

## ***Interactive comment on “Characteristics of Chromophoric and Fluorescent Dissolved Organic Matter in the Nordic Seas” by Anna Makarewicz et al.***

### **Anonymous Referee #2**

Received and published: 7 March 2018

The manuscript by Makarewicz and colleagues presents combined results from three summer expeditions to the Nordic Seas, where they investigated the characteristics of both colored and fluorescent dissolved organic matter and their correlations to hydrographic variability. The dataset from the under sampled Nordic Seas presented in this study is interesting and of great value providing new and useful information on CDOM/FDOM in the study region. I suggest this manuscript for major revision before acceptance for publication in Ocean Science.

### **GENERAL COMMENTS**

- The manuscript presents three major objectives in the introduction (Lines 113-118),

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however the discussion as well as the abstract and conclusions do not clearly present the answer to the questions that the authors have raised. The authors may need to address those points accordingly through the manuscript. For instance, there is no clear distinction between large and regional circulation processes (objectives 1 and 2) with presenting and/or discussing the findings in the MS. The authors also state in their objective 3 that the impacts of phytoplankton on CDOM distribution (apart from FDOM), but only results concerning the impacts on the FDOM fraction are presented.

- Several figures (e.g. Figs. 2, 3, 4) need to have their labels enlarged.
- The authors mention several times through the text that some correlations or comparisons among years/expeditions were significant. However, the p-values are not given. Authors must present the p-values and also indicate the tests that have been applied.
- The discussion section is superficial. The authors present results from other studies (like a review) and establish comparisons between their results and previous studies. However, no proper discussion regarding the results presented in the MS was performed.
- Although the manuscript is generally well written and easy to follow, there are several grammar issues through the manuscript and I therefore suggest this manuscript to be revised by an English native speaker before submitting the revised version it.

## SPECIFIC COMMENTS

L. 83: what do the authors mean by efficient? Please, be more specific, otherwise the reader can read interpret it as the other methods are not efficient.

L. 93-94: the contrasting optically properties of AW and PW are also with respect to the FDOM fraction (see Jørgensen et al 2014; Gonçalves-Araujo et al 2016). Additionally, such a contrast in CDOM properties is also highlighted in Stedmon et al 2015.

L. 94-97: missing references

L. 100: would be interesting to shown how CDOM/FDOM measurements can be used as a proxy for tracing other parameters (e.g., freshwater, DOC, phytoplankton primary production, etc).

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L. 102-118: There is a recent paper that may be useful for the authors: Gonçalves-Araujo et al 2018.

L. 110-113: please consider re-writing the sentence. It is missing a verb.

L. 139: consider adding the (Bjørnøya Current) BC to figure 1.

L. 171-172: the water sample collected below the Chl-a maximum. Authors can provide more information regarding it. Was it within the pycnocline? Or at the bottom of the deep Chla maximum layer? How was the sampling depth determined?

L. 178-189: have the authors performed any test to check whether the differences in storage affected their results?

L. 250: was is the meaning of CRM?

L. 303: Sagan et al 2017. Authors should avoid citing unpublished work.

L. 315-317: It is worth to mention that the referred water masses are found within the "upper layer", given that no water mass from deep layers was presented.

L. 324: the meaning of the PSWw (as well as its thermohaline characteristic intervals) was not presented. Additionally, and for consistency... the PSWw is mostly referred in the literature as ASW (e.g. Pavlov et al 2015 and Gonçalves-Araujo et al 2016).

L. 339: I suggest the authors to add some specific words to the title that would better reflect the results presented in this sub-section: spatial variability and hydrography.

L. 340-357: please consider including the standard deviation or standard error to the averaged values presented through the text.

L. 353: 200 m water layer. The indication for meter is missing.

L. 358: what do the authors mean by distinct? Please be more specific.

L. 371-372: How do the authors explain the higher CDOM in PSWw compared to PSW? Shouldn't the DOM signal in PSWw be diluted by sea-ice melt waters and therefore,

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lower than the DOM observed in the core of PSW as highlighted in Pavlov et al 2015, Stedmon et al 2015, Granskog et al 2012, Gonçalves-Araujo et al 2016, etc.?

