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Interactive comment on "A semi-analytical model for diffuse reflectance in marine and inland waters" *by* J. D. Pravin et al.

Anonymous Referee #1

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The work presents an advancement of the author's previous model for irradiance reflectance in water [Dev (or Pravin?) and Shanmugam, 2014b]. It is indeed very similar and the claimed innovations and advantages are neither properly highlighted nor discussed with inter-comparison. However, the main criticism concerns the general uncertainty analysis of the new model. It is stated that the agreement with in situ measurements is in general good, but the analysis comes to short, also with respect to existing models from other researchers. Specific comments:

1. R is usually referred to as (spectral) irradiance reflectance or, alternatively, irradiance ratio. The term "diffuse" reflectance (also in the title) suggests that direct parts of the downwelling irradiance are not taken into account. Consult the oceanopticsbook.info for widely used terminology.

2. The definition of R should appear in the abstract.

3. Abstract: "The model is valid for a wide range waters within coastal and open-ocean environments." And inland waters – as the title suggests – too?

4. Page 1894, 26: R is a measure of how much of the radiance travelling in all downward directions is reflected upward into any direction. The definition provided here is not clear. Also, if R varies between 0 and 1, how can it beyond 1?

5. Page 1895, 28 ff: The innovation of the article or model is not totally clear to me. Differences to the previous model must be clearly highlighted and justified by means of validation to show the improved performance. To include measured IOPs does not automatically mean that the model is more accurate. This has to be proven by careful inter-comparison.

6. In the end, it is still not clear to me how the model works. What exactly are the input parameters; IOPs (absorption and backscattering coefficients) with sun zenith angle (introduce the parameter theta_s!) and chlorophyll concentration? Are the coefficients spectrally or at 400 nm only?

7. How is Kd and Ku determined and defined in this work?

8. The attenuation coefficient, which is measured with an AC-S, the backscattering coefficient, which is measured with a BB-9 (at nine channels), and chlorophyll concentration, measured with FLNTU based on – as I think – insufficiently understood fluorescence, have – as I think – a very high uncertainty level. For example, it has become clear that in situ absorption measurements using the AC-S are subject to significant potential errors associated with imperfect correction for scattering artifacts (McKee, D., Piskozub, J., & Brown, I. (2008). Scattering error corrections for in situ absorption and attenuation measurements. Optics express, 16(24), 19480-19492.). A discussion section should be devoted to the summation of uncertainties from each model input parameter. How sensitive is the model to input changes.

9. Page 1899, 3 ff: I don't really understand why the absorption coefficient at 400 nm is chosen. The mentioned dominance of pure water absorption starts – in the presence of phytoplankton – rather beyond 500 or 600 nm, depending on the concentration. The 400 nm spectral range is strongly affected if CDOM (and also mineral absorption) is present. From a remote sensing point of view, this spectral range would be a rather bad choice because of critical aerosol-depending atmospheric correction. Please specify your selection.

10. Page 1900, 1 ff: The irradiance ratio, R, is probably not equal throughout the water column; near the surface it might differ because of total reflection of upwelling radiation and thus additional downwelling radiance contribution. However, it's quite homogeneous a bit deeper. Fluctuations of R in the water column are also associated with possible insufficient averaging of downwelling irradiance measurements, which exhibit large variability due to wind-related light fluctuations in the upper layer. This is why the measurement method of R should be mentioned and carefully discussed. In the corresponding Figure 4 necessary information on water depth, wind speed, and stratification of IOPs are missing. If the depth is 6 to 10 m only (near shore), we could assume strong vertical mixing of the whole water body due to waves and thus rather well mixed water?

11. Figure 4: Which site with what IOPs is shown? Could you include the vertical distribution of IOPs used for the model! What are the related cases in Figure 3? It would be in addition helpful if you could include the percent relative difference of model vs. in situ data. The differences seem to be larger than normally considered as "generally good" (page 1902, 24), see e.g.: Zibordi, G., Donlon, C. J., & Parr, A. C. (2014). Optical Radiometry for Ocean Climate Measurements (Vol. 47). Academic Press. Mean but also highest values should be reported for all water types and carefully discussed.

12. The model accuracy should be shown in detail for all water types. It would be desirable to see, where the model fits best and where other models, e.g. optimized for the open ocean, are able to keep up (e.g. similar to Dev, P. J. and Shanmugam,

P.: New model for subsurface irradiance reflectance in clear and turbid waters, Opt. Express, 22, 9548–9566, 2014b)

13. Table 1: Is the table related to Figure 4? Could you provide percent relative differences too? It seems to be better to distinguish water types too.

14. Figure 3: Could you provide further information in a table: a, c, bb, Chl, TSM, theta_s, and turbidity!

Interactive comment on Ocean Sci. Discuss., 12, 1893, 2015.