First of all, our sincere thanks for valuable comments from both referees, which significantly improved the manuscript. Our replies to individual comments are detailed below:

Replies to comments from anonymous referee #1

SPECIFIC COMMENTS

1. I would recommend removing "The" from the beginning of the title.

Thanks for the comment. We have removed "The" from the beginning of the title.

2. The Introduction section is quite good except for some minor edits (see 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.

Thanks for your suggestion. We have added a sentence regarding with these papers to the revised manuscript (page 9993, lines 14-15 in the original manuscript).

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).

According to this comment, we have compared our $S_{275\text{-}295}$ and S_R values with those reported in Stubbin's and Helms's papers in the revised manuscript (Page 9998, lines 6-10; Page 9998, lines 21-25 in the original manuscript).

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?

The water masses in the subarctic region were characterized as Oyashio water from temperature. The Oyashio is the western boundary current of the western subarctic gyre in the North Pacific. Thus, our sites in the subarctic region could be characterized as a part of the western subarctic gyre in the North Pacific. However, Oyashio water is also influenced by marginal sea waters (i.e., the Sea of Okhotsk). Even though there is no information regarding with terrestrial DOM contribution to our study area, $a(320)$ only weakly correlated with salinity in the upper 50 m (R=-0.39, p=0.04, n=29), indicating that contribution of terrestrial CDOM was not a major factor controlling the CDOM level in the upper 50 m. We have discussed this issue in the beginning of section 4.1 in the revised manuscript and added a statement regarding that our sites were located in the western subarctic gyre in the North Pacific (page 9997, line 2 in the original manuscript).

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.

Yes, $S_{275\text{-}295}$ values of uppermost samples in June were slightly higher than those in April and May. We have added this statement to the revised manuscript (page 9998, line 6 in the original manuscript). Because we only have two stations in each month, we can't apply statistical test, thus, we removed "significantly" from this sentence in the revised manuscript (also from other sentences throughout the revised manuscript).

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.

Please see our response for the specific comment #4.

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.

Thanks for the comment. Yes, salinity and temperature were also relatively uniform. According to this comment, we have revised this sentence in the revised manuscript (page 10003, lines 6-8 in the original manuscript).

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 nonphotochemical influence on S350-400…

According to this comment, we have moderated our statement in the revised manuscript (page 10005, lines 24-25 in the original manuscript).

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.

Figure 1 has been revised accordingly.

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 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.

Please see our response for the specific comment #4.

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."

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 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.

Thanks very much for comments and recommended corrections. We have revised manuscript according to all technical corrections the referee kindly provided.

Replies to comments from anonymous referee #2

Specific comments

First, the authors report that absorbance measurements were collected employing a spectrometer equipped with a 10 or 5 cm cell, without any further detail on when each cell was employed. Figure 4 shows that most of a320 values reported in this work are around 0.1 and 0.3 m-1 for Subtropical (ST) and Subarctic (SA) waters, respectively. These values would correspond to an absorbance of 0.002 and 0.0065 (OD) if the spectra were collected with a 5 cm optical cell; and to 0.004 and 0.013 if collected with a 10 cm cell. In either case, the ST waters exhibited absorbance values (at 320 nm) at or below the instrument detection limit of common spectrometers (the detection limit of this instrument is not reported in this manuscript). While the absorbance values for the SA waters are close to the detection limit only when collected with the 5 cm cell. If a320 is already at the detection limit, the absorbance at wavelengths > 320 nm would be even smaller, within the S/N ratio. Thus calculating the S (and thus SR) over the 350-400 nm range would be meaningless, at least for the ST waters. This is clearly indicated by the random distribution of S350-400 (and to some extent SR) along the water column (Figure 4) and during photobleaching experiments (Figure 6).

