

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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General comments In sum, this paper requires a massive reanalysis of the data and a massive effort at rewriting.

The corrected version of the manuscript was totally re-structured, many sections rewritten, new figures, new analysis of data and deletion of redundant tables

Specific Comments: Page 1, lines 7-9. The language in this manuscript can be pretty cryptic. Some expansion is required here and below for clarity even though I realize abstracts are supposed to be kept as short as possible. Suggested wording: Abstract.

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Empirical mass-specific absorption (a_{SPM}^*) and scattering (b_{SPM}^*) coefficients of suspended particulate matter (SPM) were measured for different size fractions (proposed to be 0.2-0.4 μm , 0.4-0.7 μm , 0.7-10 μm , and $>10 \mu\text{m}$) in the surface waters (0-5 m depth) of the Saint Lawrence Estuary and Saguenay Fjords (SLE-SF) during the spring of 2013. True optical absorption and scattering cross sections were determined for the total PIM and POM, in addition to mass-specific absorption and scattering coefficients.

The abstract was rewritten and we talk now about true optical absorption and scattering cross sections

Page 1, line 10. A synopsis of the results of the determination of the true optical absorption cross sections also needs to be reported here. It requires pulling together the results on the spectral range of absorption cross sections of at least PIM to document the effects of adsorbed iron on clay minerals or suspended iron oxides in the PIM. That is, an analysis of the true absorption cross section, a_j , for chemical fraction j , organic or inorganic. The true optical cross sections determined here provide the information to interpret the empirical coefficient ratios reported for size fractionation, etc. This information documents the statements in the final sentence of the abstract such as for the effects of chemical composition and absorption variability on what is reported here.

Spectral changes on a_{SPM}^* are now reported and discussed in terms of iron effects on particulate absorption.

Page 1, lines 13-14. It is not at all clear here what the authors mean when identifying variability of the empirical mass-specific absorption and scattering coefficients. In addition, the results of this study call into question the utility and feasibility of utilizing these empirical coefficient ratios. Suggested wording: (Gironde River). a_{SPM}^* ... particulates. Correlation analysis of the optical properties and the empirical ratios of this study suggests that particle composition has the most significant impact on variability of a_{SPM}^* and particle size distribution has the most significant impact

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on bSPM* variability. The fact that knowledge of the optical cross sections is necessary to interpret these empirical ratios calls into question the utility of aSPM* and bSPM* in general models of microphysical and biogeochemical processes proposed for all coastal/estuarine systems.

The abstract was rewritten and results clarified

A fundamental problem with all correlational analyses, as opposed to a well-defined regression analysis, is the fact that correlational analysis merely records the co-occurrence of phenomena without postulating a fundamental relationship between variables of the phenomena. High correlations simply mean that other, more fundamental relationships may be causing the co-occurrence of unrelated phenomena. An analysis based on aSPM* and bSPM* will always be correlational and limited to the specific region where the relationships were defined.

Like any other statistical analysis there are pros and cons. A well-defined regression analysis has many and strict assumptions that should be met such as normal distribution of variables, random sampling, etc. This issue is absent when non-parametric correlations are used. Yes, we are always talking about our study area regarding correlations results

Page 2, lines 1-2. Algorithms based only on CSPM will never have the accuracy required for optical inversions because SPM is undefined optically, an unknown mixture of inorganic and organic matter. Therefore partition of SPM into at least major chemical composition classes (PIM and POM) and estimation of size distribution are required independently for optically-based remote sensing algorithms of primary productivity and suspended mineral dynamics of "disappearing shorelines" etc.

