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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 #2

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General comments

This paper investigates the effect of photobleaching and biodegradation on the optical properties of CDOM in open oceans and proposes to employ the changes in the spectral slope S over the 275-295 range as an indicator of photochemical history of CDOM in open oceans. This paper can be considered for publication once the main issues reported below are addressed.

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).

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.

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?

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.

Technical comments

Combine figures 3 and 4.

Interactive comment on Biogeosciences Discuss., 10, 9989, 2013.