Ocean Sci. Discuss., 10, C108–C115, 2013
www.ocean-sci-discuss.net/10/C108/2013/

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

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Received and published: 29 March 2013

Dear Editor,

We thank the reviewers for their valuable comments and suggestions.

Suggested additional references have now been added and referred in the text. The reviewer suggested emphasizing the limitation of a model which use one input to get two outputs, and to explain why bbp and Kd(490) are significantly correlated: A summary description of the problem has now been included, and appropriate references provided. Most of the queries and comments raised by the reviewers are addressed in the revised manuscript. Brief answers and explanations are also provided in this

document.

With reference to the IOCCG Report 2013: This model has the potential to derive the particulate backscattering coefficient in oceanic waters (the backscattering coefficient (bb) varies from 0.0003-0.1 m-1 reported in IOCCG Report 2013, pp. 24).

The paper title can be modified in the final revised version due to the limitation of the model over turbid and highly turbid waters. The title can be as follows: "An optical model for deriving the spectral particulate backscattering coefficients in oceanic waters".

C. jamet (Referee)

General comments:

The manuscript could be improved. It lacks references, prior Shanmugam et al. (2011). The dataset description is not at all clear. The authors should really state the differences between the three datasets. For my knowledge, NOMAD-A corresponds to SeaBASS and NOMAD is a sub-dataset of SeaBASS, corresponding to a match-up exercise. It's very confusing in the text. So I am not sure that the three datasets are truly independent. Moreover, I am not sure that these datasets have a large set of data obtained in turbid waters (this is shown through the Kd values with most of the data below 0.15 m−1). It would have been interesting to test the new method with the synthetic dataset of the IOCCG. The accuracy of the new method presented by the authors could be directly compared to the IOCCG report. So I sort of disagree with the authors when they state that their new model is also suitable for turbid waters.

The description of the algorithm is not very clear and would deserve a couple of references or a flowchart. They present a validation of their algorithm with NOMAD-A. But this dataset has been used for the determination of their new algorithm. It can not be considered as a true validation. I would remove this part or the authors should explain why they think it's important to present this part.

I think the figures could be better presented, moving the legend on the top-left and choosing other colors. It's very difficult to see the different algorithms and so very difficult to see the differences between them. Moreover, I don't think that the statistical parameters need so many numbers. 2 or 3 significative numbers should enough, especially for R2, slope and MRE. All the statistical parameter seems to be calculated with the log value of bbp, explaining the low values of MRE. What is the impact? What would the results be in the authors would have chosen the true values of bbp?

The main point of the method is to relate Kd(490) to bbp(530) and bbp(555). The relationships between those parameters are simple. In the entire manuscript, the authors do not discuss the impact of using an intermediate step for estimating the values of bbp. Moreover, they use an outdated version of Kd(490) (see http://oceancolor.gsfc.nasa.gov/REPROCESSING/R2009/kdv4/) and it is know that the algorithm of Mueller is not suitable for coastal waters (Lee et al., 2005; Jamet et al., 2012). We know that Kd from Mueller estimates Kd with a 30% error. How does it impact the final results? If the authors take another Kd(490), what would the results be? This should be included in the discussion part. At last, it is not clear at all which Kd the authors took for their validation exercises. Is it calculated from the Rrs? Moreover, as shown by Lee et al. (2005), Kd is related to a and bb. How does this relationship can impact the relationships?

The proposed model is based on the power function (i.e., f (X) = a [X]ˆb + c), not a simple linear equation. The offset values are considered constants in the present study because there is weak dependency of model on solar zenith angle and absorption, only the scattering effects are dominant on Kd at 490 nm.

The relationship between bbp and Kd(490) is derived, exploiting the fact that the inverse spectral dependency exists for particulate backscattering coefficient (bbp) (i.e.,). In such case, the optical variability in the geometric structure of the underwater light field is governed by the relative intensity of 'bbp', whereby leading to the formation of isotropic region. The diffused field so formed becomes less dependent on illumina-

tion geometry and solar zenith angle at blue wavelength (Zheng et al., 2002). The dominant effect of scattering over absorption process at Kd(490) relates a very weak dependency of solar zenith angle and absorption on Kd(490). Therefore, it is of prime importance to study the change in apparent optical parameter Kd(490) and the spatial effect induced due to particulate backscattering (bbp) to the geometric structure of the underwater light field, in proper correlation.

