

## ***Interactive comment on “A synthesis of light absorption properties of the Pan-Arctic Ocean: application to semi-analytical estimates of dissolved organic carbon concentrations from space” by A. Matsuoka et al.***

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General comments:

Matsuoka et al. present constituent absorption data for several Arctic regions and apply these data and a previously published semi-analytical (SA) CDOM algorithm to develop and present pan-Arctic satellite distributions of aCDOM<sub>443</sub> using MODIS monthly climatology. One of the major findings is that CDOM contributes the greatest proportion of absorption to the Arctic Ocean compared to particle absorption. Existing DOC relation-

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ships with aCDOM for two Arctic regions (Kara and Laptev seas and Beaufort Sea) and SA CDOM algorithm were applied to generate a MODIS climatology of DOC for those regions. The applicability of the aCDOM<sub>443</sub> and DOC algorithms for areas beyond the regions sampled is not addressed. The evaluation of the satellite-derived DOC based on general comparisons to measured values rather than more rigorous validation approaches (e.g., Bailey and Werdell, 2006 RSE). The satellite DOC retrievals should be removed from the manuscript unless an analysis can be conducted to demonstrate the capability of the algorithm to retrieve DOC, even a comparison of matchups of in situ DOC and satellite DOC for in situ DOC used in the satellite algorithm development would be useful. The work presented does improve our understanding of the optical properties of the Arctic and their spatial distributions. The application of satellite absorption products from the SA will permit further analysis of the optical properties of the Arctic in both space and time. Unfortunately, the authors do not interpret the distributions of the the satellite data presented in much detail, other than differences between the WAO and EAO.

Specific comments: See additional comments on the manuscript.

1. The title of the article is not consistent with the data presented. The field data synthesized come from a modest portion of the Arctic – northeastern Alaska/Chuckchi, Beaufort Sea and Laptev Sea with a few data points from the Kara Sea. The satellite distributions of aCDOM<sub>443</sub> in Fig. 8 are truly pan-Arctic, but the validity of the SA algorithm is not evaluated beyond the regions sampled on the MALINA and ICESCAPE 1 and 2 cruises. Also, the satellite DOC distributions are not evaluated using a rigorous approach.

2. Page 6, lines 128-139 and Fig. 1: There is inconsistency in the attribution of season to the cruise data presented. For example, NABOS data collected from 14-30 September are classified as Summer, but the MR cruise from 1 September to 13 October is classified as Autumn. Cruise dates could be listed in Figure 1 rather than season or the MR cruise could be classified as Summer/Autumn. Regardless, if sea-

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son is used, then it would be best to define the period for each season. Especially, since prior work in other regions have shown differences in aCDOM to DOC relationships (intercept primarily) with season. This could impact the application of the DOC algorithms/relationships as indicated by the authors in the conclusion section.

3. Section 2.5 (lines 264-268) describes some limited statistical analyses, yet there are many other statistical computations presented in the Results and Discussion. It is not clear how the  $r^2$  and p-values were calculated. Were these Pearson product-moment correlations, Spearman rank correlation or linear regression analyses? How is normalized mean bias computed?

4. Page 15 – line 352: The 9% uncertainty value listed for aCDOM443 is not consistent with the data shown on Fig. 7. There are many data points close to +/-35% error compared to points that fall on the 1:1 line. How was the 9% value computed? What is the mean absolute percent difference of the error values plotted on Fig. 7?

5. Page 10-11 – lines 269-27; Fig. 10; lines 410-417: The authors indicate that the Walker et al. DOC to aCDOM relationship is based on data from the mouths of 5 rivers, 4 of which are from the Russian portion of the Arctic and the 5th being the Mackenzie [this cannot be verified as Walker et al. is in press and the journal was not listed]. Because the Walker et al. relationship yielded erroneous satellite DOC values for the Beaufort Sea, based on comparison with field data from the literature, the M12 relationship derived for the Beaufort Sea was applied. So, the DOC satellite retrievals for the two Arctic regions are based on the 2 lines shown on Fig. 10. The data and statistics associated with these regression lines are not shown. Since no data are shown for these regression lines, why not simply list the equations and references. There is no need for figure 10.