It is a relative questioning. Depends on the level of accuracy you are interested. Many studies have been proposed for estimating CSPM based on remote sensing methods. Adding remotely sensed PIM and POM to calculate SPM will also have a large error due to the addition of two errors linked to PIM and POM algorithms

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Page 2, lines 13-17. A fundamental issue here, not often discussed in the literature but should be, is consideration of what constitutes Inherent Optical Properties and how this concept should be applied to the measurements taken in the field. One can take a bulk absorption or scattering coefficient of an undefined mixture of material suspended in water and easily determine their mass-specific coefficients but what do they really mean? The absorption coefficient of dissolved matter such as CDOM can be related to a general chemical class of dissolved compounds and we can come up with a measurement of absorption that can be related by refractive index or whatever to a similar group of compounds and the absorption coefficient of CDOM can be analyzed in a quantitative manner. That is, an absorption coefficient of CDOM from one region can be related quantitatively (absorption cross section, etc.) to an absorption coefficient of CDOM from an entirely different region. So an absorption coefficient of CDOM can be called an optical property as per the definition of Bohren and Huffman (1983, p. 227), "There are two sets of quantities that are often used to describe optical properties: the real and imaginary parts of the complex refractive index $N = n + ik$ and the real and imaginary parts of the complex dielectric function (or relative permittivity) $\epsilon = \epsilon' + i\epsilon''$." In other words, genuine optical properties must have defined complex refractive indices and permittivities which the absorption and scattering coefficients of SPM do not have. Again, SPM is an unknown mixture of both mineral and organic matter and the SPM composition varies from point to point in the same region and furthermore varies between different regions. If we separate out mineral and organic matter we can approach true optical properties of this material by having more narrowly defined complex refractive indices and relative permittivities. By this definition the absorption and scattering coefficients of SPM cannot be called optical properties and their mass-specific versions, aSPM* and bSPM*, should only be called empirical mass-specific ratios. At best the absorption and scattering coefficients of unpartitioned SPM can be referred to as "optical proxies." Thus the term "optical properties" should be limited to the optical cross sections and absorption and scattering coefficients for PIM and POM only. This rationale will be followed in my subsequent comments.

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I understand your point and thank you for your wonderful insight but we try to go along with definitions of optical properties currently used in the current literature. This should be a good topic for another publication.

Page 3, lines 15-20. The description of the procedures utilized here is confusing. The process of size fractionation of suspended matter in water is tricky (Sheldon and Sutcliffe, 1969; Sheldon, 1972). It is important to recognize the difference between screens and filters as was pointed out by Sheldon and Sutcliffe (1969). A screen is designed for separation of materials in suspension of a particular diameter and a filter is designed for retention of all materials in suspension greater than a given diameter. That is, the manufacturer guarantees that a filter of a given nominal pore size will retain all material larger than the nominal pore size. However, as a filter slowly gets clogged it will retain material smaller than the nominal pore size. All the filters mentioned in this section were not designed to be screens and the nominal manufacturer's pore size is not the median pore size for retention as demonstrated by Johnson and Wangersky (1985), Sheldon (1972), and Sheldon and Sutcliffe (1969). The median size of particles retained is a function of the volume of sample filtered and the concentration of particles in the sample. The use of manufacturer's nominal pore size to delineate the size fractionation, as is done in this paper, does not correctly give the limits of the size fractions unless the authors did extensive tests on the particle size-range and retention capacity of the filters they utilized under their particular conditions of filtration. It is not clear which filter was used for the loss-on-ignition determination of the total suspended mass and partitioning of it into PIM and POM. If the Whatman GF/F filter were used for SPM, PIM, and POM determination then why was the same filter used for fractionation into the supposed 0.4-0.7 and 0.7-10 mm size ranges? The Whatman GF/F filter can work well for removing nearly all particles down to about 0.2 mm out of suspension. Chavez et al. (1995) reported about 95% particle retention down to 0.2 mm by Whatman GF/F filters. Johnson and Wangersky (1985) derived a theory, involving diffusion and adsorption of suspended materials and filter pore walls, demonstrating that filters will retain particles much smaller than the nominal pore size reported by the manufac-

