**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.

Anonymous Referee #1

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After a careful reading of the paper entitled ÂńÂǎSatellite views of global phytoplankton community distributions using an empirical algorithm and a numerical modelÂż, I inform that I recommend major corrections before publication in "Biogeosciences Discussions". You will find below my analysis of this manuscript.

Firs, the title of the manuscript is not in agreement with the text and objectives of the work presented. Indeed, the paper is rather a validation, then a comparison, of two different approaches (published before) to estimate the distribution of major phytoplankton groups : from space and based on a model output. The authors should add a reference to this comparison / errors estimation in the title. The more interesting part

is about the seasonality analysis, and this should also be clear in the title.

The first part present a quick validation of the satellite and model approaches based on in-situ observations. The second part present a comparison (distribution and seasonality) between the model and the satellite approaches, without a clear link with the first part.

The aim of this paper could be potentially interesting if clear information about which method should be preferred in specific regions or for specific groups are added. It's not the case in this first version, sometimes rather confusing. I suggest that the authors change the organisation of their paper, by adding more links with the in-situ validation part (only in the 2.1 part for the moment) when they discuss about the comparison between satellite and model observations/output, ie in each of the 3.2.X part and in the discussion part.

Please find below some more precise remarks :

-It's not clear why the authors chose to use monthly climatology over 10 years to estimate the errors of their methods compared to in-situ dataset. If there is no clear reason to do that, the authors have to use matchup as the Seawifs documentation (from NASA) defined it : daily pixels observation (from the 9*9km box closer from the in situ measurements, at least) and, ideally within +-3 hours (but at least daily data for satellite part will be better..). Climatology use is not acceptable unless the authors have a good reason for this. The authors should also add the years 2008-2010 for SeaWiFS data ?

- line p1084-26, the carbon fixation should be presented as an example and some references added. - line p1085-0 to 24, please separate the satellite and the model part, it's rather confusing like that (for example, it's difficult to understand (line 16) if the approaches are those from satellite or from modeling. - line p1086- 0 to 25 : clear lack of references in this part...! ie for page 1087. Please check your references for each previous results or an hypothesis.

- The authors should address and discuss the question of satellite observation bias due to underestimation or overestimation of chlorophyll-a concentration in some regions (see Arrigo et al. 1998 or Dierssen et al 2000, for the Antarctic for example). This could also be an explanation of some errors they noticed.

-page 1088, line 5 : is it the NOMAD dataset ? If not, the author should use this dataset also. This part is really too short :

-The authors have to explain the method they used to define which phytoplankton is present in the water based on their in-situ observations.

- The impact of the use of a climatology for the seasonal study should be at least precisely discussed.

- A table for validation results could really help the reader.

- Parts 3.2.1, 3.2.2... please add some reference to the "in-situ based" errors estimation part to make these parts useful. Which method is the best... ? Presented like this, this paper can only be used by people in charge of the development of the satellite and model approaches. With more information it could be useful for the community also.

- p1090 : could you precise the method used to compute the p value ?

- The seasonality part analysis is better even if it looks like a succession of results.

- The discussion should address separately the potential bias due to model or satellite method, then specficity of regions. Once again, a discussion based on a careful validation exercise is needed, unless the authors focused their article only on a comparison between a specific satellite approach and a specific model, but this will be really less interesting for the community.

- P1095 : this part is not clear and rather vague. It's difficult to extract a clear information from the discussion part as it is written in this first version. - The 'phaeocystis part' (in the conclusion part) is a hypothesis and should be presented like that, at this stage,

it's not clear that it will improve this inter-comparison exercise. - The two last sentences are off topic if we consider previous results addressed in this paper.

- Table and figures are clear.

With many thanks in advance to take into consideration this review,

Sincerely yours