

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2021-99

Jochen Wollschlaeger (Referee)

Referee comment on "Modelling the influence of light on the biological characteristics of coastal waters" by Paulo Felipe Lagos et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-99-RC1>, 2021

General comment:

The objective of the paper, thus the modelling of primary productivity and its relation to environmental parameters via their influence on the underwater light field is an interesting approach. However, I see some flaws in the methodology applied. The authors state the effect of CDOM and suspended particular material on the diffuse attenuation coefficient K_d in the introduction and discuss it to a certain extent at the end on the manuscript. However, the methodology applied is an approach that takes into account only biogenic light attenuating substances, represented by chl-a, thus an approach that is commonly used for case I water types. As the work was conducted in coastal waters, which are typically case II, this probably introduce errors in the analysis as substances like CDOM or suspended inorganic particles are ignored in the calculations, although they are known to play an important role.

Furthermore, there is a considerable lack of diligence in the manuscript, e.g. inconsistent use of variable names/terms or incorrectly displayed equations. Also, I had the impression that some bio-optical terms have been misunderstood. This makes the text difficult to read and to understand. Many different analyses have been made and the results are presented, but the reader is not well guided towards the main findings and the important connections. I would have also liked if an overall conclusion would have been presented in the end, but only the single result chapters are discussed as stand-alone parts.

I suggest that the authors improve the manuscript and hope that the following specific comments are helpful.

Specific comments:

Introduction:

Line 15: "Increment of the mixing layer" maybe more suitable?

Line 17-18: Influencing the underwater light environment in which respect? Increase? Decrease?

Line 23: Chlorophyll- a is not equivalent to primary productivity. Although it is also not really equivalent, I think "phytoplankton biomass" might be more suitable than "primary productivity" in this context.

Line 29-30: How do the changes in temperature and wind speed influence the amount of light penetrating the water? More intensive waves? Or is it rather that these conditions influence the depth of the mixing layer and thus the average amount of light the phytoplankton is receiving (which in turn influences its productivity)?

Line 34: Why do you state here explicitly the range 280-490 nm?

Line 36: Primary productivity is rather a process, not a parameter that attenuates light. "Phytoplankton" or "phytoplankton biomass" would be more appropriate.

Line 39-43: Why are you here explicitly discussing UV light decay in the water column? How is this relevant to primary productivity?

Line 50-58: The use of K_{bio} ($=K_d - K_w$) to predict chl-a values is, as far as I know, based on the assumption of a case I water type, where all optical properties co-vary with phytoplankton. The research presented here has been done in coastal waters, where, as you stated correctly, also dissolved and inorganic matter plays a role. Coastal waters are often case II water types, where the optical properties do not covary with phytoplankton. Thus, I would argue that the variability in K_d/K_{bio} is not only based in phytoplankton variability, especially as you state that your study area is influenced by river runoff (thus probably terrigenous CDOM), and your study site lies in the entrance of a harbor, where

resuspension of sediments might be an issue. Could you please explain why you used this approach anyways?

Line 62-64: I would expect that chl-a concentrations inferred from in situ measurements and satellite remote sensing would differ, and thus also the PP/B values of the model. Why do you expect otherwise and, if you expect the same results, why do you do the comparison?

Line 64-66: It becomes not clear why your hypothesis includes these exact values. It seems that you anticipate the results that you got to formulate your hypothesis.

Materials and Methods:

Generally, check the equations in the manuscript. Many of them are incorrect. Please show more diligence regarding this matter, as it questions the way the results of the study were calculated. The things I noticed so far:

In equation 2 and 3, the subscripts are not placed correctly (they should be on the depth and the irradiance, not the wavelengths). In line 99, the notation is correct. Further, the notation for logarithm is commonly not given in upper case letters.

Equation 4 and 5 both define Z_u

Equation 8 is wrong. K_{bio} is not equal to K_w .

According to Hernandez et al. (2012), equation 9 is not as you showed it here.

Line 75: Can you give an indication about the water depth in your study area?

Line 85: Maybe converting nautical miles in kilometers?

Line 86-95: Why was K_d only measured from surface down to 5 m depth? What about the rest of the water column? Furthermore, I would like to know how the instrument was deployed and if there were precautions taken regarding avoiding ship shadow etc. This might be relevant, as high quality hyperspectral light measurements are often difficult to obtain directly below the sea surface. Furthermore, you mention here that you measured from 300-700 nm, while in line 82, 280-700 nm were mentioned.

Line 110-111: Omit "radiation" in line 111, as UVR is the abbreviation for global radiation according to table 1. However, I noticed that this term is rather differently used in the manuscript. Please check this and use terms more consistent. Also, do you measure underwater global radiation? What exactly would be the spectral range of UVR?

Line 115: Give an abbreviation for "no cloud cover". I assume it is "NCD" as used later in the text (line 207)?

Line 130: K_{bio} at 490 nm as well?

Line 138-139: Is it DOM or CDOM that is calculated? And it would be nice to indicate the satellite transect in Fig. 1b to see the overlap with the station measurements.

Line 144: The average ± 1 x the standard deviation seems not very extreme to me. If you want to show the extreme values, why not just give min/max values?