Zheng et al. (2002) discussed that a weak dependency of Kd at blue wavelengths based on early experimental (e.g., Hojerslev et al. (1974) in clear water off Sardinia, Nelson and Aas (1977) in turbid Oslofjorden waters, and Baker and Smith (1979) in San Vicente Reservoir near San Diego) and model results reported by different researchers, indicated that the effect of solar elevation on Kd is relatively small. It is also studied that at shorter wavelengths, Kd has been considered to be independent of solar zenith angle. However, more recent experimental results based on the time-series data collected with moored instruments in the upper layer of the Sargasso Sea showed a significant correlation between Kd and solar zenith angle, especially at the red wavelengths (Stramska and Frye, 1997).

The construction and performance of the model are done using the updated version of Mueller (2000) formulation updated versions [http://oceancolor.gsfc.nasa.gov/ANALYSIS/kdv4/]. Mueller (2000) proposed Kd at 490 nm which is derived using the remote sensing reflectance band ratios at two spectral bands 490 and 555 nm. The derived Kd (490) uses as input to estimate the bbp at 530 and 555 nm. Theoretically, it is well understood that the effects of absorption properties are more in the shorter wavelength region (e.g., at 412 and 443), there is a severe problem to partitioning individual materials in coastal waters. Furthermore, atmospheric correction errors increase with the decreasing wavelengths, with the 412nm band being the most affected one (Shanmugam, 2012). Therefore, the bands 412 and 443nm are not used in this study. Since there is a minimal effect of CDOM, suspended sediments and other coastal waters materials at 490, 555 and 670nm, these bands are used for modelling. Though several algorithms exist in the literature to estimate the Kd(490), we got maximum number of data sets for Mueller (2000) algorithm. Moreover, the Mueller (2000) model derived Kd(490) values showed excellent agreement against NOMAD-A in-situ data which is used for model parameterizations. More accurate diffuse attenuation coefficient at 490 nm would give more accurate bbp values. Why we used Mueller (2000) Kd(490) is that this algorithm is globally accepted and routinely used to produced standard Kd(490) products.

Note: True measured Kd (490) as well as Kd(490) computed by using any other algorithms can also be used to estimate the bbp.

The authors state that they have developed a spectral algorithm but for my understanding, it also estimated bbp(530) and bbp(555) and then they use the classis relationship between bbp(λ) and bbp(555).

The spectral backscattering spectra follow the classic power-law function, thus the governing equation is the same in the present study, but we introduce new parameterizations which differ with other models. As demonstrated in this study, the proposed algorithm is well correlated with particulate backscattering at 530 and 555 nm.

Moreover, why did the authors not include the model developed in 2011 in this paper?

The model developed in 2011 by Shanmugam et al. (2011) is based on the optimization iterative method, which provides bbp in the blue-green wavelength domain. The present study is different from the optimization model of Shanmugam et al. (2011) and the parameterizations used in the present study are also different.

Even if the work is timely and interesting, I recommand major revisions.

Minor comments:

page 263, line 12: I think other authors showed that before 2011. Add adequate references

page 264, line 20: as stated previsouly, the different datasets should be better described, especially the OOXIX one. Add adequate references also.

References are already inserted in the text.

page 268, first paragraphe: the explanations are not very clear to me. Need references page 268, line 19-20: The statistics are not the same that are presented in Table 1, while it is the same dataset. Could the authors explain why? page 269, line 19, 20: What do the authors mean by "systematic and random errors"

The root mean square error (RMSE, random error) and mean normalized bias (BIAS, systematic error).

page 270, line 14: References are missing for the three methods.