Thanks for your comments. A10 cm cell was basically used for the absorbance analysis. However, due to limited sample volumes, a 5 cm cell was used for the samples (that contained high levels of CDOM) obtained from photo-irradiation experiments. We have clarified this issue in the revised manuscript (page 9996, line 5 in the original

manuscript). In addition, we have added specifications for our spectrophotometer (UV-1800) provided by the manufacture (photometric repeatability = < ±0.001 OD at an absorbance of 0.5 OD, noise level = < 0.00005). We have also added the results of experiments regarding with analytical errors (triplicate measurements) using two subtropical surface waters with 5 cm cell. The results were as follows; Sample 1: $a(320)$ = 0.108±0.003 m^{-1}, $S_{275-295}$ = 0.0408±0.0004 nm^{-1}, $S_{350-400}$ = 0.0131±0.0004 nm^{-1}, S_R = 3.11±0.08, and Sample 2: $a(320)$ = 0.082±0.003 m^{-1}, $S_{275-295}$ = 0.0459±0.0004 nm^{-1}, $S_{350-400}$ = 0.0164±0.0018 nm^{-1}, S_R = 2.82±0.35. We have added these specifications and experimental results to the end of section 2.4 in the revised manuscript. The analytical errors described above are the largest and errors would be much smaller in the cases of the subarctic or deep ocean samples. Also use of a 10 cm cell could yield better results with smaller errors. Even though we only used the 5 cm cell for photo-irradiation experiments of deep water samples (that contained high levels of CDOM), according to this comment, we have noted that "the results for $S_{350-400}$ and S_R might have relatively large analytical errors" (page 10005, line 11 in the original manuscript) and have moderated our discussion (page 10005, lines 24-25 in the original manuscript) in the revised manuscript.

Second, the effect of two distinct processes (bio and photodegradation) on the S parameters is investigated to gather CDOM history in open oceans. The impact of these processes on S is quite different. However, the waters employed as CDOM source originated from the Subarctic (SA) region (5 m) for the microbial incubations and from the Subtropical (ST) region (400-766 m) for the photodegradation incubations. Is it possible that the impact of these processes on S was so different/biased because the CDOM source itself (ST versus SA) was so different? Can the authors exclude this hypothesis? A more rational experiment would have employed the same waters to investigate different processes.

We agreed with this comment. However, as the first step for determine changes in $S_{275-295}$ and S_R with microbial alternation and photobleaching, it is hard to carry out the photo-irradiation and microbial incubations using the same water. For the better assessment of changes in CDOM along with photobleaching, we needed to use "photo-sensitive" DOM for photo-irradiation experiments. Since surface water samples might be substantially photobleached, we chose the deep water samples for photo-irradiation experiments. On the other hand, we had to use "bio-available" DOM for microbial incubation. Because deep water DOM is mainly composed by recalcitrant DOM, we chose surface water samples for microbial incubation. As pointed out by the referee, a more rational experiment using the same waters is necessary to investigate the different processes. We are going to conduct such experiments for future challenges.

Third, the spectral slope (S and/or SR) is calculated in either case excluding the a320 values employed to investigate the CDOM spatial distribution. Wouldn't be more meaningful to include the 320 nm in the spectral range used to get S? Or, to use a different wavelength to describe CDOM spatial distribution that is included in the range employed to derive S?

We wanted to compare quantity of CDOM in this manuscript with other region of the Pacific to confirm that quantities of CDOM in this manuscript were within the range of the Pacific. Since the basin scale distribution of CDOM in the Pacific has been reported with the absorption coefficients at 320 nm (Yamashita et al., 2009, Limnology and Oceanography) or 325 nm (Swan et al., 2009, Deep-Sea Research part 1), we chose 320 nm as a quantitative parameter of CDOM.

Last, the chemical composition of CDOM cannot be assessed from the spectral slope S because S is not an indicator of chemical structure nor is a chemical test. S is indeed the output of a pure fitting routine. On the other hand, variations in S (as those upon bio or photodegradation) do indeed suggest changes in CDOM composition and can be legitimately employed to address CDOM 'history'. This idea should be clarified in the manuscript.

Thanks for the comment. According to the comment, we have clarified this issue in the revised manuscript (page 9992, lines 23-27 in the original manuscript). In addition, "chemical composition" have been changed to "composition" throughout the revised manuscript.

Technical comments

Combine figures 3 and 4.

Thanks for the suggestion. We have combined the original Figures 3 and 4 for Figure 3 in the revised manuscript.