Ocean Science Discussions

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Reply on RC1

Paulo Felipe Lagos et al.

Author 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-AC1, 2022

Reviewer #1

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 Kd 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.

Reply to general comments reviewer #1:

We would like to thank the reviewer for the time spent reviewing this manuscript and for the large amount of constructive critique. We acknowledge many of the weaknesses pointed out by the reviewer, especially the case 1 water problem and the potential need to add CDOM/DOM data. However, as explained later in the specific question is hard to categorise the area of study as a case 1 or case 2 waters and addressing that particular issue was never in the scope of the manuscript. Therefore, we believe the study presents a valid approach for presenting important information that has never been published for

the region, keeping in mind some compromising concerning the amount of data given had to be done to construct a model and avoid an unnecessary lengthily and tedious research article. Moreover, we acknowledge the persistent issue with the presentation of the equations. But, we can say with confidence that these were just typos in the manuscript and do not reflect nor are present in the code. Therefore, they do not compromise the quality of the results presented here. Finally, we apologize for the syntax issues and inconsistencies. This study was written early in my PhD and vast improvements have been made since then. We believe that all the corrections pointed out by the reviewer can be easily incorporated into the manuscript.

Specific comments:

Introduction:

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

Answer: Term changed

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

Answer: This is further explained later in the introduction.

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.

Answer: term changed

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)?

Answer: Stronger winds generally increase the depth of the mixing layer. More specifically, for the Otago area and generally, for mid-latitudes, higher wind speed often correlates with periods of atmospheric instability, characterized by low atmospheric temperatures, high cloud cover and stronger wind speeds, which combined decrease the amount of light penetrating the water column.

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

Answer: It was intended to specify more biologically active radiation. Terms have been deleted.

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

Answer: Agree, the term has been corrected

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

Answer: This was mistakenly leftover from a previous version of the manuscript where we used short wavelengths in the UV spectrum to infer chlorophyll values in addition to the calculation performed. It has been deleted.

Line 50-58: The use of Kbio (=Kd-Kw) 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 Kd/Kbio is not only based on phytoplankton variability, especially as you state that your study area is influenced by river runoff (thus probably terrigenous CDOM), and your study site lies at the entrance of a harbour, where resuspension of sediments might be an issue. Could you please explain why you used this approach anyways?

Answer: That is correct, the equation is mostly used for case I type waters. However, as you also correctly stated, the use of this equation is based on the assumption that optical properties of the water co-vary with the biogenic constituents of the water. The waters in the area of study can't be really strictly classified as case I or case 2 type waters. Although, I grant that still, formal research about this in the area of study is yet to be found. I use the equation starting on the assumption that coastal waters in the area of study oscillate between having very low concentrations of phytoplankton and being close to case II waters to be close to type I waters. This assumption is based on personal observations and studies of phytoplankton abundance and other oceanography studies, which give the area nuances that make the area hard to classify into one type of water or the other. As I have to start from somewhere, I use the equation based on Morel et al. 2001 "Bio-optical properties of oceanic waters: a reappraisal", where he discusses reconstructing Chl values from Kbio is generally a better approach than reconstructing the values from pure attenuation coefficients for downward irradiance.

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?.

Answer: Mainly because we were not expecting satellite measurements of Kd and surface temperature to be much different to in situ measurements; after all they are supposed to be similar and close enough not to get a completely different estimation of the ratio. Briefly, the comparison, as mentioned in more detail in the answer to the question asked in regard to lines 245-252, is intended as a method to test the robustness of the results.

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

Answer: We did not specify a value for the ratio because we did not know the value the estimation would give us. However, we just hypothesise that it might be twice the unknown value of winter, by only keeping in mind that productivity increases during summer in most parts of the world.

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 Equations 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.

Equations 4 and 5 both define Z_u

Equation 8 is wrong. Kbio is not equal to Kw.

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

Answer: We double-checked the text and the code and we agree, many of the equations in the MS have typos; we apologize for the oversight and thank the reviewer for catching this mistake. However, after carefully checking the implementation of the equations in the code, we can say with confidence that the result of the model is correct and the equations in the code are implemented correctly. The equations in the manuscript will of course be corrected.

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

Answer: I added the maximum depth of the study area in the text, it is 15-25 meters.

Line 85: Maybe converting nautical miles in kilometers?

Answer: Agree. Units have been changed

Line 86-95: Why was Kd only measured from the 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.