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turer. One of their conclusions was that a Whatman GF/C filter of nominal pore size 1.2 mm, depending on concentration of materials in suspension and flow rate of filtration, could be an efficient method of separating out materials in suspension larger than 0.7 mm

No, different filters were used in this study to fractionate 0.4-0.7 and 0.7-10 micron fractions. This sentence was clarified as:

Size fractionation of SPM into four size classes ($>10 \mu\text{m}$, $0.7\text{-}10 \mu\text{m}$, $0.4\text{-}0.7 \mu\text{m}$, and $0.2\text{-}0.4 \mu\text{m}$) was done after sequentially filtering the original samples through pre-weighted membranes having a diameter of 47 mm and a pore size of $10 \mu\text{m}$ (Whatman, polycarbonate), $0.7 \mu\text{m}$ (GF/F, Whatman, glass fiber), $0.4 \mu\text{m}$ (Whatman, polycarbonate), and $0.2 \mu\text{m}$ (Nucleopore, polycarbonate), respectively.

Also, we add the whatman GF/F was used for PIM and POM determinations.

We wrote: The mass of PIM was obtained after removing the organic fraction (i.e., POM) from the total mass of SPM as computed for CSPM determinations. The mass of POM was eliminated by combustion of GF/F filters at 450°C and during 6 h. The concentration of POM was calculated as the difference between the dry mass of SPM and the dry mass of PIM. The precision of PIM determinations was 25% since an additional variability of 10% was added to the error measurement of SPM mass due to the dehydration

We are aware of the issues using the nominal pore size and the retention of small particulates. We mention that issue when computing mass-specific optical coefficients in discussion

The authors need to give a table of the suspended masses retrieved by the various filter sizes and compare it with the total mass retrieved on a single filter. Given the fact that the nominal pore sizes of the filters do not correspond to the actual sizes of material retained on the filters, I would be surprised if the masses of the sub samples from

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various filters added up to the total mass retrieved from one filter. Since the masses retained by the filters are the key to the results reported in this paper, I suggest some sort of optimization scheme to adjust the total mass and the sum of the subsample masses so that a probable mass partition can be utilized based on the masses retained on the filters, i.e. adjusting the various masses to sum up to the total mass filtered. This would presumably require various weighting factors to be applied to the measured masses. It appears the Whatman GF/F filters were used for both total mass filtration and for determining two sub-sample ranges. The authors must explain carefully just how this was accomplished.

Good point. Total mass of SPM was calculated based on gravimetric determinations based on 0.7 microns GF/F filters. This is standard in the literature (Stavn and Richter, 2008; Rottger et al, 2014). However, it is true that $aspm^*$ and $bspm^*$ are overestimated since $aspm$ and $bspm$ are based on particulates above 0.2 microns This is due to the pre-filtration of samples through nucleopore membranes in order to remove CDOM+seawater contributions.

In the other hand, size fractions of IOPs correspond to the same size fractions of mass. Thus, there should be no bias on mass-specific coefficients of IOPs for SPM. To evaluate the effect of sieving on retaining smaller particulates than pore size, comparison were made between filtered samples without pre-sieving vs sum of size-fractioned samples. In average, adding mass fractions resulted in a total mass difference for particulates larger 0.7 microns of +31.4%. In other words, a 31.4% overestimation of mass for >0.7 microns particulates when the sum of weights of fractions is performed rather than weighting the unfractionated sample

These filter 'effects' on retained SPM mass may be possible to correct as the example described above. However, we didn't filter total unfiltered samples through 0.2 or 0.4 microns membranes since we did sequential filtering. Thus, factors such as sum fractions mass/unfiltered mass for size fractions >0.2 or >0.4 microns could not be calculated in our study.

