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Biogeosciences Discussions

Interactive comment on "An evaluation of ocean color model estimates of marine primary productivity in coastal and pelagic regions across the globe" *by* V. S. Saba et al.

Anonymous Referee #1

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Review of "An evaluation of ocean color model estimates of marine primary productivity in coastal and pelagic regions across the globe" by V.S. Saba and 22 co-authors

This paper is the latest in a body of work that presents results from the Primary Productivity Algorithm Round Robin (PPARR) effort. The Authors perform a skill assessment of 21 NPP models in 10 varied marine regions. At face value, such an analysis might not seems like a huge leap forward scientifically; however, the Authors present a nice series of validation analyses and introduce a timely analysis on model input uncertainties, both of which provide useful models for future skill assessments in other disciplines. In general, the paper is clearly written. Overall, I recommend this paper

for publication in Biogeosciences with a few minor revisions – general and specific comments provided below. For what it's worth, my general comments include several recommendations for additional text that will make the paper more useful for a wider audience.

General comments:

* Many readers will look to this paper to recommend a particular NPP model (or models) to most reliably help answer their particular science questions. One conclusion of the work is "Surprisingly, even though certain models performed significantly better than others in specific regions, the ocean color models generally performed equally well in terms of their average model skill across all ten regions" (Section 4.2). While still an important result, these readers will likely leave unsatisfied with this answer. In Section 4.1, the Authors discuss bulk model performance region-by-region and explore the features of each region that might contribute to or degrade model performance. In Fig 4, the Authors show model skill for each region. I recommend the Authors take these a step further and provide summary text or a graphic that elaborates (summarizes) observed model performances as a function of general, bio-geo-physical characteristics of a water mass (e.g., which models performed best in shallow waters with low temperatures, which models performed best in regions with high dynamic ranges of chl, and so forth). I acknowledge that, given the temporal and spatial variabilities of the in situ data, such a summary will be incomplete – however, it will still be highly useful for many readers.

* The Authors present a nice uncertainty analysis. I'd like to see them go a step further and make preliminary recommendations as to how this information could be used within an operational (forward-stream) environment for generating NPP time-series (e.g., the Oregon State's Ocean Productivity group's time-series).

* One central theme of this manuscript is the use of satellite-derived chl in the NPP models. In the context of assigning chl uncertainties, the paper would benefit from a

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brief discussion of the type (level) of satellite data typically used as an input field. Are 8-day, 9-km data, for example, the standard input, or is it equally as common to use daily, Level-2 (unbinned) data and/or monthlies? I'd also like to see the Authors elaborate on their Chl uncertainty calculations. It's uncommon to compare 8-day, 9-km satellite data with an instantaneous in situ value, particularly in ecologically complex or patchy regions with high temporal and spatial variability. Did the Authors consider (normalize?) their Chl uncertainty values by the standard deviation or number of samples in each 8-day, 9-km bin (both of which vary spatially and temporally depending on latitude, cloudiness, and local biogeochemistry)? Did the Authors consider using the standard Level-2 satellite-to-in situ Chl match-up statistics provided by the NASA Ocean Color group (perhaps assigning a default uncertainty to various magnitudes of chl)? There's also an inverse way of looking at this topic – how good to the satellite chl need to be to retrieve reasonable estimates of NPP? Given their uncertainty analyses, might the Authors comment on the requisite Chl quality levels for reliable calculate of NPP?

Specific comments:

* p. 6754, lines 24-29: Please state the in situ depth range(s) of the Chl measurements considered in this analysis. For clarity, please state the units of PAR (PAR from SeaWiFS, e.g., is Einsteins m-2 day-1). Please provide a reference for the shortwave radiation to PAR conversion factor of 0.43. Please provide a reference for the surface offset method (e.g., Levitus 1982?).

* p. 6757, line 10: Stow et al. 2009 is not listed in the References section.

* p. 6758, line 25: Table 4 is introduced in the text before Table 3.

* Sections 3.2.1 and 3.2.2: I believe that all references to Fig 3 should be to Fig 4 (p. 6750, lines 10-15) and that all references to Fig 4 should be to Fig 3 (p. 6760, line 25 and p. 6761, lines 3-15). If this is true, then Fig 3 and Fig 4 should be reordered (Fig 3 renamed to Fig 4 and vice versa).

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Interactive Comment

* p. 6761, line 15: Fig 6 is introduced in the text before Fig 5.

* p. 6763, lines 9-11: Please reword to clarify how these r2 statistics illustrate “that models have somewhat higher skill at moderate temperatures”. For clarity (particularly in support of the statement made in lines 13-15), consider adding the linear trend line to the SST panel in Fig 8.

* Section 3.3.3 and Fig 9: The (a) and (b) captions for Fig 9 should be switched (Fig 9a is currently on right side and Fig 9b is on left side).

* p. 6764, line 9: Change “can not” to “cannot”.

* Table 2: Change “Bahrenfeld” to “Behrenfeld”.

Interactive comment on Biogeosciences Discuss., 7, 6749, 2010.