Answer: Mainly because below 5 meters depth we measured no significant amount of light. And yes, we took several precautions to avoid shading the instrument. For instance, we had a ~1.5m extension arm to deploy the instrument and we launched the instrument always avoiding any shading of the sensors, deploying always during calm conditions as described in the methods and with the sun in its highest position. Maybe including the light profiles in the supplementary material would be advantageous to clear out any questions in regard to the quality of the measurements.

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?

Answer: Consistency of the terms UVR has been fixed. Underwater radiation was measured between 280-700nm. However, we did not integrate total global radiation from these measurements.

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

Answer: That is correct, NCD correspond to "no cloud cover" through the text.

Line 130: Kbio at 490 nm as well?

Answer: Yes that is correct, we only used the 490nm wavelength to calculate Kbio.

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.

Answer: There is a mistake in this part of the text, it should have been POC which

corresponds to the remote sensing measurements, as stated in the text. However, a transect in the map can be easily added to match the transects shown in the appendix figures c2 to c5

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

Answer. Agree. However, please consider that all the extreme and average values for all the measurements used in the study are present in Table 3

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 too optically complex (coastal) waters. Why are you assuming that the difference between Kd and Kw is only phytoplankton-derived (Kbio)?

Answer: Because it is the only assumption we can make given the formulation we are using. To the best of our knowledge, a formulation to derive chlorophyll concentration for case 2 waters do not exist.

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

Answer: The sentence has been rephrased.

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

Answer: Chl-a is not part of those equations; eq.6 and 7 are only used to calculate Kbio and Kw. Both terms are later used to solve for chlorophyll using eq.8

Line 160: Kbio is calculated, not approximated.

Answer: Correction done.

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.

Answer: Inconsistency in regard to this specific reference has been fixed throughout the MS

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?

Answer: Agree, the table can be introduced early in the text and the values of the parameters can be easily added to the table. The ratio was fixed following recommendations from other authors (Hernandez et al., 2012). We know the ratio can change with light conditions and temperature. However, in Cloern et al. (1995) it is discussed how, generally speaking for phytoplankton growing under laboratory conditions, the ratio usually oscillated between 0.003 and 0.1. Therefore, we fixed the ratio to these values to avoid complications due to the lack of real data on the ratio for the area of study. Finally, we agree this particular sentence needs fixing so it is more straightforward to understand that those were the values we fixed the ratio to.

Results:

Line 204: “Maximum above sea surface”

Answer: Yes, that is correct. Measurements were above the water.

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

Answer: it is not. A figure has been added to the appendix to show the period of max solar radiation.

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.

Answer: To highlight the difference between summer and winter. However, this has been re-phrased and now it should be easier to read and follow the comparison.

Line 218-219: Where can I see this in Table A1? There is no increment of solar radiation during the 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

Answer. A reference to table 3 has been added to follow what is in the text.

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

Answer: Agree, this line has been rephrased.

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 (Kd, not kd). Also, you should explain why do you specifically analyze now Kd(320)? For what is this value an indicator?

Answer: Should have said Table 3. In regard to Kd(320), we measured all wavelengths. However, we only use Kd(420) in the model and we limit the information given in regards to other important wavelengths such as Kd(320) to brief statements in the results that show one of the many checks we did to ensure the measurements of Kd(420) were correct, for instance by studying the correlation of Kd320 with Kd420. However, much of this detailed information was left behind to keep the MS as brief as possible.

Line 231: POC is not mentioned in table 3.

Answer: Has been added to the table.

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 Kd as described) and Kd(490), which are closely interlinked. I was wondering if this redundancy is influencing the analysis as the parameters co-vary.

Answer: We did run the PCA analysis several times with different combinations of datasets just to exactly see if the redundancy of some of the datasets influenced the results and to see which datasets had more weight. Ultimately as expected other datasets such as POC and the calculated clines had more weight, and taking out or adding the calculated chlorophyll or the Kd490, in the same manner, did not influence the results in a significant way, as these two were closely linked..

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

Answer: Term has been replaced

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

Answer: The difference is that lines 230 - 234 show the results of Kd490 which is the same wavelength used for the PCA and for calculating chlorophyll values. In contrast results in lines 238 -239 expand the results by analysing the Kd for all wavelengths from 300 to 700nm.

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?

Answer: Yes, we could have used the coefficients from Pope and Fry. But as you correctly mention the larger differences are the blue part of the spectrum. Therefore, giving we use Kd420 to do all the important calculations we did not give much consideration to this. However, this would be an easy change to make.

Line 243: "absorption coefficient"

Answer: Could not find this.

Line 245: Differed from what?

Answer: Differed from values below 600nm. This has been rephrased.

