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Biogeosciences
Discussions

Interactive comment on "Seasonal variations in concentration and composition of dissolved organic carbon in Tokyo Bay" *by* A. Kubo et al.

Anonymous Referee #1

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In this manuscript the authors investigate the bioavailability and degradation rates of dissolved organic carbon (DOC) in rivers flowing into and seawater collected in the Tokyo Bay, Japan. They find correlations between both bioavailable and recalcitrant DOC with salinity and chlorophyll, and suggest that recalcitrant DOC in this system is mainly of oceanic origin. Lastly they also compare their results with measurements made 40 years ago and they suggest that the DOC pool has become more recalcitrant over this period.

The strong points of this work are 1) that samples were collected both in rivers and marine waters and 2) the seasonal sampling data. Overall I think this is a very nice manuscript with several important findings regarding the degradation of DOC in coastal waters. However, I do have several general and specific comments that should be

addressed and/or considered before the paper is published and therefore I recommend a major revision of the manuscript before it can be considered for acceptance.

Comments: Generally the quality of the English writing needs improvement; I recommend getting a native English speaker to read the manuscript before you submit a revised version

Abstract: Line 1-6: Be more specific and concise. Line 6-10: Please join these two sentences. Line 13: Use "concentration" instead of "abundance", and specify what causes the higher levels during autumn and winter.

Introduction: Page 10204 Line 23-25: Combine these sentences. Page 10205 Line 3: Is this "open ocean" or "coastal waters"? Line 17: Use "measured" instead of "observed"

Material and methods: Page 10206 Line 6: How long time after collection did you start the filtration? Line 10-16: Was a microbial community added? Or did you assume that microbes passing the GF/F filter did the job? Did you filter your DOC samples? Were initial inorganic nutrient concentrations measured either in the field or your incubations? Were nutrients likely limiting the microbes? Line 16-18: Why did you store the samples differently? Line 18-20: How did you ensure the consistency of your DOC measurements? Were DOC reference samples used? Line 21: Rephrase to "RDOC was here defined as . . ..". Page 10207 Line 1: I guess BDOC was calculated as the difference between initial and final DOC concentration, and RDOC was defined as the end concentrations, right?

Results and discussion: This section needs to be more precise as currently various ideas/results are discussed more than once. Page 10208 Line 5-10: These concentrations are very low, is this due to e.g. a large input of groundwater? How did you verify that these low concentrations were correct? Line 24: Please explain what a "secondary treatment" is? Page 10209 Line 23: If you consider the variation in your rates and the standard deviation of your average, this value is not statistical different from the average reported for coastal waters in the cited paper. Line 29: The methods used by Ogura (1975) were different from the ones you used (e.g. He used GF/C filters and a wet oxidation to measure DOC) this should be mentioned and the likely implications of these differences for the comparison with your results needs to be discussed. Page 10210 Line 5: The rates reported by Ogura (1975) (measured at 20°C) are generally lower than the rates you report in table 4 (at 20°C), following I don't agree with this statement. Line 13-24: This section should be rewritten and shortened, currently it is very difficult to read. Page 10211 Line 9: Previously you mentioned that the DOC variations were controlled by BDOC, so how does this fit with your previous statement? Line 12-13: What does this suggest? Please explain. Page 10212 Line 2-3: This has already been mentioned previously, so there is no reason to repeat the same idea. Line 4 to page 10213 line22: The multiple linear regression approach is very nice, but I am not convinced by the validity of the individual functions proposed in this section. The assumptions made are very rough (E.g. I don't believe our assumption of 0 μg L-1 chl a in the open ocean), so I suggest that this part is shortened and only the multiple linear regression approach is reported. Also why is the same type of relationship not shown for BDOC? Page 10213 Line 10: How can a value within 3 times the standard error be reasonable? Line 13: I am not convinced by this statement. In order to conclude this you should e.g. construct a biogeochemical box model or use a mass balance approach. Following I suggest deleting this part of the discussion.

Section 3.2.3., this section should be combined with your previous discussion of the Ogura (1975) data.

Tables/Figure Table 1: Report the standard deviations of your DOC and POC concentrations. Table 2 and 4: Report the standard deviations of your KDOC estimates. Figure 3: Delete "concentrations" after salinity in the figures. Figure 4: Show the standard deviations of your DOC values. Figure 5: Show the standard deviations of your DOC, BDOC and RDOC values. Figure 6: I suggest deleting this figure as I do not trust the approach used.

Interactive comment on Biogeosciences Discuss., 11, 10203, 2014.