Review of the paper entitled: "*Modelling the influence of light on the biological characteristics of coastal waters*" by P.F. Lagos et al. for publication in Ocean Science.

General comments

This paper investigates the seasonal relationships between meteorological and oceanographic parameters over a three-year period and the effect light on the marine productivity (production/biomass ratio) in the Otago coastal area (New Zealand) through modelling approach. Although this study brings some interesting elements, it suffers from weaknesses both in terms of content and form.

Firstly, it seems to me that the authors completely obscure essential variables in their approach to determining the attenuation of solar radiation and its impact on marine productivity: 1) dissolved organic matter (DOM), and more particularly chromophoric DOM (CDOM), which is the main attenuator of solar UV radiation and which also very significantly absorbs visible radiation in aquatic ecosystems, furthermore in the coastal environment subject to riverine inputs. 2) The mixed layer depth (Zm), which strongly influences the occurrence of phytoplankton blooms and controls the impact of solar radiation.

Secondly, the study is not very extensive from a spatial and temporal point of view, which limits its scope and conclusions. The work was carried out for the Otago coastal waters but what is the implication for all the coastal areas (i.e., Case 2 waters)?

Then, the title of the paper is "*the influence of light on the biological characteristics".* However, the specific influence/contribution of light on/in the production-biomass ratio (PP/B), relative to the other parameters (POC, nitrogen, Chla...) is not at all assessed in this paper. There is no sensitivity study applied on light parameters (Kd, irradiances at specific depth…) to really determine their impacts on the PP/B ratios. More generally, there is a lack of cohesion and clarity between the objectives, the methodology, the results presented and the discussion which makes reading and understanding the manuscript quite difficult.

Also, as mentioned by the authors, their PP/B model is not validated by PP/B real measurements in the study area. In addition, it seems that there are no Chla concentrations measured on real water samples either to compare with the Chla data derived from bio-optical properties. This makes the results and conclusions much less impactful.

Finally, there are many syntax and editing errors (I cannot list all of them) and something wrong with the numbering of tables and figures.

For all these reasons, I recommend that this paper in the present form, which needs significant improvements, be rejected. Find below other comments/corrections.

Other comments/corrections

- Introduction, page 1, lines 18-20: "In the open ocean, solar radiation is the most important factor forcing atmospheric and ocean circulation…" What about wind?

- Introduction, page 1, lines 20-24: "*However, …..in coastal waters*". This sentence is not clear. Indeed, it is mentioned that changes in productivity depend on the complexity of coastal ecosystems which itself depends on factors including productivity.

- Introduction, page 2, line 34: Why the wavelength range 280-490 nm for Kd here?

- Everywhere in the manuscript: photosynthetically available radiation and ultraviolet radiation should be defined respectively as PAR and UVR the first time they are used with their associated wavelength range. By the way, "*PAR light*" and "*UVR light*" (or "*UV light*") should be replaced by "*PAR*" and "*UVR*". In addition, the interest of studying UVR should be better justified in the introduction.

- Everywhere in the manuscript: check the numbering of Tables and Figures. For instance, it seems that Table B2 is cited before Table B1, Figure 4 before Figure 3,…

- Everywhere in the manuscript: Sometimes the references are not cited in the chronological order (example line 38).

- Introduction, page 2, line 42: The reference *Gregg and Rousseaux, 2017* should be also cited here (Gregg WW, Rousseaux CS, 2017. Simulating PACE Global Ocean Radiances. Front. Mar. Sci. 4:60. doi: 10.3389/fmars.2017.00060).

- Introduction, page 2, line 42: Remove the reference "*Cao et al., 2014*" (cited twice).
- Material and Methods, page 3. Study site description (2.1): Figure 1 should be cited first in this section.

- Material and Methods, page 3, line 85: "*3.57 nm^2*". Something wrong with the unit I guess.

- Material and Methods, page 4, line 92: "*Table S1*". I cannot see Table S1.

- Material and Methods, page 4, equation 2 (Kd): I think there is a mistake, this is not λ1 and λ2 but Z1 and Z2. Moreover, in the denominator, this is not Z1 – Z2 but Z2 – Z1?

- Material and Methods, page 4. Kd calculation. It is not clear whether irradiance measurements have been made just above the sea surface (Es or Ed0+) 1) to correct in-water measurements in case of variation of surface irradiance due to cloud, and 2) to derive irradiance just below the sea surface (Ed0-). To determine Kd, sometimes it is much more relevant to use Ed0- derived from Ed0+ than the real measured Ed0- which can be affected by lens effects due to waves.

