

Interactive comment on “Inversion of the volume scattering function and spectral absorption in coastal waters with biogeochemical implications” by X. Zhang et al.

Anonymous Referee #1

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I. General comments.

The manuscript presents various bio-optical properties of suspended matter (e.g., volume scattering function-VSF, absorption and scattering coefficients) acquired in a coastal environment, namely the Chesapeake Bay (USA). The inversion of the in-situ measurements is performed using a previously developed methodology (Zhang et al., 2012) to retrieve biogeochemical parameters (chlorophyll concentration) and some pieces of information on the particle size distribution. The main results of the study are as follows : (i) the chlorophyll concentration could be derived satisfactorily from the VSF in coastal waters despite the fact that this parameter does not primarily drives the variation of the VSF in the study area, (ii) it is suggested that one fraction of the Colored

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Dissolved Organic Matter (CDOM) could induced some significant backscattering in such environment while CDOM is often considered as a non-scattering material when dealing with open ocean water type.

The manuscript is very well written, documented and pleasant to read. In particular, the authors clearly justify the assumptions or approximations they do; they discuss as well nicely their results over the course of the manuscript. They provide good perspectives to their work. Despite the fact that the methodologies used in this manuscript are not new (they were already published previously), the interpretation of the inversion of the bio-optical properties of marine particles in term of biogeochemical applications is of great interest for the community. From this point of view, the manuscript is original and is a significant contribution to the ocean optics field of research. I do not see any major errors in their analysis. Based on the high quality and on the originality of the discussion of the results outlined in the manuscript, I recommend the manuscript for publication in Biogeosciences journal. I have a few general comments and many specific comments (section II) which could help to improve the manuscript and which should not be so difficult to address by the authors.

1. Influence of the multispectral data on the VSF inversion results.

The instrument that is used to measure the full VSF, the MVSM instrument, is able to measure the VSF at several wavelengths which are of great interest for the objectives of the authors. However, the multispectral information is not really exploited in the inversion technique (just one wavelength, namely 532 nm, is used). This is too bad since such spectral information could help to better constrain any inversion technique to increase its performance. As an example, the wavelength dependence of the VSF might be useful to better identify the type of marine particles, the size of particles and the absorption effects of particles (as shown for example by Chami et al., "Spectral variation of the volume scattering function measured over the full range of scattering angles in a coastal environment," Applied Optics, 45, 3605-3619, 2006). So, it would be interesting to mention in the manuscript whether (and how) the inversion technique

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could be improved by taking into account all the multispectral VSF information. Would it be feasible to include all the wavelengths in the inversion procedure ? If yes, do the author could mention if the performance of their inversion procedure would be weakly or significantly improved ?

2. Consideration of VSF shapes for mineral-like particles.

It seems that the inversion procedure does not take into account the VSF of inorganic mineral-like particles for which the angular distribution of the scattering properties could be significantly different from that of phytoplankton-like particles. In the manuscript, it is mentioned (end of section 2.4) that one limitation of the inversion technique is that the NAP (Non-Algal Particles) that are modelled have a similar angular distribution of VSF as phytoplankton (or maybe, I misunderstood). However, in their study area, it is likely that mineral-like particle could represent a significant fraction of the suspended matter (as the authors say, the contribution of phytoplankton-type particles is not dominating in these coastal waters). It would be interesting if the authors could simply clarify or discuss how the mineral-like particles are accounted for in their inversion procedure since these particles should play a major role in the scattering properties of the study area (i.e., the scattering coefficients values are high while they are not correlated with chlorophyll).

II. Specific comments

- Title (p.9003) : When reading the title, one could think that an inversion method is developed and applied to the retrieval of inherent optical properties and biogeochemical properties of particles. However, the manuscript is not really dealing with the development of an inversion methodology since it was previously published. I suggest to slightly modify the title which should focus more on the new results obtained in the manuscript.

- p. 9006, line 1 : I think that the word “absorption” is missing after “NAM”

BGD

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- p. 9007, line 16 : the reference Twardowski et al., 2012 is missing in the reference section at the end of the manuscript.

- p.9009 : line 10 to 17 : since the samples were collected in turbid waters (i.e., the scattering coefficient values are up to 4 m^{-1} , see figure 3), it is likely that multiple scattering effects could significantly alter the measurement of the VSF using the MVSM instrument (figure 4 shows a “smooth” VSF with angles). In other words, the VSF is supposed to be measured for a primary scattering regime of ambient light and turbid samples may change this primary scattering regime into a strong multiple scattering regime within the chamber of the instrument (despite the pathlength of the instrument is relatively low). So, the measured VSF may not be fully representative of particulate VSF. Did the authors use a specific protocol for measuring the VSF of turbid samples ? Since the MVSM measured continuously near the surface, it was probably not possible to dilute the samples to reduce the multiple scattering regime. At least, did the authors perform some experiments in the lab using beads to determine a threshold concentration of suspended matter from which the multiple scattering processes strongly affect the VSF measurements by MVSM ? It would be interesting if the authors could discuss this point relative to the possible influence of the multiple scattering effects on the VSF measurements in the manuscript; at least, they could mention that multiple scattering effects could be a limitation of the VSF measurements in their study area.

