

## ***Interactive comment on “Optical properties of size and chemical fractions of suspended particulate matter in littoral waters of Quebec” by Gholamreza Mohammadpour et al.***

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Page 1 line 14. ‘high concentrations of chromophoric dissolved organic matter’, why this matters? Originally was suggested since CDOM may create a coating around the particle that is expected to enhance the light absorption per unit of weight. However, this effect is likely minor, thus this CDOM influence was deleted from the text

Page 3 line 20. How PIM and POM were estimated? We expand the sentence as: ‘The inorganic fraction of SPM (i.e., particulate inorganic matter or PIM) was obtained after removing the organic fraction (i.e., particulate organic matter or POM) of the original sample by combustion at 450°C for 6 h. Due to the dehydration of clays, this procedure

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may introduce an additional uncertainty of -10% and +10% on particulate inorganic (PIM) and organic matter (POM), respectively (Barillé-Boyer et al., 2003; Stavn et al., 2009’).

Page 3 line 26, CDOM is the fraction below 0.2 $\mu$ m typically. Here you seem to call any fraction passing a filter CDOM. Make sure people understand that or use a different name, for example filtered fractions.

We agree and clarify: ‘CDOM is defined here as the fraction of dissolved organic matter passing through a membrane with a nominal pore size of 0.2  $\mu$ m’.

Page 3 line 30, I believe this type of method has been used way before Rottgers. Yes, the ‘flat method’ it was originally proposed by Bricaud and Stramski (1990) The reference was added

What did you hope to achieve with a baseline correction (e.g. Zaneveld et al., )? This is a first order correction for scattering effects on non-water absorption coefficient estimates. Is it reasonable to assume scattering is spectrally flat? It is an assumption and there is debate. Some studies have reported a spectral dependency on volume scattering functions or particulate backscattering ratios (Chami et al., 2006; McKee et al., 2009; McKee et al., 2013). But there is doubt regarding if this assumption can be generalized (McKee et al., 2013).

Why did you choose this scattering correction (e.g. as opposed to the proportional or Rottgers one)? We are aware that other methods exist (e.g., proportional to wavelength, Monte Carlo) (Zaneveld et al., 1994; McKee et al., 2013). However, the performance of these techniques to correct for residual scattering is not satisfactory either and/or may require additional optical information that we didn’t have during the field surveys (e.g., particulate backscattering ratio) (McKee et al., 2013).

Also we say, ‘Thus, the calculation of particulate absorption coefficients is expected to have a bias with respect to true values measured using absorption-meter instruments

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that are less influenced by particulate scattering (e.g., point-source integrating-cavity absorption meters) (Röttgers et al., 2014).

Page 4 line 20. It looks like you are using something similar to Gordon's formulation (replacing  $bb$  with  $b$ ). Why not do it from the get go and skip equations (1) and (2)?

These optical proxies were deleted and no need to make reference to Gordon's formulation. We work now with two optical proxies commonly used in the literature,  $\alpha_{SPM}$  and  $Svis$ .

Page 4 line 25,  $\alpha_{SPM}(676)/\alpha_{SPM}(444)$  This is basically an indicator for [Chl]. You will get a better one by doing a line-height subtraction.

BOI indexes are not longer part of the manuscript

Page 4 Line 26. you may want to look Boss et al., 2004, JGR & 2009, LOM, for the use of  $a(676)$ -line height/ $c(660)$  for particulate composition.

Thanks for the advice. We checked two indices of particle composition suggested in Boss et al. (2004) JGR and Boss et al. (2009) LOM. The first index relates  $bb/c$  to POC/SPM and the second index relates  $bbp/bp$  to chlorophyll concentration/cp. Although very interesting, these two proxies were not evaluated since no backscattering measurements were obtained during our surveys.

Page 4 Line 29. Don't forget you have the spectral-slope of beam attenuation to work with as well Yes, we are aware of relationships between the spectral slope of cp and the hyperbolic slope of the particle size distribution (Boss et al., 2001). Additional correlations of cp spectral slope values were included as part of the analysis

Page 5 line 1, 'it will be very useful for an optical oceanographer evaluating your result if they could see figures of the SPECTRA of the mass normalized IOPs.

We added one additional figure (fig 2) where averaged  $ap^*$  and  $bp^*$  for the whole study area and each subregion are shown as a function of wavelength are shown

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Page 5 Line 5, 'why not also compute  $c^*$  for which there is a longer literature?' Although it is possible and interesting, our main interest is focused in IOPs that separate scattering from absorption effects. This is not the case for  $c$  or  $cp$ , thus their use makes interpretation of optical processes less clear.

Page 5 line 12, 'you may want to refer to it as the exponent of the power-law distribution. Junge is usually used to denote the one with a differential exponent of 4' Done

Page 5 line 16, 'you can use  $D50$  from the LISST as a more robust parameter' Additional correlations between parameter  $D50$  (here symbolized with  $Dm$ ), spectral slope of particulate beam attenuation ( $\alpha_{SPM}$ ), differential slope particle size distribution ( $\alpha_{SPM}$ ), mass fraction of PIM or concentration of PIM/concentration of SPM ratio (FSPMPIM), and mass-specific optical coefficients did not show a general improvement with respect to parameter  $\alpha_{SPM}$  (see below). The correlation will depend on the size fraction

Correlations in the following table are based on 23 sampling locations. FSPMPIM  $Dm$   $\alpha_{SPM}$   $\alpha_{SPM}0.2 - 0.4 \mu m$   $-0.42^* -0.51^* 0.53^* -0.28^{**}$   $FSPM0.4 - 0.7 \mu m$   $0.35 0.41^* -0.43^* 0.11^* FSPM0.7 - 10 \mu m$   $0.23 0.08 -0.38^* 0.12^* FSPM > 10 \mu m$   $-0.08 0.21 0.13 -0.04$

Page 7 line 1, 'you paper is totally lacking an uncertainty analysis. You need to add uncertainties in all your calculated values based on:replication. Assumptions (e.g. scattering correction used, finite acceptance angle of the ac-9)'.

One additional section 4.1 was included in discussion to summarize the different uncertainties involved in measuring IOPs. There was not replication of discrete samples, however it was possible to compute the optical variability during the ac-s measurements. This information is described in discussion along with the assumptions regarding the transmissometer and the scattering effects on the estimates.

Page 7 line 16, 'not having backscattering measurements and radiometry, this is a hard case to make.' We agree, we talk now about optical proxies instead of remote sensing

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proxies

Page 8 line 22, 'But note that multiple scattering may have affected their optical measurements' We added this observation to the discussion. 'Notice that part of this decrease can be attributed to an incomplete removal of multiple scattering effects'.

Page 8 line 24, 'This is known for a long while, e.g. Morel's 1974 work' We added this reference

Page 8 line 31, 'CDOM cannot explain increase in  $a_{SPM^*}$ ' The effect of CDOM on  $a_{SPM^*}$  was not quantified and is likely to be minor, thus it is a weak statement. Thus it was deleted from the text

Page 9 line 12, 'this will be true for in-situ aggregates (Slade's work). However, you are disrupting aggregates, so it is less likely'

We clarify the sentence as follows: 'Since particle aggregates were altered during our experiments, the influence of particle density on mass-specific optical coefficients cannot be quantified as this effect is mainly observed in undisrupted marine aggregates (Slade et al. 2011.)'

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