- Material and Methods, page 6, line 169: It is mentioned that Kw data from Smith and Baker (1981) were used. However, in Table B1 it is mentioned Smith and Baker (1998). In addition, why not using the Kw data from Morel et al. (2007)?

- Material and Methods, page 6, equation 8: "*Kbio = Kw = …*". I guess Kbio = Kd – Kw

- Material and Methods, page 6, line 172: "*In this study we assume that Kd(λ)…*" Here I think this is Kd(490) which is used to compute Chla concentration?

- Material and Methods, page 7, line 181: "*5mt depth*". Please correct.

- Results, page 7, line 204: "*Maximum surface levels of solar radiation between*…" Those data are issued the weather station measurements, right?

- Results, page 8, line 206: Replace "*peak of solar radiations*" by "*peak of irradiances*".

- Results, page 8, line 207: "*NCD*" is not defined I think.

- Results, page 8, line 216: Please provide wind speed in the same unit through the manuscript (kt, m s^{-1} or km h^{-1}).

- Results, page 8, line 224 and elsewhere: Salinity is given in PSU but should be given without any unit.

- Results, page 8, line 226: "Kd(320)". This wavelength of 320 nm (at the limit between UVB and UVA) is not justified.

- Results, page 9, line 240: Remove the part: "*spectral values of*".

- Results, page 9, lines 257-258: "*The parameters used for the calculations of Chl a, aW and bW, were those of light as a function of each wavelength in pure sea water and led to some overestimated values of Chl a*." This sentence is not clear.

- Results, page 10, line 283: "PP/B ratios from MODIS-Aqua and field obtained models had correlations values above≈ 45". What does this number (45) mean? A coefficient of correlation of 0.45?

- Conclusion and discussion, page 10, lines 287-288: "*The atmospheric components included in this study only represent a sub-set of environmental mechanisms that control incident UVR reaching the surface of the water*". Why only UVR here and not PAR? The focus on UVR is not enough explicit in the introduction/objectives.

- Conclusion and discussion, page 10, line 300: "*UVA*" and "*UVB*" should be defined before.

- Conclusion and discussion, page 11, line 308: "*the correlation between wind and solar radiation became less clear (5)*." What does this number (5) mean?

- Conclusion and discussion, page 11, line 331: "*We found an strong*" Please correct.

- Conclusion and discussion, page 12, lines 338-339: "*The Otago coast is influenced by a high discharge of run – off from the Clutha river, bringing more particular organic matter into the study region, potentially driven by an increase of rainfall".* Here, DOM/CDOM should be mentioned.

- Conclusion and discussion, page 12, lines 341-343: "*These differences in rainfall plus higher solar radiation levels driven by ozone movement in the higher atmospheric levels due to wind speed have the potential to increase the productivity at a local scale, thus* *maintaining higher levels of particulate matter in the first meters of the water column increasing Kd(λ) further*". This sentence is not clear at all. Please rewrite.

- Conclusion and discussion, page 12, line 352: "*We found spatial differences*".

- Conclusion and discussion, page 12, lines 355-356: "*We found that remote sensing attenuation coefficients Kd(490) correlated with in situ measurements of Kd(320)*". And not with in situ measurements of Kd(490)?

- Conclusion and discussion, page 12, lines 356-357: "*However, the process of obtaining valid measurements of in situ Kd(λ) was extensive and required comprehensive sampling to avoid erroneous measurements.*" I do not understand, why it is more difficult to obtain valid in situ Kd than modelled Kd?

- Conclusion and discussion, page 12, lines 364-356: "*Murdoch et al. (1990) it was expected to observe*". Please rewrite.

- Conclusion and discussion, page 13, lines 383: Replace "*lineal*" by "*linear*".

- Conclusion and discussion, page 13, lines 384-387: Actually it is not clear what it is compared in terms of Kd: Kd(490) versus Kd(Bio), measured versus modelled, Kd(Bio) from Chla versus Kd(Bio) from Kw.

- Conclusion and discussion, page 14, lines 420-421: "*Finally, the model implemented in this study was capable of interpreting seasonal changes in the PP/B ratio*". But it is not mentioned what is the importance of Kd in these seasonal changes.