Line 153: As I mentioned before, the approach you use is (also according to the references you cited) for the "clearest ocean waters", thus not to optically complex (coastal) waters. Why are you assuming that the difference between K_d and K_w is only phytoplankton-derived (K_{bio})?

Line 156: Please rephrase: If the functions were described initially by Mobley 1994, then they could not have been used from Morel 1988.

Line 159: In equations 6 and 7, no chl-a is included.

Line 160: K_{bio} is calculated, not approximated.

Line 168: The Smith & Baker reference is incomplete in the reference list (see also line 599). Furthermore, in Table B1 is stated that the used values are from Smith and Baker 1998. Please clarify/correct.

Line 190: This is the first time you refer to Table 2. Maybe it would be better to introduce it earlier as it contains also abbreviations and parameters that were used before. However, where in the table can I see that the C:Chla was limited to values >20? Furthermore, in the next line you mention that the ratio was fixed to values from 0.003 to 0.01. Where do these numbers come from?

Results:

Line 204: "Maximum above sea surface"

Line 207: Where is period of peak solar radiation shown in table 3?

Line 212: Why "in contrast"? You state before that also in summer the effect of clouds has an influence on irradiance reaching the sea surface.

Line 218-219: Where can I see this in Table A1? There is no increment of solar radiation during summer months shown? Or do you refer to the table only for the mild seasonal component part? Can you set the values you use in the text in

Line 221-222: You have already shown that cloud cover influences downwelling solar radiation

Line 226: You refer to "(3)". It is not clear if that means figure or table. Furthermore, please be consistent in the naming of the diffuse attenuation coefficient (K_d , not k_d). Also, you should explain why do you specifically analyze now $K_d(320)$? For what is this value an indicator?

Line 231: POC is not mentioned in table 3.

Line 230-239: Being not specifically an expert in PCA, I noticed that both temperature and SST (sea surface temperature) are included in the analysis. Furthermore chl-a (I assume calculated from K_d as described) and $K_d(490)$, which are closely interlinked. I was wondering if this redundancy is influencing the analysis as the parameters co-vary.

Line 236: "...a higher percentage of incident light..." instead of "...more light..."

Line 238-239: What is the difference between the analysis of K_d mentioned here and the analysis mentioned in line 234-235? Both are about the seasonal difference of K_d , right?

Line 241-243: The differences between the absorption coefficients of pure water proposed by Smith & Baker 1981 and Pope & Fry 1997 are rather small (differences predominantly in the blue part of the spectrum), but the spectral shape is more or less identical. The differences are enlarged in Pope & Fry by the logarithmic presentation of the data. Furthermore, as the Pope & Fry coefficients are more accurate, why not using them instead of those by Smith & Baker?

Line 243: "absorption coefficient"

Line 245: Differed from what?

Line 245-252: Here, I have several issues: When you subtract K_w from K_d , you obtain generally the attenuation coefficient of all optical active constituents in the water. In this context, you refer to it as K_{bio} , implying that only biogenic components are in the water. As I said before, this might be not true so close to the coast. However, of course, there is a positive significant correlation between K_d and K_{bio} , as one is calculated from the other. Therefore, I also do not understand the part about the two independent methods

Line 257-261: Here UVR means apparently only the UV-radiation. This is in contradiction to how UVR has been used in the text before and also in table 1 (UVR = global radiation). Please be more consistent. Furthermore, of course chl-a is related to K_d (by the way, the mentioned figures C1d and C1d show the relation between chl-a and K_{bio} , not K_d as mentioned in the text).

Line 266: Chl-a is not a function of a_w and b_w , but K_{bio}

Line 273-284: In this paragraph, you evaluate PP/B derived from satellites and say they are overestimated. In this context, the field derived PP/B estimates are the "truth"? Further, according to your findings, the waters are more productive in winter than in summer?

Conclusion & Discussion

Line 289: Why "while"? In both cases, you describe a reduction in UVR.

Line 295: Wind speed and temperature do not decrease solar radiation, but there is a correlation, as you showed. However, this is rather due to the different temperature and cloud patterns in the different seasons, right?

Line 300: What are the numbers? What is UVA / UVB in this context?

Line 308: What does "5" mean here?

Line 333: Primary productivity is a process, not a light absorbing component. This would be phytoplankton. Be more precise in your phrasing. Also nutrients themselves are not light absorbers (at least in the visible spectrum), but lead to increased phytoplankton biomass.

Line 337-339: As you have only measured K_d in the upper 5 m, the effect of the mixed layer depth on K_d is difficult to assess, I guess. However, the fact that seasonally more particles are introduced by river runoff makes sense. And this might be also an explanation for the higher PP/B in summer than in winter. The phytoplankton biomass B represented by chl-a is overestimated as K_d (from which chl-a is derived) is enhanced by particles from the rivers.

Line 359-363: The exponential decline of light in the water column that is driven by K_d is well known:

Line 378-379: K_d , thus the diffuse attenuation coefficient of water plus the constituents therein, cannot be calculated from the absorption and scattering characteristics of pure water.

Line 385-386: K_{bio} is not the sum of K_d and K_w , but its difference in case I waters

Figures and Tables:

Figure 2: What is the point of having chl-a from satellites and then calculate it again?

Table 3: What is the difference between "Average from field obtained measurements (F)" and "Average value derived from field measurements (D)"?