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Interactive comment on "Modelling of underwater light fields in turbid and eutrophic waters: application and validation with experimental data" *by* B. Sundarabalan and P. Shanmugam

Anonymous Referee #1

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The paper presents a radiative transfer model for aquatic media, obviously capable of simulating radiometric quantities in clear oceanic waters, coastal and eutrophic waters. The achievement of the work is an apparently more reliable inclusion of vertical profiles of optical properties of the water column. The introduction gives a review on existing radiative transfer models and methods, e.g. based on Monte Carlo method, discrete ordinate method or "invariant imbedding technique" (Hydrolight). The article suggests that "these RT models developed based on numerical as well as analytical solutions perform well in clear oceanic waters but have limitations in turbid coastal and productive waters", and that "the key problems (. . .) include the assumption of flat or randomly chosen slope of the sea surface, the treatment of material reflectance instead of the effective bottom reflectance (. . .), the constant phase function along the depth and the inadequate source function (. . .) which introduce significant errors in the simulated underwater light fields". "Thus, a reliable RT model is needed accounting for the vertically varying IOPs and treating the surface and bottom boundary conditions adequately in order to provide accurate underwater light field data in turbid coastal waters". This statement is the leitmotiv of the present paper – but I have trouble with it. I thoroughly believe that the RT models could have increasing uncertainties in strongly absorbing and scattering (i.e. eutrophic) media. But to my knowledge this has less to do with the mentioned "key problems", but rather with Monte Carlo noise or model-simplifications of the real world, e.g. the scattering function itself. One of the mentioned RT models is Hydrolight, a widely-used quasi-standard model for ocean optics applications. The literature contains many comparisons between Hydrolight predictions and measurements – also for optically complex waters (see references at http://www.oceanopticsbook.info/view/radiative_transfer_theory/level_2/hydrolight). Hydrolight explicitly allows including vertically varying IOPs (e.g. from an AC-S). There are many options for phase functions (also the here used Fournier-Forand model), bottom reflectance and so on. In the first place, the model output accuracy depends on reliable model input. Maybe I don't understand the whole concept of homogenous and inhomogeneous effects in the water column – as far as I know, Hydrolight calculates the entire radiance distribution (direct and diffuse terms) and is able to treat well-mixed (homogeneous) and horizontally stratified (inhomogeneous) water bodies. And as far as I know, the same holds true for the other mentioned RT models. Nevertheless, the presented RT model is capable of reasonably simulate the under- and above water light field. I was not aware of the used RT approach and I think this is worth to draw attention on it.

Specific comments:

1. Chlorophyll and suspended sediments are identified to be most important in coastal waters, but what is with colored dissolved organic matter CDOM, which is also en-

hanced especially near river run-offs. It is stated that CDOM absorption is measured. Than these values should be included in Table 2 and be further discussed.

2. Regarding Data and methods, it would be helpful to further specify the applied methods to derive chlorophyll concentration, CDOM absorption, etc., since the validation with experimental data is one key point of the work.

3. Some of the equations lack good description. All variables need to be specified for example: Eq. 1 (VSF?), Eq. 28 (Es?..), Eq. 32 (SnLw), Eq. 34 (Cm, zm), Eq. 35 (SSsur, zsur), . . .

4. Regarding Eq. 2: "The homogenous and inhomogeneous effects are included in the present RT model (by taking the average of these two terms) to simulate the underwater light fields in a wide variety of waters (including relatively clear, turbid and eutrophic waters)." This is not clear to me. I am not convinced that both parts weight equally: in clear water near the surface the direct component normally dominates; with inhomogeneous insolation conditions, only diffuse radiation occurs.

5. The wave tilt angle is not calculated based on Snell's law. Eq. 5 is the slope of the equation of a line.

6. I am not sure if Eq. 6 is valid for all cases, e.g. if the tilt is negative or smaller than theta. Please check this!

7. Regarding surface transmission: What is the exact shape of the used wave surface? What parameterization is assumed, Cox-Munk based wind-dependency? With high standing sun and low or moderate wind speeds (like the cases under consideration in Table 2), a flat surface gives more or less exactly the same irradiance transmission as a strong wind roughened surface. In the introduction it is stated that the model shall overcome key problems with random surface slopes. But what is actually the applied method and what are the effects and advantages? Later it seems to be just one single slope. "The new sea surface boundary condition and the estimated sea surface

transmittance have significant effects on the downwelling irradiance (Ed)” (page 2147). Is it possible to account for focusing effects? This major point needs major revision.

8. Provide a reference for Gordon’s parameter g! Is this parameter for case 1 waters?

9. Eq. 19: What do the numbers mean? Where do they come from? Is that applicable for optically complex waters?

10. Comment on the source function: As far as I know, chlorophyll fluorescence is not well understood. It depends on photo-inhibition, cell age, and many other things. This is a potential source of errors.

11. I am not sure about the effects of the homogeneous term (3.1.4). Maybe both terms should be compared, to get a feeling of the diffuseness.

12. Regarding the bio-optical model, isn’t it better to use Rrs data even if they are small? The random factors in Eq. 30 and 31 are probably depending on skylight diffuseness and may induce errors. Where do they come from? “The coefficients are derived based on the measured Rrs, [Chl]sur and [SS]sur data (Table 3).” This is not clear from Table 3!

13. CDOM profiles are not shown in Figure 3 – but it is said.

14. Concerning the use of a specific particulate absorption, what is with the sediment concentration? According Table 2 this makes half of the particulate concentration. I am sure that the specific phytoplankton absorption also works for high concentrations. The absorption should depend on the species and mineral material. How is the empirical relation Eq. 45 derived?

15. Page 2141, line 4: Rrs is no slope.

16. Page 2141, line 21: Figure 7c: it must be more light attenuation towards the sea bed.

17. Page 2148, line 2: “The bottom reflectance affects the entire water column” in

optically shallow water only!

18. Page 2148: “It was found that the predicted underwater light field parameters (Ed, Eu and Lu) from the present RT model using the Pred_IOPs were better consistent with the measured radiometric data as well as those obtained from the same RT model using the in-situ IOP data in clear waters, turbid waters, phytoplankton-dominated waters and eutrophic waters.” This is not clear to me, and I think Figures 11 and 13 show the opposite.

19. Table 5: Include units and be consistent: wavelengths in nm. Table 1 refers to micro-m.

20. Figure 1: Is suspended sediment a potential source? What is the slope of sea surface?

21. Figure 7: What is model and what is measured?

22. Figure 10 ff.: It would be good to include the specific water depths. And could you also include Eu/Ed or Lu/Ed in order to see how AOPs may change with water depth – which shouldn't theoretically.

23. In the end, there is nothing about the model performance in terms of simplicity and computational time. A statement on this could be helpful.

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Interactive Comment