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Ocean Science Discussions

Interactive comment on "An optical model for deriving the spectral particulate backscattering coefficients in clear and turbid coastal waters" *by* S. P. Tiwari and P. Shanmugam

Anonymous Referee #4

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In this paper, the authors have developed an empirical model to assess the particulate backscattering coefficient at 530 and 555 nm from the diffuse attenuation coefficient at one given wavelength (490 nm) in clear and turbid coastal waters. This is based on simple linear relationships between bbp and Kd performed on the NOMAD data set. Comparison with some previous algorithms is also provided. While the present topic is totally suitable to the present journal, and is an important topic for the international optics community, several major remarks should be taken into account prior this paper can be considered for publication. Minor remarks are also listed below.

Major Remarks:

A strong theoretical background is missing to first explain the limitation of a model which use one input to get two outputs, and very importantly to explain why bbp and Kd are significantly correlated (I'm very surprised about that), as Kd is also, and in many cases, driven at first order by the absorption coefficient (Kirk, 1981;1984, 1991, Morel and Loisel, 1998; Lee et al., 2005). For instance, they should discuss the value of their constants in comparison with those appearing in the model of Lee et al. (2005), which provides Kd as a function of a, bb, and sun angle. If we do a parallel with the present model and the Lee et al. (2005)'s model, the offsets (-0.000162 at 532 nm and -0.000157 at 555 nm), should varies as a function of the absorption coefficient, and sun angle. Besides, the problem with absorption, it is well know that Kd is also a strong function of sun angle. How do the performance of the model is affected by sun angle and absorption variations. It is fundamental to examine how the constants values (the two for each wavelength) of the model vary with the absorption coefficient and sun angle.

They should test their formulations on the IOCCG synthetic data set which is available (http://www.ioccg.org/groups/OCAG_data.html). This is a truly independent data set, and can be done quickly. The advantage of this data set is that it is error free (synthetic) so can be used to truly assess the performance of the model.

The authors should clearly show the range of variability of the different data sets (histograms of all data sets and parameters in log scale). They should also more carefully check the independence of these different data sets (not sure that they are totally independent). Moreover, they use the data set nomad-A to build their model at 532 and 555 nm, and test the retrieval accuracy on the same data set but at the other available wavelengths. The problem is that in NOMAD, a fit on the data has already been performed, meaning that the bbp values are all dependant, once you have calculated the slope (calculated using 532 and 555 nm).

It is not clear whether or not the construction and performance of the model are done using the true measured Kd available in the different data sets, or the estimated Kd using the Mueller formulation. It is well know that this formulation does not work in turbid waters as recently shown in Jamet et al. (2012). How the performance of the model is affected by the Kd retrieval errors ? The impact of another algorithm to retrieve Kd should be tested.

Minor Remarks:

The authors should give the range of applicability of their models as clear oligotrophic waters generally have bbp values of about 0.0002 to 0.001 m-1 (Antoine et al., 2011; Loisel et al., 2011), and turbid waters have values above 0.03 up to 0.3 m-1 (Neukermans et al., 2011; Boss et al., 2009). Their bbp values presented in this paper cover a restricted range of bbp values: 0.0003 to 0.01 m-1. It does not cover turbid waters. The bbp/Chl value of 0.009 mg-1m2 can be used to discriminate case 1 and Case 2-very turbid waters (Loisel et al., 2010).

Introduction should be re-written, as many references are missing, and there are many inaccurate statements which may induce in error the reader. They are listed below: The authors should explained what they name “clear water”. If it is oligotrophic water, the retrieval of bbp is still very challenging (see Antoine et al.,2011).

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Line 21-24: Should be reformulated as bbp depends, at first order, to the particulate concentration, and to second order to the chemical composition (index of refraction), PSD, and structure of the bulk particulate matter (Stramski et al., 2004).

Line 24-24: Recent field measurements showed that particles smaller than 3 microns contribute to about 50% of bbp, in contrast to the results based on Mie scattering theory (Dall’Olmo et al., 2009).

P263 Line 1-5: Inexact sentence, it should be reformulated. It is not only the proportion of the living vs. non-living fraction which explain bbp, but first the concentration. Line 7: Atmospheric deposition can occur in open ocean waters (see Loisel et al., 2011 about

the particulate backscattering anomalies). Line 18 : You should give the references where it is stated that empirical algorithms give Âń better estimates of bbp in clear ocean waters”.

P264/265 The authors used the Smith and Baker bw values. They should discuss their choice, as new and more accurate measurements (the more recent ones are those of Zhang et al., 2009) are now available for pure sea water; which can make a big difference in clear waters (see discussion in Twardovski et al., 2007). They should check which bbw values have been used in the NOMAD data set during the processing of the data.

P266: Note that the model of Loisel et al. (2006) allows bbp to be retrieved at different wavelengths because it is not based on prior spectral assumptions on the bbp specral shape.

P273: "Though several models are available to retrieve bbp as the function of chlorophyll concentration or spectral remote sensing reflectance, none of these models provide bb values over the entire visible spectral bands that are available with satellite sensors such as SeaWiFS, MODIS and MERIS". This statement is wrong, as QAA and GSM (actually done for this paper), and many other inverse methods, provide also bbp at different wavelengths.

P274: "The present study is expected to form the basis for robust relationships between bbp and Kd in a wide range of coastal and open ocean waters. More measurements of these optical properties in typical coastal waters will allow the refinement of the new model which can be used to derive information on the refractive index and particle size distribution based on certain optical models to study the particle populations and their characteristics in coastal waters". Prior to any refinement based on new measurements, the authors should first examine the effect of a and sun angle on their relationships.

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