

Interactive comment on “The photobleaching as a factor controlling spectral characteristics of chromophoric dissolved organic matter in open ocean” by Y. Yamashita et al.

Anonymous Referee #1

Received and published: 4 August 2013

GENERAL COMMENTS This is a well-written manuscript that will be of interest to a broad range of marine and aquatic chemists, as well as specialists in the fields of bio-optics, remote sensing and DOM characterization. The scientific questions addressed in this paper are timely and receive here an appropriate level of both empirical investigation and discussion. The scope subject and quality are appropriate for BIOGEO-SCIENCES. With little exception, the conclusions reached are appropriate given the results here and in cited works. I recommend the paper for publication pending minor changes and grammatical/typographical corrections.

SPECIFIC COMMENTS 1. I would recommend removing “The” from the beginning of the title. 2. The Introduction section is quite good except for some minor edits (see

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below). However, I think that it would be appropriate to add a sentence or two about two very recent papers (Helms et al. 2013. Marine Chemistry v155: pp81-91 and Stubbins, et al. 2012. Biogeosciences v9: pp1661–1670) dealing with very similar themes. Alternatively, these could be discussed/cited in the Discussion section. 3. In the last two sentences of section 3.2 (I think) there are also comparable values given in the two references mentioned in the previous comment (The Stubbins paper may not have any values from <300m, but it's worth a look). 4. P,15 “. . .indicating that major factors controlling the. . .” Is there coastal or terrestrial influence at these stations? If not is there evidence that you can provide that this is the case? 5. P. 15 “. . .spectral slope parameters. . . were not significantly different among sampling periods. . .” It looks like there is a very small (statistically significant?) seasonal difference in S275-295 in the uppermost samples. 6. P. 15 Could the higher levels in the upper 50m be related to terrestrial impacted waters advecting off the shelf? You may be able to rule this out using temperature/salinity data, but you should mention either that it is a possibility, or if it isn't, why not. 7. P.18 “. . .suggesting that the same water mass was distributed for. . .” Is this supported by the physical oceanographic (i.e. temperature, salinity, density) data? If so add a brief description of this evidence. 8. P. 21 “Thus, Sr calculated by S275-295. . . cannot be used as a tracer of photochemical history. . .” I'm not sure I follow your argument here. If (in the so far limited data available) terrestrial waters show a slight increase in S350-400 and ocean waters show little or no change in S350-400 during irradiation, the driving influence of S275-295 on Sr suggests to me that it would still provide information about photochemical history (i.e. higher tends to be more photobleached). Perhaps you should moderate your statement a bit to say that S275-295 may be more appropriate given the relative insensitivity of and possible non-photochemical influence on S350-400. . . 9. Figure 1. Minor point, but if it is an easy fix, you should include the STP station in the key as well as in the caption. 10. Also, looking at Figure 1. . . Is there a larger source of terrestrial OM in the sub-arctic waters than the sub-tropical (i.e. they seem to be closer to the shelf/shore)? Is there a clear trend in surface optical properties as you move further off shore for the sub-tropical stations? If

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these are all fully open ocean sites, you should state it clearly and support it with data or a reference, otherwise simply mention briefly that the sub-arctic samples may have more terrestrial influence. I don't think it weakens or invalidates your conclusions.

TECHNICAL CORRECTIONS

Page 2, third and eighth sentences: change "...CDOM at UV region..." to "...CDOM in the UV region..."

Page 2, seventh sentence: "increases and unchanging" is awkward revise sentence.

Page 3, first sentence: add the before open ocean.

Page 3, second sentence: delete "It has known that", change "to be controlling" the to "controls", add a comma before "which", change "primary, microbial production" to "microbial primary production", and add "the" before "marine food web."

Page 3, third sentence: change "has" to "is" and delete "highly."

Page 3, second sentence of second paragraph: add "the" between "However," and "major" and delete "to be related."

Page 3, third sentence of second paragraph: delete "as" from between "considered" and "biologically."

Page 4, second line: add "the" before "open ocean."

Page 4, second paragraph: change "the indeces of molecular weight of DOM and" to "an index of DOM molecular weight and/or", and change "retrieve method" to "retrieval method"

Page 5, first sentence of second paragraph: change "is basically" to "are basically."

Page 5, third sentence of second paragraph: change "surface waters (~300m)" to "the upper 300m"

Page 5, last sentence of second paragraph: delete "a" and change "tracer" to "tracers."

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Page 6: it might be useful to include the manufacturer and nominal pore size of GF/F filters.

Page 7, first line: change “evaluating” to “evaluate.”

Page 8, last sentence: change “spectrum was” to “spectra were.”

Page 10, last sentence of 3.1: change “most closely located to” to “near.”

Page 10 first paragraph of 3.2: the use of “accompany” is confusing here; reword.

Page 11, sixth line: add “in the” between “especially” and “upper 100 m.”

Page 11: I think Mats Granskog has also published some S275-295 numbers for Hudson bay area, and you may want to compare your ranges with those reported in Helms et al. (2013) mentioned above.

Page 12, end of first paragraph: see also Helms et al. (2013).

Page 14, end of first paragraph: change “change with” to “exhibit.”

Page 16, last sentence of first paragraph: change “whether” to “either.”

Page 17, first paragraph: I think “solar insolation” is redundant, change to just “insolation.”

Page 17, last paragraph: change “closely located to” to “near”, and (I think) change “a year” to “the year”

Page 18: between the discussion of Stubbins et al (2012) and Ortega-Retuerta et al. (2010), I would mention the recent results from Helms et al. (2013) mentioned above.

Page 22, second sentence of section 5: Helms is misspelled (Helmes)

Page 23: “CDOM at visible region. . . Matsuoka et al., 2007).“ is confusingly worded. I’d recommend revising it as follows: “Measurement of CDOM optical properties have often been focused on the visible region, because it is a long recognized interfering

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factor in satellite remote sensing of chlorophyll and has been previously determined by satellite remote sensing (e.g. Sasaki et al., 2004; Matsuoka et al., 2007).” or similar revision.

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