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Interactive comment on "Fast and accurate irradiance calculations for ecosystem models" *by* C. D. Mobley et al.

Anonymous Referee #1

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Fast and accurate irradiance calculations for ecosystem models

by C.D. Mobley et al.,

This paper is rather disappointing, particularly because its content does not reflect its title. It gives the impression that it is an inhomogeneous mixture of two writings and indeed it juxtaposes two topics, which could advantageously be dealt with separately. It is understandable (actually quite obvious) that when envisaging ecosystem modeling, the computational burden associated with the optical segment has to be reduced to an acceptable level. Having said that as a premise, the paper should concentrate on the way of preserving accuracy despite the drastic reduction imposed to the com-

putation time (needed by the Hydrolight code, taken as the reference starting point). The various optimizations of the initial code are well described (Section 2.3) . What is expected after this description, however, is (according to the title) a check of the resulting accuracy, which could be assessed, for instance, by comparing spectral (scalar) irradiance profiles, or PAR profiles, as obtained after successive simplifications of the initial (azimuth independent) code.

Instead we are presented (Section 4) with comparisons to an "analytical light model", that one which is embedded in EcoSim (and is not described, even succinctly). Must this analytical model be considered as a valid reference? Perhaps I missed something, but this point is unclear for me. In addition, the comparisons are not made in terms of optics and radiometric (irradiance) profiles, as a reader expects, but in terms of chlorophyll concentrations computed at selected levels for one-year simulation (or after 10 years). So, where is the baseline? Where is the optical "truth" to decide if the simplified simulations of the RTE remain acceptably accurate compared with the nominal one. The stability of the results (in terms of Chl) is certainly one possible criterion, but is insufficient to decide of the quality of the results. Why such time series of Chl would be "better" than another one?

The importance given to the description of the ecosystem model (ROMS then EcoSim), then to the ecosystem behavior is superfluous and does not seem appropriate in the frame of the present paper; it is another, self sufficient, topic in itself. Perhaps the solution could have been to prepare two companion papers; one on optics/RTE, with the rationale of developing a fast numerical model for application to ecosystem modeling, and providing a complete optical test of its efficiency (not presently the case); and a second one (?) devoted to improvement of the ecosystem simulation (better Chl ? e.g. Fig. 6 and 7; actually the reader is perplexed, and cannot be convinced. Even if the paper, piece by piece, is generally well written and clear (apart from the abuse of acronyms –AL, EL, HR… runA ..), its philosophy and logic are not well defined nor expressed. At this stage it is difficult to recommend the publication of this work which has to be rethought and recast accordingly.

Interactive comment on Biogeosciences Discuss., 6, 10625, 2009.