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In discussion, we described the general overestimation of $aspm^*$ and $bspm^*$ values and 'filter effects' on mass-specific properties of size fractions of SPM

Page 3, lines 22-23. The authors point out that they did not use the correction factors discussed by Barille-Boyer et al. (2003) to account for the loss of structural water by the suspended clay minerals. The authors state that an error of about 10% will accrue to the PIM and POM estimates if ignored. The 10% error is only for the inorganics while the 10 % error in inorganic mass will generate a greater error in the organic mass, easily as much as 30% overestimation error in the POM estimate. I suggest the authors utilize the extensive geochemical publications on the St. Lawrence Estuary to estimate the probable concentration of the various clay mineral species in their samples in order to calculate this error. One possible source is Danglejan and Smith (1973).

We added to the text the larger error of POM mass determinations By using the Danglejan and Smith (1973) data related to clay composition in the SLE, we calculated an underestimation of PIM mass of 3.1% Also, 3.22% of loss of ignition PIM must be removed from POM in order to obtain a POM mass corrected by structural water of clays

Based on Barillé-Boyer et al. (2003) factors and clay composition data obtained in the Saint Lawrence Estuary (D'Anglejan and Smith, 1973), the estimated error of PIM determinations due to dehydration of clays was 3.1%. Thus, PIM mass determinations has a maximum uncertainty of 18.1%. Notice that error in POM mass estimates is slightly greater than that associated to PIM mass (18.22% of loss on ignition PIM mass

Page 3, line 30. The weightings used to correct the mass fractionation of the filters should be applied here to the estimates of spectral absorption by the various estimated size fractions.

See above the issue of using this weighting for certain size fractions

Page 4, lines 4-5. The spectral measurements of c should also be adjusted by the

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weightings for the SPM size fractions as mentioned above for the absorption coefficient. Of course this then results in weighted values of b for the size fractions. Then one should check this with the known optical relation that the various b values measured for the sub fractions should add up to the b value recorded for the total SPM. All of this information about a and b values should be recorded in a table.

Actually IOPs after each filtration are not added up but are decreasing in magnitude as samples are filtered through membranes having a smaller pore size. New figures are shown for size fractions of a_{spm}^* and b_{spm}^* . For some samples, the calculation of IOPs lead to negative values at some wavelengths. These curves are not included as part of the plots and might be related to issues linked to the filters or particle aggregation/disaggregation effects.

Page 4, lines 5-7. The authors mention the use of a LISST-100X for determining particle size spectra in the range 3-170 μm and then never mention these data again. If the data were important they should be brought into the discussion, especially considering the lack of precision and accuracy in the attempt to do size fractionation of suspended matter in this study. Were the LISST data used to estimate the Junge slope g ? If so the extensive analysis of submicron materials in this study will not have relevance to g and these correlations must be removed from the analysis. If the LISST-100X data were not used then the use of the LISST-100X is irrelevant to this study and should not be mentioned.

Yes, LISST-100x was an important instrument to compute differential Junge slope. More interpretation and results and included now regarding $\tau_{A\lambda}$. We don't think correlations between smaller than 2 microns particulates and $\tau_{A\lambda}$ are spurious since it is feasible correlations due to the fact that smaller than 2 microns optics is correlated with greater than 2 microns optics. We verified that possibility.

Page 4, lines 15-30. This section is totally obscure as many relationships are brought in that do not directly reflect on the studies proposed here and may have some relevance

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to the material at hand but I am hard put to find relationships or relevancies. The introductory material introduces the Morel and Prieur (1977) formulas for estimating R_{rs} that depend on measurements of backscattering b_b which, however, are not used in this study. Further on, the equations (1) and (2) are supposedly used to derive Eqs. (3) – (5), the biogeo-optical (BOI) indices which do not utilize b_b . Clearly, the reader requires a derivation of how one gets from a backscattering formulation to a scattering formulation. The BOI indices are proposed to estimate changes in bulk chemical composition and size distribution of SPM. From these formulas of BOI indices to the end of the page the argument becomes increasingly obscure and hard to follow. It would help to write the variables used in the manuscript into fractions created by a math editor rather than the plethora of inline fractions. The inline fractions contribute to the obscurity of the argument. The relation between formulations with backscattering to derive formulations with total scattering have to be laid out clearly. The BOI for “size 1” and “size 2” have to be explained clearly. Does this refer to all the size fractionations attempted here or to just one or two? If so, which size fractions? Where does the polynomial function F come in and how do we get this from the derivation of Eqs. (4) and (5)? What is the relevance of Gordon's (1988) formulation for R_{rs} , in terms of b_b and Eqs. (4) and (5) in terms of b ? Again, the reader has to be led carefully from a backscattering formulation to a total scattering formulation. This section requires expansion and a total rewrite.