(http://www.ioccg.org/groups/software.html) Z. Lee, “Remote Sensing of Inherent Optical Properties: Fundamentals, Tests of Algorithms, and Applications”, IOCCG, Dartmouth, NS, Canada, IOCCG Report 5, 2006.

page 271, subsection 5.1: Figure 3 is not very easy to compare. They are too many spectra. The only thing that can be said is that the spectral shape is correct. Did the authors look at the spectra that are very different? I am not sure this sub-section is necessary.

This figure is plotted to show the spectral variation of in-situ and modelled bbp in the entire visible wavelengths using the same datasets used for the model development (but using bbp at only two wavelengths).

page 271, subsection 5.2: I disagree with the fact that the authors name this part, validation as they use the datasets used for the development of their algorithms. So they use the same datasets to validate their algorithm. It is not at all a validation. The axis of Figure 4 could be narrowed, there is no obvious reason for not having the axis between 0.0001 and 0.01 m−1. These scatterplots in Fig. 4 are shown to demonstrate how closely the model results match with in-situ measurements (from which bbp at 530 and 555nm are actually taked and used in the model development). These plots do not show independent validation of model; however they give insight into the model performance across the entire visible wavelengths. It also shows how the new parameterizations based on the selected wavelengths perform well at other independent wavelengths. Axis can be scaled between 0.0001 to 0.1 m-1.

page 271, line 26: How do the authors explain that their algorithm is wavelength dependent, i.e. why the RMSE is not stable with the wavelength? Moreover, the RMSE is higher than the values of bbp by itself. Is it because the RMSE is calculated in log?

The present study suggests estimating the slope and bbp values at a reference wavelength (555 nm) based on the relationships between bbp and Kd (490). Thus, the new model is based on only the two selected wavelengths and diffuse attenuation coefficient at 490 nm. All the statistical analyses have been performed on the log-log data. The theoretical aspects of the model are already explained before.

page 273, line 2-3: I sort of disagree with the authors. If one looks at table 2, GSM provides better estimates than the new model for _=490, 510, 555 in term of RMSE, slope and R2. The new model is overall slightly better than GSM in term of RMSE and is less biased. But the slope, intercept and R2 are worse.

The revised manuscript presents the validation considering all the data from each data set (without eliminating data corresponding to LM model, which produced abnormal bbp values). The statistical results are better for the new model.

page 272-273, subsection 5.3: I am a bit surprised of the results presented in figure 7 and 8. If I understood well, NOMAD-C is the OOXIX datasets and NOMAD-B is the match-ups datasets. So the results show that using satellite Rrs lead to better results than using in-situ Rrs. In a way, it does not make sense as the in-situ Rrs are "true" while the satellite Rrs have an error between 10 and 50%. Is it the case or did I misread it? If it is the case, how can the authors interpret this result?

I would suggest reviewer to check the table 2 and 3, where one can see that using in-situ Rrs leads to better results than using satellite Rrs. It is also clear from the respective tables.

page 273, line 15-27: this paragraph should be in the introduction and not in the discussion. Nothing is discussed there.

This paragraph is in connection with the importance of bbp model which is discussed subsequently in the same paragraph. Thus this sentence is retained in the same place to show continuity.

page 274, line 6: I disagree that the new algorithm predict the spectral variability of bbp. The new method allows to estimate bbp(530) and bbp(555). Then the author use the classic relationships between bbp at two wavelengths.

This manuscript aims to present a new model for estimating the spectral particulate backscattering coefficients in the visible wavelengths domain. The present model has the potential to derive the bbp at any wavelengths in the entire visible domain, which means that this model could be used to study the spectral variability of bbp which will have important implications in various bio-optical model and remote sensing applications.

page 275, line 8: "Antoine", instead of "Antonie"

It is corrected in the revised manuscript.

The authors wish to thank the reviewer for his insightful efforts and positive contribution to the outcome of this paper.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/10/C108/2013/osd-10-C108-2013-supplement.pdf

Interactive comment on Ocean Sci. Discuss., 10, 261, 2013.