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

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General comments:
Fujii and co-authors present an integrated physical-biological-optical model applied to the equatorial Pacific upwelling region. They demonstrate and discuss the potential benefit gained from adding inherent optical properties (i.e. absorption and backscatter) and a model of radiative transfer to their more conventional biomass-based model framework. Incorporating optics into biological and biogeochemical models is a hot issue (especially given the emergence of ocean observing systems and the now routine use of bio-optical and chemical sensors) and this paper is an important contribution to this area. I found the presented sensitivity analysis and case studies (model with and without optics) especially valuable, as they illustrate how exactly optical information can

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provide additional information beyond the commonly used types of data. I'm raising a few critical points and a list of suggested minor modifications below, all of which could easily be addressed in the revision process. Overall I feel this paper presents an important step toward the longer-term goal of assimilating optical data into biogeochemical models. I recommend accepting this paper with minor revisions.

Specific comments:
With regard to POC and NAP: In the parameterization of total scattering by particles, the ratio of phytoplankton carbon-to-POC is fixed at 30%, although this ratio is known to vary and although NAP concentrations are explicitly predicted by the model. Further justification of this choice is necessary, i.e. has this indeed been found an important aspect of the parameterization and if so why? Why not use the model-predicted NAP in some form? Or is this component of scattering almost negligible, with only very small impacts on the results? Also, it is not clear how POC1 and POC2, both used in the calculation of backscatter, have been calculated. POC is the sum of algal carbon and detrital carbon (both predicted by the model), but how is detrital carbon split between the two phytoplankton groups?

With regard to case studies and tuning: The authors tuned different variants of the model by hand (presumably), which is subjective and does not provide error statistics. Without information on error statistics it may be premature to say, for example, "with the spectrally-resolved bio-optical model we could tune the vertical phytoplankton assemblage more accurately" (line 26-28 on p. 1601). The use of formal data assimilation would be desirable, e.g. the variational adjoint method can provide information on the error structure through the Hessian matrix (see e.g. Fennel et al. 2001, J. Mar. Syst. 28:45-63). However, implementation of such a procedure is a significant task and the presented work is already an important step and valuable contribution that warrants speedy publication.

Technical corrections:
The paper is very concise and well written, except for a few sentences that would ben-

efit from minor editing:

Abstract, line 5-6: "it is necessary to model the light distribution accurately" is an inference. There are good arguments for it, but "necessary" is too strong a word.

Abstract, line 24-25: awkward sentence, consider revising

P. 1588, line 15: "Rothstein et al. (2006) have recently reviewed the state of the art of modeling harmful algal blooms" is not correct. The only mention of the word "harmful" in the whole paper is in one reference (Sosik, in press). Optics, on the other hand is mentioned may times (see e.g. their Fig 14).

P. 1594, line 22: Include the value for the time step used.

P. 1595, line 26: "biogeochemistry" may not be the best choice of word here. I would prefer "biogeochemical properties." Same for p. 1598, line 11 and p. 1599, line 25.

P. 1597, line 23: Include formula used to calculate c_p here.

P. 1598, line8-10: awkward sentence, revise

Caption to Figure 5: What do TT011 and TT012 stand for?

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