All this section was rewritten and BOI indexes were removed and replaced by traditional indexes used in the literature (spectral slope of particulate beam attenuation and mass-specific particulate absorption coefficient within the visible spectrum). Part of decision of eliminating BOI indexes was the lack of b_b measurements.

Page 5, line 3. Utilizing empirical relations involving POC (essentially the CO_2 from ashed organic matter) generalized to POM is difficult in marine systems because the crude relations between POC and POM are based on chemical analysis of detritus from higher plants. The relation between the two variables is not straightforward in marine

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systems because the organic content of phytoplankton differs in quantity and quality from higher plants and the various groups of phytoplankton, diatoms and prochlorophytes for example, differ from each other. I always recommend coupling POC data with POM data because of this difficulty. So relations coupling aSPM(I), CSPM, and POC become increasingly problematic and definitely region-specific. The derivation of the BOIsize indices for particle size being based on the unknown spectral slope of backscattering also becomes problematic. For that matter, there is still controversy about whether there actually is a spectral slope associated with the backscattering coefficient. These indices along with BOIcomp may be of some empirical use but they will always be regionally limited without independent information on chemical composition and size distribution to interpret them. The problem with the indices proposed here and similar indices proposed elsewhere is that they are qualitative in nature. At best, ignoring all the problems, one can only come up with qualitative “greater than or lesser than” estimates of size or chemical composition without any quantitative information which is what is needed for valid and accurate predictions of particle and biogeochemical dynamics.

We agree with the reviewer. BOI indexes are no longer part of the manuscript

Page 5, line 9. Since the empirical mass normalizations reported here do not fit the Bohren and Huffman definition of optical properties, I suggest the following, 2.6 Optical cross sections and mass-normalized coefficients, and the substitution of “optical coefficients or mass-specific ratios” throughout the manuscript when the term “IOP” is used to refer to the empirical mass-normalized coefficients or absorption and scattering coefficients determined for SPM.

Done Hopefully well understood. Replacing whenever IOP is present

Page 5, lines 10-11. The mass-specific absorption and scattering cross sections were estimated with Model I multiple regression. Just as Model II regression was used elsewhere in this paper, Model II multiple regression must be used for the best estimates

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of the mass-specific cross sections (s). The only time that Model I regressions can be used in place of Model II regressions is with a high R^2 value between the proposed dependent and independent variables, say $R^2 > 0.95$. That is not the case here. It is my experience that the best estimate of slopes (as used to estimate s values) with R^2 values as low as reported here is definitely with Model II multiple regression (Stavn and Richter, 2008; Richter and Stavn, 2014).

Sorry, model II was missing from the text. Now was added and it means that response and independent variables have a random error

Optical cross sections for chemical fractions of SPM were calculated based on multiple regression model II analysis (i.e., independent and response variables have random errors) (Sokal et al., 1995; Stavn and Richter, 2008): $Y = \beta_1 [CPIM] + \beta_2 [CPOM]$

Page 5, line 12. In light of above I suggest in this line the replacement of “optical property” with “optical coefficient.” done

Page 5, line 16. As suggested earlier, the masses used for calculating the mass-specific absorption and scattering coefficients of size-fractionated SPM should be optimized and weighted for these calculations. As mentioned before, this is not possible for all size fractions

