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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.

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Reviewer #1:

Comment: 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

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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-geophysical 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.

Response: This is an excellent suggestion. We have added an additional figure (Figure 10) that shows individual model skill in the three depth ranges shown in Figure 9, three SST ranges (<10, 10-20, >20 C) at each of the three depth ranges, and three surface Chl-a ranges (<0.5, 0.5-1.0, >1.0 mg m-3) at each of the depth ranges. The figure is a quilt diagram with RMSD in the color bar. We also show each column's N because the sample sizes for each column in the quilt will be different and can thus bias RMSD. For example, the majority of the stations at depths between 250 and 750 m are at SST less than 10 oC. Comment: 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).

Response: We have added text to the end of section 4.3 suggesting that ocean color NPP estimates that are either published or made available online for public access should note the magnitude of uncertainty in the PP estimates due to uncertainty in input variables such as SeaWiFS Chl-a and MLD.

Comment: 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 brief discussion of the type (level) of satellite data typically used as an input

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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? Response: Comparing in situ Chl-a to 8-day Level 3 SeaWiFS data was not ideal but it was a compromise solution to get a maximum uncertainty estimate for each region. However, ocean color models typically use Level-3, monthly or 8-day, satellite-derived Chl-a and thus we were able to get an idea of RMSD sensitivity to in situ versus satellite-derived NPP estimates. Using daily Chl-a was not possible because there are many dates in the various time-series when the sensor did not pass over the sites on the particular days that the NPP measurements were taken. In section 4.3, we noted that the reduction in RMSD from uncertainties in Chl-a are likely closer to what Friedrichs et al. 2009 report because that paper based the error in Chl-a on the in situ measurement. However, our goal here was to demonstrate the level of uncertainty when considering differences between in situ and the most commonly used satellite-derived Chl-a product.

Comment: 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

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the surface offset method (e.g., Levitus 1982?). Response: We have added text stating the depth range of in situ surface Chl-a measurements (0-5 m). We added the units of PAR and listed a reference for the shortwave radiation conversion factor (Olofsson et al., 2007). We added the citation for Levitus, S., 1982: Climatological Atlas of the World Ocean. NOAA Prof. Paper No. 13, 173 pp to cite the MLD surface offset method.

Comment: p. 6757, line 10: Stow et al. 2009 is not listed in the References section. Response: We have added the reference for Stow et al., 2009 to the references section.

Comment: p. 6758, line 25: Table 4 is introduced in the text before Table 3. Response: Table 4 is now Table 3 and vice-versa. Comment: 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). Response: This was an error in our submission. The Figure order is corrected in the revised paper.

Comment: p. 6761, line 15: Fig 6 is introduced in the text before Fig 5. Response: We removed the citation to Figure 6.

Comment: 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. Response: We have removed text regarding model skill at moderate SST. We have added the linear trend to SST in Figure 8 so that both regressions are shown.

Comment: 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). Response: The figure placement has been switched.

Comment: p. 6764, line 9: Change "can not" to "cannot". Response: Fixed.

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Comment: Table 2: Change “Bahrenfeld” to “Behrenfeld”. Response: Fixed.

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