The corroboration of the satellite DOC is limited to a qualitative comparison of the range of DOC observed from the literature (lines 410-417). The Walker et al. DOC relationship was applied to obtain satellite DOC distributions well beyond the coastal region of

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the Kara Sea and Laptev Sea. There are no corroborating results to demonstrate how well the approach retrieves satellite DOC. This is particularly important when applying a relationship such as from Walker et al. to a region well beyond where the data were collected (mouth of the rivers). If one of the objectives of the article is to present “Pan-Arctic scale” satellite DOC distributions, then there must be a rigorous evaluation as to how well the algorithms perform as was accomplished for aCDOM443. Also, DOC retrievals are limited to a much smaller region of the Arctic (Fig. 11) than aCDOM443 (Fig. 8). There is limited explanation in the manuscript for this. The non-remote sensing community has not fully accepted satellite data as a source of high-quality data products. Thus, scientists applying remote sensing data must be cautious in presenting new satellite products that are not rigorously evaluated as we may further alienate the larger scientific community. The burden is upon the remote sensing experts to demonstrate the quality of the satellite data products that are developed. The authors should provide a more robust evaluation of the DOC satellite product or remove it entirely from the manuscript.

There are publically available datasets of DOC from SBI and likely other cruises that could help address the evaluation of the DOC algorithm presented here.

6. Page 16 – lines 382-383: A linear regression analysis was conducted on the DOC vs aCDOM data to derive the intercepts and slope values. Was a type II or type I regression applied? Did the DOC values meet normality assumptions for the regression analysis?

7. Page 16, lines 385-392: Fig. 11 does not show much seasonal variability in DOC within the Beaufort Sea region. One would expect seasonal differences due to in situ DOC production by phytoplankton as well as from terrestrial contributions from variability in river runoff.

8. Page 17, lines 414-417: This sentence is a bit confusing and not completely correct. Semi-analytical (SA) algorithms such as this one and GSM are fundamentally

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empirical, just more sophisticated than band ratio or other empirical algorithms. The true value of “semi-analytical” algorithms is the inversion of radiometric data for obtaining the various optical parameters. Also, within this semi-analytical algorithm, there are empirical relationships such as deriving aNAP from bbp. The SA algorithm is regionally tuned, so I don’t understand the distinction between this SA and the other algorithms cited – “this algorithm depends less on empirical relationships established for particular time periods and areas.”

9. Pages 16-17: In its current form, there is not much value in the discussion pertaining to DOC. The issue that DOC is higher in the Siberian seas than the Beaufort Sea is already known based on field data presented in the literature. If the DOC algorithm could be validated or confirmed, then a more detailed discussion of the satellite DOC observations is warranted. Figure 11 appears to show higher DOC later in summer for the Laptev Sea. Can this be attributed to river runoff, in situ primary production or melting sea ice? The increase in DOC seems to correspond with increases in aCDOM443.

10. The seasonal and spatial variability of aCDOM could be further exploited in the manuscript to further demonstrate the utility of satellite data to improve our understanding of Arctic biogeochemistry and oceanography.

11. Table 1 – the column headers show % for the absorption constituents, yet the values in the table are proportions rather than percentages. These should be consistent.

12. Table 3 can be deleted altogether by including the regression values within Fig. 6 caption or on the plot. Were Pearson correlation analyses conducted or regressions? The caption states correlation, but Table 3 reports r<sup>2</sup> values.

13. Fig. 8 and 11 – individual figures too small to see the aCDOM443 and DOC distributions within the coastal waters of the Arctic. Fig. 11 could be modified to show the Siberian and Beaufort regions in separate plots to enlarge these regions. Don’t see much of a DOC gradient in the Beaufort Sea due to the color scale in Fig. 11.

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14. Authors should consider including a reference on MERIS retrievals of DOC within the Kara Sea by Korosov et al. 2012 - *Advances in Space Research*, 50, 1173–1188.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C7421/2013/bgd-10-C7421-2013-supplement.pdf>

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Interactive comment on *Biogeosciences Discuss.*, 10, 17071, 2013.

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