L. 378-379: highest aCDOM(350) found within PSW and AAW in 2015. . . are those values for each water mass significantly similar? How do the authors explain such a high CDOM content associated to the AAW?

L. 380-382: Could this represent a consistency in the DOM content (fingerprint) with respect to each water mass?

L. 386-391: Since SUVA<sub>254</sub> and a<sub>350</sub>\* are both normalized by the same value (DOC concentration), what do the differences between the two parameters mean?

L. 396-397: is that decrease significant?

L. 396-399: what was the correlation between a<sub>350</sub> and Chla? If autochthonous DOM is dominant in the sampling region, there should be a good correlation between the two parameters, right? (or at least for AW, with little continental/Arctic influence)

Fig 3b: It is clear that the I<sub>chl1</sub> had the highest fluorescence intensity values in R.C., but, since the results of this MS are more focused on the I<sub>chl3</sub> (autochthonous), I suggest the authors to present the I<sub>chl3</sub> in this figure as well.

L. 469-470: "The relationship was more significant in 2014. . .". It looks like that the significance is the same for both years. Did the authors want to say that the relationship was stronger (given differences in r<sup>2</sup>)?

Fig 5a: Looking at the legend it is not clear what the black line means. Is it the regression curve for the combined dataset from 2014 and 2015 or was it the difference between them? I also think that the information displayed in Table 4 can be easily incorporated to Fig 5.

L. 508: At what salinity did Granskog et al 2007 found a<sub>cdom</sub>(355) greater than 15m<sup>-1</sup>? It would make it easier to establish comparison to the other studies.

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L. 502-518: There is a recent paper from Gonçalves-Araujo et al 2018 that shows aCDOM values for the central Arctic and surroundings that the authors may want to look at.

L. 521: do the values also agree with results published in Stedmon et al 2015?

L. 541: "... and similar features of CDOM properties were..." - What do you mean by similar features of CDOM properties?

L. 549: What do the authors mean by "Statistical distribution"?

L. 553: How did the authors infer that the waters are from different origins based on their dataset?

L. 556: Gonçalves-Araujo et al 2016 has studied FDOM and not CDOM as mentioned in the text.

L. 565-566: Have the authors tried to look for the a350 vs. temperature correlations?

L. 571-572: where did the authors take this information from?? Was it from the literature (then it is missing a citation) or from their dataset (then I would like to know how the data supports such an affirmation)?

L. 573: "S300-600 varied little between water masses in a given season..." Have you tested it for significance?

L. 612: "... could indicate freshly produced CDOM." . At what depth was this found? Have you looked at the correlation between S300-600 and Chla? Is there any correlation for the high Chla samples?

L. 611-614: This is an important discussion of the main results and could be discussed more in deep.

L. 617-620: Not clear whether the authors have checked that or it is only an assumption.

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- L. 623-625: Not clear what the authors wanted to state in that sentence.
- L. 654: Authors may want to look at the results from Walker et al 2013 for comparison.
- L. 682-683: Why do the authors think the a350\* vs. S300-600 behaves like this? Was the correlation also strong (and significant) for each year analyzed separately? It does not look like that (specially for 2013) when looking at Fig. 8.
- L. 695-696: significant differences for humic-like and PTN-like FDOM. . . With respect to what were these differences?
- L. 699-701: How do the authors explain such behavior?
- L. 702-705: not clear if the authors are presenting their own results or results from other studies.
- L. 722-723: It is not completely clear based on the results presented here. The authors may want to rephrase that sentence. The authors state that the PTN-like FDOM is the dominant fraction at high salinity. However, studies have shown that the humic-like (visible fluorescence) can be the dominant signal at sal>34 in the Arctic Ocean.
- L. 732-734: This sentence does not help the discussion and could be easily removed.
- L. 742: The paper from Gonçalves-Araujo et al 2016 does not show CDOM results.
- L. 756: ". . . and same dominant factor controlling these parameters in time and space." How do your results support that statement?
- L. 762-763: Could the authors identify what FDOM fractions are produced through each of the mentioned processes (e.g. phytoplkt extracellular release, phytoplkt degradation or lysis)??

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