Page 5, lines 23-25. Although the slope of the power-law formulation is often used to describe the particle size distribution (PSD), the actual PSD's for estuarine systems as reported previously (Risović, 2002; Zhang et al. 2014; Zhang et al. 2017) should be mentioned. The power-law distribution is a first-order approximation of the PSD for particles greater than about 2 mm diameter. Therefore it will often work for total scattering with calculations involving only particles in the 2 mm + range. It fails for submicron particles and since assertions are made for size fractions less than 2 mm diameter, the use of this assumption becomes questionable. Again, the use of the g slope gives a qualitative feeling for a relative distribution of large and small particles in suspension but fails when quantitative relations are desired. This is especially true

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of backscattering estimates as demonstrated by Risović (2002) and there seem to be analogies being made for parameters proposed and used in this paper that are based on backscattering. The bottom line again is a development here that is qualitative at best and regionally limited.

I guess you mean The power-law distribution is a first order approximation of the PSD for particles greater than about 2 μm diameter, greater than 2 microns, right? We included in methods the limitations of using Junge slope vs Risovic

'Although particle size distribution in natural waters may not follow a Junge-type slope, its use here was justified since our main interest was to have a first-order assessment of size effects of particulates on optical coefficient's variability'. Indeed, the calculation of τ_{λ} is only valid for particulates greater than 2 μm . A more realistic representation of PSD is the model proposed by Risovic (2002). This parameterization mainly includes two particle populations ('large' and 'small') having different refractive index and was applied for the first time in littoral environments by Stavn and Richter (2008). Thus, relationships between τ_{λ} and optical coefficients in this study are local and should not be generalized to other littoral environments.

Page 5, lines 25-27. It is important to keep in mind here that the SPM parameters proposed and the SPM relations utilized in this paper are only useful when correlated with actual determinations of chemical species and some independent estimation of the size-classes of PSD. The authors claim that functionalities between "IOP's" and BOI indices were investigated with linear regression analyses. However, I see no report of regression coefficients in the data tables, only correlation coefficients.

BOI indexes are not apart of the manuscript anymore. We do correlations not linear regressions

Page 6, lines 7-11. How were the g slope's calculated? Were they from the masses of the various size sub-ranges or from the LISST data? This is important because of the rampant inconsistencies between the size fraction masses and the g slope estimates.

C13

The mode of calculation must be delineated and the data shown in a table to be able to evaluate what is reported here. Even though the largest mass of 0.2- 0.4 mm particles is reported for the LE the smallest g slope is reported for this region. The 90% error for the g coefficient, for which the area is not delineated here, is strong evidence for the inability of the Junge-type slope to describe, even qualitatively, the PSD patterns for this study. A table is required for the g slopes and their errors.

Error and range of τ_{λ} values was added to the manuscript. Also, a detailed calculation of τ_{λ} is included

Page 8, lines 7-9. Even if the n value is low for correlations between the BOI indices and the optical cross sections, this is the only way to validate the BOI indices and the correlations should be given with proper caveats.

In Discussion we highlighted the limitations of the reduced number of samples when correlating optical proxies 'Also, the reduced number of sampling locations and the geographic variability of τ_{λ} - τ_{λ} relationships were additional factors likely explaining the lack of a general functionality for the study area'

Page 8, line 10. The Discussion section, in general, reads too much like a Results section. We should assume that the relevant statistical relations are in the results section and here we are interested only in the overall pattern of the results and the explanation of the patterns laid out in the results section.

The discussion was improved with results regarding iron and new optical proxies Comparison of our mass-specific optical coefficients and optical cross sections with those in the literature is a common procedure of discussing results in most publications

Page 9, lines 22-23. The assertion here is that a larger portion of large particles and lower g slope's (how were they calculated?) were found in the LE region. Yet Table 2 indicates that parts of the LE region had the greatest contribution of 0.2-0.4 mm particles and a contribution of particles greater than 10 mm equivalent to or less than

C14

that of SF and UE. Here is an obvious problem with the g slope. The interpretation that these large particles may have been organic in nature contributes to the uncertainty of the interpretation of the SPM-based coefficients and measurements advocated in this paper.

