**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "The value of adding optics to ecosystem models: a case study" *by* M. Fujii et al.

M. Fujii et al.

Received and published: 31 July 2007

First of all, the authors greatly appreciate the constructive review on our manuscript. We have revised our manuscript as attached, basically according to the reviewer's comments in such ways as described below.

Responses to specific comments

(1) Ecolight is given as the radiative transfer model, but the references cited on pg 1595 are for Hydrolight and so it would be helpful for the non-expert reader to have a statement about how they relate to each other.

−> Ecolight is a reduced version of the full-blown Hydrolight radiative transfer model. In Ecolight, the radiance is azimuthally averaged and is used to deduce undesirable computation load in running bio-optical models. The authors have added following

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

two sentences to describe how Hydrolight and Ecolight relate to each other to the last paragraph of Section 2.2: "The model-derived spectral absorption, a Fournier-Forand phase function with the model-derived particulate backscattering ratio (Fournier and Forand, 1994; Mobley et al., 2002), and sky and surface wave conditions, are all input into a radiative transfer model which calculates the underwater light field from which the downwelling photosynthetically available radiation (PAR) (W m-2) is obtained and used as an input to light-sensitive processes in the ecosystem model. For a radiative transfer model, we used Ecolight (Sequoia Scientific, Inc.), a new and reduced version of the Hydrolight radiative transfer model (see Mobley and Sundman (2005a, b) for details of Hydrolight). In Ecolight, the radiance is azimuthally averaged and is used to reduce the computation load. A semi-empirical sky model based on RADTRAN (Gregg and Carder, 1990), which is embedded in Ecolight, is used to calculate the hourly irradiance at the sea surface for the appropriate date and location, assuming no cloud cover and a surface wave field consistent with a daily-averaged wind speed of 5 m s-1."

(2) Figure 5: TT01 and TT012 are mentioned in the legend, but not explained: there is a reference for Figure 9. It would also be useful to have some detail on the in-situ data in the main body of the paper as there is a large spread for some of the parameters e.g. NO3 and SiOH4, but the text talks about standard measurements of 6 and 3 mmolN m-3 (pg 1595).

$-$> The authors have added a phrase in the text so that the sentence (page 1596, lines 8-14 in the previous manuscript) would be: "Given that the maximum specific grazing rate by mesozooplankton (G2max) has a relatively large uncertainty in its value, and the estimated value differs among previous studies with the same ecosystem model (Chai et al., 2002; Jiang et al., 2003; Fujii and Chai, 2007), we modify this parameter's value (tune it) so that the modeled surface nitrate and silicate concentrations would be the closest to the standard measurements in the equatorial Pacific of 6 (mmolN m-3) and 3 (mmolN m-3), respectively (Fig. 5 (b), (d)), which were derived from the US JGOFS EqPac observation in August-September (Survey II; TT011) and October

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

(Time series II; TT012) of 1992 (Murray et al., 1995; Barber et al., 1996; Chai et al., 2002)."

Also, the sentence in caption to Figure 5 has been modified to: "Dots denote the U.S. JGOFS EqPac observations in August-September (Survey II; TT011) and October (Time series II; TT012) of 1992 (Murray et al., 1995; Barber et al., 1996)."

(3) I thought that the order could be improved in Section 3. Section 3.1 should clearly state it is about the tuned optics model or it could become the first Section in 3.2 that would be renamed something along the lines of "Optical (Case 2) model". The introduction to the non-coupled optics models needs to be within Section 2 as it is methodology rather than results. Also, the figures are discussed in terms of the optics (Case 2) model up to Section 3.3.1 and then Section 3.3.2 brings in the Case 1-1 and 1-2 discussions. I found it difficult to split my understanding of the figures in this way (wait for further explanations in a latter section) and wanted to see the three models discussed together. I would therefore encourage the authors to consider this and if the sections are not rearrange there needs to be pointers in the earlier sections that let the reader know this discussion will follow.

−> Thanks to the reviewer's comment, the authors found that it would be much clearer for readers to read if the two sections of "bio-optical model results" and "model comparison with and without optics"; would be tied together in the same section. Therefore, the authors have rearranged Section 3 (Results and discussion)

from: (in the previous manuscript)

3 Results and discussion

3.1 Biological properties

3.2 Optical properties

3.2.1 Absorption

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

3.2.2 Backscattering

3.2.3 Beam attenuation

3.3 Optics as a constraint for determining variables and related parameters

3.3.1 Sensitivity to optical parameters

3.3.2 Comparison of model results: with and without optics

to: (in the revised manuscript)

3 Results and discussion

3.1 Bio-optical model results

3.1.1 Biological properties

3.1.2 Optical properties

Absorption

Backscattering

Beam attenuation

3.1.3 Comparison of model results: with and without optics

3.2 Optics as a constraint for determining variables and related parameters

(4) The three curves in Figure 6 all show a decrease at the surface that is not obvious from the in-situ points and is not mentioned in the explanation (other than through the mention of subsurface maxima) within Section 3.2.1.

$->$ The in-situ points are from Dupouy et al. (2003) and Simeon et al. (2003), and it is very difficult to reproduce vertical profiles of a_phi(440), aNAP(440) and aNAP/ap(440) from the surface to subsurface layer. This is because the measured values have wide range horizontally, both zonally (Simeon et al., 2003) and meridionally (Dupouy et al.,

2003). Therefore, further measurements would be necessary to improve simulation capability (and to obtain more accurate values of parameters introduced in the optics model). The authors have added following sentences to the end of the 2nd paragraph of Section 3.2.1 (Absorption): "The model results of a_phi(440), aNAP(440) and NAP contribution to total particle absorption all indicate linear increase with depth from the surface to subsurface layer. However, the in-situ measurements show wide horizontal variation in these components, both zonally (Simeon et al., 2003) and meridionally (Dupouy et al., 2003) in the equatorial Pacific, and further observational data of the optical components are necessary for improvement of simulation capability by the optical model." The authors have also added more data from Dupouy et al. (2003) and Simeon et al. (2003) to Figure 6.

(5) Pg 1586 Line 12: ".. Surface ocean color field[s] and subsurface light field[s] are .."
(6) Pg 1588 Line 8 ".. as [an] input to .."

$->$ The authors have revised following the reviewer's comments.

Interactive comment on Biogeosciences Discuss., 4, 1585, 2007.