim. Analysis of environmental conditions is restricted to classification of source waters (river, lake, wetland) and links between DOM biodegradability and photochemical degradation inferred from simple (and not particularly strong) correlations with other chemical determinands (TN, TP). The evaluation is based on assumptions and speculations e.g. Page 6689, lines 25-29 and Page 6690, lines 1-2. TP is hardly an indicator of biological activity without information on the percentage contribution of soluble reactive phosphorus.

One of the major flaws of the experimental setup is lack of measurements of low excitation wavelengths <270 nm; these spectral regions contain a large proportion of FDOM that is both photochemically reactive and biodegradable.

The paper is poorly organised with the parts of discussion appearing in all other sections (Introduction, Results). Some parts of the discussion are not relevant to the main topic of the paper e.g. page 6687, lines 14-24, as the paper does not focus on the determination of the age or freshness of DOC in water samples. Similar, conclusions (Page 6695, lines 9-11).

The authors confirm what is already known from previous studies, for example studies of Andy Baker and co-workers who correlated BOD with protein-like fluorescence. Simple and not very strong correlations between variables do not confirm the causal links between DOM character and its behaviour in environment. The results of this study do not support conclusions drawn by the authors on the large-scale patterns of DOC biodegradability and photochemical reactivity.

The authors should critically evaluate their results, their significance and appropriateness of their experimental setup and rewrite the discussion section, largely by remov-

BGD

11, C2393–C2396, 2014

[Interactive
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



ing all the speculation and assumptions not supported by the data, to reflect the study itself.

Specific comments:

Extensive parts of the introduction e.g. page 6676 lines 7-30 and page 6677 lines 15-20 are a discussion of the results and therefore should be shortened/moved to the discussion section.

Experimental setup – why fluorescence measurements were constrained to 275-450 nm excitation wavelengths? If the aim of the study was to characterise biodegradable DOM, the authors should have considered analysing lower excitation wavelengths ~225-230 nm. Large proportions of protein-like, biodegradable DOM lie in this region. Likewise photochemically reactive DOM of humic-like origin lies in this region. Thus the result that component C3 was the strongest predictor of Pd-DOC can simply result from not incorporating lower excitation wavelengths in this study. This serious limitation of the study should be discussed and the results can be significantly biased.

Page 6678 – TN concentrations are not reported, line 6.

Page 6678, line 8 – should read Strahler order.

Page 6678, line 23 – please rewrite the sentence ‘TN was analysed as nitrate’ to correctly describe how TN was calculated.

Results – authors should separate results from their discussion e.g. pages 6681-2, lines 23- 3 and entire section 3.7.

Results, page 6682, lines 10-16 and Figures 2 and 3: I am not convinced that the relationship between concentrations of Bd-DOC and Pd-DOC is meaningful. The absolute concentrations of both fractions simply increase with DOC concentration and this relationship should not be over interpreted to infer DOM functionality.

Page 6686, lines 10-11 – this is speculation.

BGD

11, C2393–C2396, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Page 6688, lines 9-14 – this discussion is redundant and irrelevant, as the authors analyse summer samples only.

Page 6691, lines 6-10 – light climate? This a sweeping statement.

Page 6691, lines 10-15 – this is speculation.

Page 6694, lines 12-16 - this is speculation.

Page 6695, lines 3-8 – this is speculation.

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

BGD

11, C2393–C2396, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C2396