The calculation of the slope \bar{A}_y is described in methods. Yes, there is a lot spatial variability on \bar{A}_y but lower \bar{A}_y values were measured in LE waters. We found a general inverse correlation between CPIM/CSPM and \bar{A}_y (\bar{A}_y vs = -0.41, P = 0.049) for the study area and suggesting that relatively large particulates have an organic origin. This relationship was intensified in LE waters (\bar{A}_y vs = -0.58, P = 0.022).

Page 10, lines 16-17. Table 4 is nearly incomprehensible. The extensions of the table without the columns being identified is what makes the table incomprehensible. And again, in this section the statistical tests should be in the results section and we are interested only in the interpretations of the patterns in the results. The correlation coefficients reported in the table are low even though presumably significant. Again, if g were determined from the LISST data then any analysis of submicron particles and g is simply invalid.

The first column of the table was labeled. More statistical details were added to the text. Now is table 2. It is true that no correlations should be expected between \bar{A}_y and mass-specific optical coefficients of size fraction 0.2-0.4 and 0.4-0.7 microns cause the LISST limitations regarding submicrometric particles. However, correlations may exist due to dependencies between size fractions. In other words, $aspm^*$ of 0.2-0.4 and 0.4-0.7 microns are correlated with $aspm^*$ of 0.7-10 and >10 microns. For the case of $bspm^*$, no significant correlations were computed for 0.2-0.4 and 0.4-0.7 microns.

Page 10, lines 24-25. The theoretical calculations of Babin et al. (2003) assumed the Junge slope g when estimating particle concentrations and calculating the Mie scattering based on the particle concentrations predicted by the Junge slope. Since the PSD has been demonstrated to not be Jungian (Risović, 2002; Zhang et al. 2014;

C15

Zhang et al. 2017), especially in coastal waters, the Babin et al. (2003) results are not relevant here. Table 4 does not show any relationship between $bSPM^*$ and g, i.e. g is not in the table at all. This closest approach is apparently in Table A1. Here we see that the correlations of the optical coefficients of nominal size fractions of SPM and g vary all over the map, from positive to negative, significant and non-significant, not at all supporting the hypothesis of g being a significant and explanatory variable in this analysis. This also falsifies the hypothesis that absorption coefficients and ratios are parameters of use in general models of the occurrence and dynamics of suspended matter.

The sentence about Babin et al. (2003) was deleted in discussion

Page 11, lines 2-4. Again, the empirical indices proposed in this paper are poorly described and defined. What do the superscripts "size 1" and "size 2" mean? The BOI indices may be of some utility but again, like all similar indices based on empirical coefficients of total SPM, they are strictly qualitative in nature. The unknowns in the bulk coefficients in their definition will always cast doubt on their interpretation if ancillary evidence on PSD and composition are not available.

BOI indexes are not anymore part of the manuscript

Page 11, lines 20-21. Suggested wording: These relationships will be useful in investigating local and regionally-limited relationships and properties of SPM. Without separate independent studies of true optical properties of PIM and POM, and of PSD, these relationships will remain problematical.

We added the first sentence to the end of the conclusions paragraph

Technical Corrections: Page 2, line 5. Bowers et al. (2009) reported estimates of mass-specific scattering coefficients and biogeo-physical characteristics of PIM, not SPM.

Ok corrected Page 6, line 26. Replace "properties" with "coefficients." done

C16

Page 9, lines 9-10. The English usage here is nearly incomprehensible. Correct this and similar constructions with a native speaker of English.

Done

Pages 19-30. The tables presented here are nearly impossible to interpret. The table extensions to multiple pages have incomplete columns and no captions to the columns. The the table captions are limited and cryptic.

Many tables were removed to simplify content. Also, more labels were added to identify columns

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-159>, 2017.

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