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Interactive comment on "DOM and its optical characteristics in the Laptev and East Siberian seas: Spatial distribution and inter-annual variability (2003–2011)" *by* Svetlana P. Pugach et al.

Anonymous Referee #2

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Review of Svetlana et al DOM and its optical characteristics in the Laptev and East Siberian seas: Spatial distribution and inter-annual variability (2003-2011).

The manuscript has the aims of reporting on the inter-annual variability in CDOM and DOC in the Laptev and East Siberian sea. It reads very much like a cruise report and would benefit from a more comprehensive data analysis and discussion of the results obtained. The referencing of previous literature is suboptimal and at times inappropriate. In my opinion there is a missed opportunity for a solid analysis on the linkage between CDOM absorption, fluorescence and DOC across several years. The

authors are in possession of a unique dataset which is only lightly touched on. Why not take more inspiration from the Belzile paper cited and include a comparison of your data with theirs from the East Siberian sea? Does the same FDOM to CDOM relationship exist? The section on the inter-annual variability is difficult for the reader to follow as is. It would likely be easier if figure 5 and 6 were combined so that the sea level pressure maps could be compared with the CDOM and salinity distribution maps. Alternatively, the authors could just compare the maps of both salinity and SLP, then in a separate figure reveal how robust the salinity CDOM relationship was. In this form the manuscript is not suitable for publication and I recommend re-submission after revising the data analysis. Whilst doing this you should consider splitting the results and discussion sections to allow for a better separation between your results and reflections on how your findings link to other studies.

Other points to address:

Line 8. "amount" rather than "volume".

Line 21. Replace "were" with "was"

Try to avoid use of "e.g." in referencing and citing very many studies. Find the most relevant and limit it to 3-4.

Line 46. Replace "gives input" with "supplies".

Line 50-51. I suggest you specify this more. Many rivers and streams have high or higher DOC but few large rivers have concentrations this high at their mouth.

Line 54. Replace "lead" with "is leading to"

Line 56-68. This section should be rephrased and better references found. If you do not want to have too many references I recommend you pick either the original papers or first to demonstrate this in the Arctic. Currently there is a bizarre selection of studies cited and not all directly relevant.

Line 78-79. Several of these references are not even Arctic. Line 80. Were there not any additional scientific aims or hypothesises? Possibly developed during the data analysis for this study? Try to mention them here. As stated now the aim reads very much as a data report.

Delete line 83-86. This has been established in the Introduction.

Line 95. Check your phrasing of " would be oxidised".

Line 133-134. Delete this. It is a standard fluorometer which is readily available. No need for this. Also the description of the interior optics can be removed. Not really necessary and appears to be copy pasted word for word from Belize et al 2006 paper, which is a little alarming.

Line 146. What ranges? I do not understand.

Lin 150. It is not valid to apply the fit across this range. The spectrum does not behave exponentially and in many samples there will be a shoulder at 280. Additionally the absorption below 240 will be mainly due to other constituents.

Line 156. Sr is not explained, and the whole this part if poorly written.

Line 157. SUVA is not that recent and include a citation of the original paper for this (Weishaar). Line 158. I do not agree with this sentence. Starting "The last parameter. . ."

Line 161. I do not agree with this extrapolation. The relationship demonstrates the expected link between MW and SUVA but not does not mean that the relationship is fixed and one can use it to determine MW in other systems.

Line 168. "The value of S increases with the decrease of the CDOM absorption coefficient". This is not true. It depends on the values of the end members (see Stedmon and Markager 2003). Line 169. Include reference for relationship between S and aromatic content/molecular weight

Line 172. Several of these references did not even measure or report the spectral slope at 275-295.

Line 182. First sentence is repetition.

Line 190. What do you mean by spectral dependency of S275-295? The spectral range should be constant.

Line 193-209. Why not expand the comparison of slope values and ratios with data available from other Siberian rivers Eg. In Walker et al 2013 doi: 10.1002/2013JG002320 (they have seasonal data to compare to). Stedmon et al 2011; Mann et al 2014 & 16. And Gonçalves-Araujo et al 2015 10.3389/fmars.2015.00108

Line 201-214. Is this analysis/interpretation only based on the 2004 data. Why not expand to include all data and compare where you see the qualitative change with where there also is a large drop in CDOM? Is it at the same region the drop in SUVA occurs across all years or is it more salinity that is driving the drop seen in the figure?

Figure 9a and b. It would be more robust to derive the relationship for the 2004 data and test in on the data from other years. I wonder if you carried out the regression analysis between DOC and salinity if you get the same predictive power. The data here look to be very conservative. Mixing is dominating.

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