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Biogeosciences Discussions

Interactive comment on "Satellite views of global phytoplankton community distributions using an empirical algorithm and a numerical model" *by* C. S. Rousseaux et al.

C. S. Rousseaux et al.

cecile.s.rousseaux@nasa.gov

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The reviewers comments are in bold, our reply are in italics

I found the paper totally inappropriate for publication. The paper just reports the mismatch between the model and the observations, plots of mismatch in the seasonal cycle and a statistical table. Too basic and no insight in the problems. The authors just say where the model matches the observations and where they do not but they do not provide any explanation in terms of physical and biogeochemical mechanisms or something that relates to ecological competition.

In the recent years the research in this area has moved from total chlorophyll to

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Interactive Discussion

Discussion Paper

looking specifically at phytoplankton composition. Because of the increasing number of products that rely on either models or satellites to estimate the phytoplankton composition, we believe that this paper reports some important results on the strength and weaknesses of the two approaches used to estimate the phytoplankton composition. The model and satellite-derived approach will most likely be used in a few years by a wider scientific community and therefore this community should know where there may be some limitations and make the appropriate choice of approach for their specific scientific question.

We do provide several insights and explanations in terms of physical and biogeochemical mechanisms. Here are a few examples:
-"... in the North and Equatorial Indian, both approaches indicate that most phytoplankton groups reach a maximum in August. This summer maximum in the Equatorial Indian is most likely related to the monsoon cycle as observed in the subtropical waters of this region. In the Equatorial Indian, most of the increase in chlorophyll in summer occurs on the western side of the basin. In this region, satellites can be contaminated by the atmosphere (Gregg, 2002). For example, dust plumes accompanying the high winds of the southwest monsoon are known to influence the chlorophyll concentration in the Indian Ocean (Wang et al. 2005)."
-" Both approaches indicate that the previously, well accepted and reported spring bloom in the North Atlantic and Pacific and austral summer bloom in the Antarctic is made of a relatively high proportion of diatoms. The mechanism driving the intense spring-summer bloom in the temperate and subpolar latitudes is well known (Sverdrup, 1953). The deepening of the mixed layer depth in winter allows for surface waters to be replenished with nutrients which in turns allows the phytoplankton to flourish in spring-summer. During these events, the dominance of diatoms in these regions has been previously reported. For example, Marañon et al. (2000) found that diatoms make up to 80% of the total phytoplankton carbon in the North Atlantic in May and was reduced in September-October which supports both the model and satellite-derived

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data. "
-"The vast blooms of coccolithophores observed in the North Atlantic for example (e.g. Okada and McIntyre, 1979;Robertson et al., 1994;Boyd et al., 1997;Balestra et al., 2004) are well represented by both the model and the satellite-derived approach. Using calcite as a representation of coccolithophore abundance, Gregg Casey (2007) qualitatively compare the model-derived coccolithophores distribution to that from Balch et al. (2005). In both datasets, coccolithophores were abundant in the Southern Ocean transition region, around 40° S in the Atlantic and Indian basins. Further south in the extremes of the Southern Ocean however, coccolithophores are abundant using the satellite-derived approach whereas they are absent in the model."

There could be some potential problems in the physics of the GCM they use, for instance, in the seasonal cycle of the mixed layer that ultimately regulates both light limitation and the nutrient supply. But they do not mention anything about this aspect so impossible to know to me....
Yes there are likely to be problems in the way the model represents mixing. There are likely many other problems, some of which we are aware of and some we are not. It is a model, after all. The same can be said about the satellite algorithm and even the in situ data we used. But this is the point of the paper: how well do they compare with each other and with a (flawed) set of in situ data? Where, when, how, and why do they diverge? We believe we approached this problem in a rigorous, scientific, and objective manner, and that these results are of interest to the scientific community. We note that The NASA Ocean Biogeochemical Model has been validated extensively (e.g. Gregg and Casey 2007, Gregg 2008, Nerger and Gregg 2007), as has the satellite algorithm. But validation does not mean they are perfect. Just that their capability and some of their flaws, have been quantitatively assessed.

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We are assuming that the reviewer copied twice the same comment by mistake, see above for a reply to this comment.

Interactive comment on Biogeosciences Discuss., 10, 1083, 2013.