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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

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This manuscrupt aims to present a new empirical method for estimating the spectral particulate backscattering coefficients in the visible. The diffuse attenuation coefficient K_d(490) is related to b_{bp}(555) and b_{bp}(530). These latter parameters allow to estimate b_{bp} at any wavelength using the classic power-law function. The authors state that their new method allows to estimate b_{bp} in clear and turbid waters. It is very useful and relevant for the ocean color community. So it is timely. The statistical results are very promising, the authors comparing their method to three other weel-established algorithms using three datasets.

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 independant. 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.

T 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 seem to be calculated with the log value of b_{bp}, 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 b_{bp}?

The main point of the method is to relate K_d(490) to b_{bp}(530) and b_{bp}(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 b_{bp}. Moreover, they use an outdated version of K_d(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 K_d(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), K_d is related to a and b_b. How does this relationship can impact the relationships?

The authors state that they have developed a spectral algorithm but for my understanding, it also estimated b_{bp}(530) and b_{bp}(555) and then they use the classis relationship between $b_{bp}(\lambda)$ and b_{bp}(555)

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

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.

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

page 271, subsection 5.1: Figure 3 is not very easy to compare. They are too mlany

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.

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}.

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 b_{bp} by itself. Is it because the RMSE is calculated in log?

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 λ=490, 510, 555 in term of RMSE, slope and R^2. The new model is overall slightly better than GSM in term of RMSE and is less biased. But the slope, intercept and R^2 are worse.

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 didI misread it? If it is the case, how can the authors interpret this result?

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

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

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

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