

Interactive comment on “The composition and flux of particulate and dissolved carbohydrates from the Rhône River into the Mediterranean Sea” by C. Panagiotopoulos et al.

Anonymous Referee #1

Received and published: 12 February 2012

General comments

In the manuscript entitled “The composition and flux of particulate and dissolved carbohydrates from the Rhône River into the Mediterranean Sea”, Panagiotopoulos et al. used measurements of DOC, POC, TSM and particulate and dissolved carbohydrates for the Rhône River obtained monthly for the period 2007–2009. The measurements were used to calculate corresponding fluxes to the Mediterranean Sea. The composition of particulate and dissolved carbohydrates was also investigated in order to provide insights on the origin of carbohydrates in the Rhône River and on the diagenetic state of POM and DOM. Overall, the manuscript is written in proper English and presents novel

C5687

data worthy and appropriate for publication in BGD. However, I think some changes need to be made about the overall organization of the paper. I also think the current version of the manuscript is too long and convoluted to effectively convey the most interesting aspects of this research. The main arguments are not presented as clearly as they should be. In my view, the organization of the “Results” and “Discussion” needs to be adjusted. Part of the “Results” reads like a “Discussion” and vice-versa. Technically, figures and tables should not be referenced to in the Discussion. I suggest that the authors present the results shown in figures and tables in the “Results” section only, following the order in which the figures and tables are presented. In the discussion, the results are used to support the arguments made in the discussion and are put in the perspective of other works. The manuscript also presents an overwhelming number of citations. In my view, these could be cut down a bit because this is not a review paper. The Methods also, tend to be lengthy and overly detailed and could be written more concisely. I think the manuscript needs to be written more concisely and be properly organized.

Specific comments

Tables: The tables are nice, clearly presented and very informative. However, I was wondering why the tables are not numbered followed the order in which they are presented in the results. Figure 4 should be Figure 2.

Figures: I question the need for figure 1 since this information is already presented in Table 1. A map of the Rhone river + sampling site showing a time-series of river discharge could be a nice replacement.

1) The sampling station is located about 50 km inshore, before the Rhone river actually flows through the Camargue region. Potential interaction between the river and the very productive marsh could potentially alter the estimated fluxes, especially during flood events. Is there any evidence that concentrations are not significantly different between Arles and the mouth of the Rhône River? Does the River interact much with

C5688

the marsh (is the river levied?) (readers may not be familiar with this area). It would good to add some comments about this. If no evidence is available, then this problem should be clearly acknowledged.

2) I understand that the PCA is used here primarily to look at changes in the carbohydrate composition of DOM and was performed using the concentrations of carbohydrates in POM and DOM. Most of the variance in the data set is carried by PC1, which simply reflects the change in the overall concentrations of carbohydrates (as is indicated by loadings approximating the value 1 in fig. 5). I would suggest doing the PCA on the relative abundance of carbohydrates (mol %) such that the principal components are indicative of changes in composition and not concentration.

3) The authors often use the correlation coefficient r to refer to the goodness of fit between linearly related variables. The authors should report the coefficient of determination instead (R^2).

4) In section 2 (Study area and sampling): I think a map highlighting the river and its tributaries, the Camargue delta and the sampling site would provide a useful addition to this description of the study area.

5) In section 3 (Methods): I did not see a method for TSM and for %OC. A one or two-line description would suffice.

6) Please add a sentence or two describing the factor of Ferguson (1987) used to improve the estimated fluxes.

7) I think the relationship $\text{Log}(\text{DOC}) = 0.23\text{log}(Q) + 1.34$ has a R^2 of about 0.19. Please justify the use of this relationship instead of using an average concentration of DOC to estimate the flux.

Technical corrections

In some equation, the author used the napierian log (\ln) (line 15, p11176) and sometimes the simple term (\log) (line 21, p 11177). Does “log” refer to the log of base 10

C5689

here? Please, check for consistency.

Page 11184, Line 13: Change Orinico to Orinoco

Page 11186, Line 20: the word “spectrum” should be replaced by “plot”

Page 11186, Line 26: replace “more than the half” by “more than half”

Last paragraph of section 4.1.4: The word “primarily” is not adequate because the ratio PCHO-C/DCHO-C is about 60%/40%.

In section 5.6, the annual TOC input to the Gulf of Lions is to represent 1% of the standing stock of TOC. It is the said to be 2% in the Conclusions.

Interactive comment on Biogeosciences Discuss., 8, 11165, 2011.