Line 245-252: Here, I have several issues: When you subtract Kw from Kd, you obtain generally the attenuation coefficient of all optical active constituents in the water. In this context, you refer to it as Kbio, 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 Kd and Kbio, as one is calculated from the other. Therefore, I also do not understand the part about the two independent methods.

Answer: The two independent methods (satellite vs field measurements) were originally thought of as a way of testing the robustness of the results (PP/B ratio) using data coming from different sources. Of course, there are uncertainties, the big one we acknowledge is using a Case 1 water approach to calculate chlorophyll values. However, given a case 2 waters approach to calculate chlorophyll does not exist, as far as we are aware, and given that we double check i) the values of chlorophyll against real measurements of Chlorophyll for the area and both are in the same ballpark and ii) the values of the PP/B ratio presented here against real-world measurements of PP/B ratios from other coastal areas, we believe this approach is valid (in the sense that "all models are wrong, but some are useful"). Ultimately, solving the Case 1 versus Case 2 dilemma was never the intention of this manuscript as we believe and acknowledge that this is a completely different problem. But at least we can say given all the "reality checks" we did that the approach seems like a reasonable approach. We are simply exploring a method that later when somebody comes out with a way of calculating chlorophyll from Kd values in case 2 waters can be further improved.

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 Kd (by the way, the mentioned figures C1d and C1d show the relation between chl-a and Kbio, not Kd as mentioned in the text).

Answer: Agree the text here needs improvements to match Table 1 which also needs some small modifications. We believe this section and table got mixed with a table and text from a previous version of the manuscript.

Line 266: Chl-a is not a function of aw and bw, but Kbio

Answer: Agree this has been corrected.

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?

Answer: We got some different results for the PP/B ratio on specific months depending if the PP/B ratio was derived from satellites or field measurements. Overall, we are attempting to explain that the first time we use the model with values using a calculated alpha parameter the model overestimates PP/B. But later using an alpha value specific for New Zeeland we got better PP/B estimations. The second part (Lines 277 - 284) should be better explained. The values there are average values for PP/B. The higher PP/B for winter based on satellite data was mainly pushed by the overestimated ratio obtained for January (summer) compared to the field-based model and using field data the model overestimate the ratio in August (winter). However, overall the PP/B ratio and productivity were larger in summer compared to winter months.

Conclusion & Discussion

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

Answer: The sentence has been rephrased

Line 295: Wind speed and temperature d 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?

Answer: Absolutely, wind and temperature do not decrease solar radiation per se. There is a correlation, but what directly influences local solar radiation is cloud cover and other meteorological factors such as ozone concentrations that are directly influenced by wind speeds and temperature patterns in the different seasons, just to mention some of the other more complex factors.

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

Answer: Correlation values between meridional wind component and ultraviolet radiation (both UVA and UVB).

Line 308: What does "5" mean here?

Answer: That is a correlation value of or around 0.05.

Line 333: Primary productivity is a process, not a light absorbing component. This would be phytoplankton. Be more precise in your phrasing. Also nutrients itself are no light

absorbers (at least in the visible spectrum), but lead to increased phytoplankton biomass.

Answer: sentence has been rephrased.

Line 337-339: As you have only measured Kd in the upper 5 m, the effect of the mixed layer depth on Kd 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 Kd (from which chl-a is derived) is enhanced by particles from the rivers.

Answer: Yes, this is an interesting point that is worth adding to the discussion. It would have been interesting to measure perhaps CDOM to try to understand to which extent the Kd we measure is driven by the run-off from the river and if this limits the amount of phytoplankton or not. However, we did have measurements of POC that give some clues about this.

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

Answer: This section has been summarized and shortened substantially.

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

Answer: Agree, Kd cannot be calculated directly from the absorption and scattering coefficients. The sentence was making reference to the fact that they are part of the calculation used to infer chlorophyll and Kbio. This needs further clarification to make the paragraph easier to follow.

Line 385-386: Kbio is not the sum of Kd and Kw, but its difference in case I waters

Answer: Yes, that is correct and the typo has been corrected.

Figures and Tables:

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

Answer: The point of the figure is to illustrate the two independent but similar methods we use to corroborate the results of the model. For model (a) we agree that it looks circular using chlorophyll to calculate chlorophyll again and that is because the figure is actually incorrect. The term Chl-a needs to be replaced by satellite values of Kd(420) to match what is shown in equations 6 to 8. Also, equation 8 has now been arranged and now it is displayed with chlorophyll on the left-hand side, not Kbio, in line with how eq 8 was actually used here.

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

Answer: Difference is that (F) is the direct mean value calculated from field observations and (D) is the average value of inferred quantities or not coming from a direct observation.