- p 9009, line 24 : “no scaling was applied to the data from either instrument” :does it mean that the LISST and the MVSM instruments were systematically consistent and matched exactly in term of absolute geophysical values of VSF (in $\text{m}^{-1} \text{ sr}^{-1}$) ? One could expect some discrepancies between both instruments (even within few percents) which should not help the merging of data of both instrument unless the data are normalized to a given scattering angle. How was the MVSM calibrated in geophysical units (did you use beads calibration for that ?) ? Did you normalize the VSF data to a given scattering angle (e.g., 90°) to perform the merging between LISST and MVSM ? You need to clarify with one or two sentences these points in the manuscript.

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- p. 9011, line 17 : “Snap < 0.012 m⁻¹” : Is this upper limit for Snap not too weak compared to what was found in other coastal waters (Snap up to 0.019) (e.g., Babin et al., 2003) ?

- p. 9012, line 18-19 : Here, the authors seem to compare optical properties of mineral aerosols (including in polarization states) with oceanic hydrosols (phytoplankton and NAP). It is confusing since the hydrosols optical properties may significantly differ from those of aerosols (especially in their polarization states). I understand that it is difficult to know what shape of particles should be used for marine particles but it may be confusing here to suggest that oceanic particles could have similar properties as aerosols. I do not question what the authors did but I just think that this sentence should be moderated and re-written to highlight that aerosols and hydrosols optical properties are different.

- p. 9013, line 7-10 : based on their results, it seems that NAP particles could be mostly inorganic and mineral in the study area. However, it is not clear to me if the authors used optical properties which are representative of mineral-like particles. How did the authors account for mineral-like particles in their inversion procedure (see also my general comment #2) ?

- p. 9014, line 19 : I agree with the authors that the errors associated with the omission of angles close to 0° and 180° are small when calculating the scattering coefficient from the integration of the VSF. One reason could be because due to the fact that the VSF is weighted by a sinus function in the integral which thus reduces the importance of the VSF values when summing the integral to get the scattering coefficient.

- p. 9015, line 28 : “multiplied by ~1.20” : this number is interesting since it could be informative on the error bar of AC-S measurements of the scattering coefficient. Is it right ?

- p. 9016, line 5-18 : As discussed by the authors here, it is not easy to perform an optical closure and to find consistent results between various instruments. The

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agreement they have is satisfactory to my point of view. It is likely as well that the noise in their data is probably reduced because the samples are collected in turbid waters which induce strong scattering signals. Perhaps, the bias between the instruments would have been larger in clear waters.

- p. 9017, line 1 : “the modelled VSF . . .” and p. 9037, figure 4 : it is very difficult to distinguish the modelled VSF (dashed line) from the measured VSF in figure 4. To better see both VSF, the authors could plot the measured VSF using dots and degrading the angular resolution of their data (i.e., plot the VSF for an angle step of 1° for example) and superimposed the modelled VSF using a solid line so one could better see the comparisons between modelled and measured VSF.

- p. 9017, line 13 : “ the angular scattering in the backward directions...increased” : this statement is not so obvious to see in figure 4. Could you clarify ?

- p. 9017, line 24-25 : here, you show that phytoplankton is not the dominant particle type. So, it corroborates that the limitation of the VSF inversion procedure (p. 9013, line 9-10) to take into account a different angular scattering shape for VSF of NAP relatively to that of phytoplankton may be a critical point. It would be interesting if the authors could discuss in few sentences the possible impact of this limitation on their results with regard to the fact that NAP contribution is dominating in the coastal waters of their study area.

- p. 9018, line 8-28 : the results discussed here are consistent to what we could expect. The explanations provided by the authors are convincing.

- p. 9019, line 4-5 : the values of the backscattering ratio used for NAP (0.002 to 0.005) seem weak if inorganic material such as mineral like particles are a major contributor to NAP. If mineral like particles are present in the study area, they may have a much higher backscattering ratio (it could reach sometimes several percents) while the values used here are less than 0.5% which are typical of organic material. Perhaps, the mineral like particles are included in the Very Small Particles (VSP) component ? The values

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used for phytoplankton backscattering ratio (0.0007 to 0.0034) seem weak as well for this type of particle where we could expect values about 5 times larger (up to 0.015). It would be good if the author could justify briefly the choice of their values that do not seem to be in the representative range of variation for this type of water.

-p. 9019, line 17 : “. . .hexahedral shape. . .” : See my previous comment about the possible confusion between the oceanic hydrosols and aerosol optical properties.

- p. 9020, line 20-26 : the results obtained here seem counter-intuitive. I would not expect that VSF could be inverted to retrieve satisfactorily the chlorophyll concentration in waters dominated by NAP contribution; the variation of VSF is not primarily driven by chlorophyll (at a 1st order). Do the authors use a specific additional constrain on the inputs parameters of their VSF inversion procedure that could explain the fact that their method is highly sensitive to variables which drives at second (or third) order the variations of the VSF ?

- p. 9022, line 10 : “fig. 6” : I think that the sentence refers to figure 5 rather than figure 6 ?

- p. 9023, line 23-25 : “. . .VSF method was demonstrated. . .” : I suggest here to moderate the sentence and to use the verb “highlighted” rather than “demonstrated” since the explanation given for the idea mentioned in this sentence is not so convincing (see my previous comment on p. 9022).

- p. 9037, figure 4 : see my previous comment of p. 9017 with regard to the quality of the plots to better distinguish between the modelled and measured VSF.

Interactive comment on Biogeosciences Discuss., 10, 9003, 2013.

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