

Interactive comment on “Regional scale characteristics of the seasonal cycle of chlorophyll in the Southern Ocean” by S. J. Thomalla et al.

Anonymous Referee #1

Received and published: 13 June 2011

General comments

Throughout the review, I use (pX, Y) to refer to page X and line Y of the print version of the discussion paper.

This manuscript describes a spatial analysis of the seasonal cycle of satellite-derived chlorophyll (Chl) in the Southern Ocean and surrounding waters. The information is used to identify and classify distinct zonal regions with differing trends in Chl biomass and seasonal dynamics, which the authors describe in relation to hypothesized physical mechanisms which influence phytoplankton production.

Although this is a descriptive study that is based exclusively on satellite data in a region
C1519

where the applied ocean color algorithms have well-described limitations, the authors offer a fairly detailed analysis of existing satellite data time series and provide plausible explanations for the observed trends which are generally consistent with the literature. In my view, the significance of such observations and hypotheses is that they can provide guidance for future field studies in this important region.

The authors elaborate extensively on some physical mechanisms for influencing phytoplankton biomass through regulation of macro- and micronutrient availability and irradiance levels within the mixed layer, yet say little about other potentially important mechanisms. With regards to iron availability, the role of aeolian dust deposition into surface waters is completely ignored in the discussion of Fe-limitation (e.g. p4777, 25). Although still controversial, some studies have postulated that atmospheric dust input of iron is a primary controller of production over large areas of the S. Ocean (e.g. N. Cassar et al., *Science* 317, 1067 (2007)). A huge body of literature exists on the role of grazing in the S. Ocean. What is the potential role of changes in phytoplankton community composition (biogeography) in determining the observed patterns of seasonality? Although the authors may not have data to explicitly address such questions, I do feel it important to include them as alternative hypotheses for consideration in the "Synthesis" section of the manuscript.

Specific comments

Different definitions of the "Southern Ocean" throughout the manuscript, for example south of 30°S (p4767, 20; p4771, 5) or south of 40°S (p4775, 28). Recognizing that the northern boundary of the Southern Ocean has never been formalized because of political reasons, the authors should just adopt one definition and stick to it. I suggest 35°S as the northern boundary, as it is close to the mean position of the N. Subtropical Front.

(p4767) As the SeaWiFS Chl estimates are the main data used in this paper, some more details regarding them should be given. What NASA reprocessing version is

used, and what Chl algorithm was used? When computing annual means and seasonal cycles, how did you deal with the lack of valid satellite data during the winter months? Ice cover, clouds, and low sun angle usually result in no valid data south of 55°S.

(p4769, 4) What is the justification for a choosing a std. dev. = 1 (= 8 days)? Was it chosen randomly, based on some underlying statistical tests of the data, or other criteria?

(p4770, 17) The definition of an Einstein as a unit of energy is incorrect. An Einstein simply refers to a mole of photons, irrespective of whether the photons are mono- or polychromatic. It cannot be directly related to energy, except in the special cases of monochromatic light or when the spectral distribution is known.

(p4776, 6) The std. dev. = 10–16 what? What are the units of the std. dev., I assume days and not 8-day periods? Are the higher standard deviations simply reflecting larger uncertainties in Chl which arise from low Chl concentrations? Why not use the coefficient of variation (std. dev. normalized to the mean or median) to characterize seasonality? After all, you define “bloom” in terms of a normalized quantity (5% increase over a median value).

(p4778, 25) and (p4786,18) I think you need to more strictly define what you mean by “seasonality”. In Fig 5d, the seasonal cycle appears to me to be well-developed yet you claim that this region and the MIZ is not seasonal. Although the exact timing is perhaps not well reproduced annually (high std. dev.) for these regions, the general seasonal pattern (at the level of a fall vs. summer bloom) is.

(p4781,12) I would also suggest that phytoplankton community composition could play a role here, not just physiological acclimation.

(p4783) The discussion regarding Fig. 8 is quite confusing. It reads as if absolute values of Chl are being depicted (e.g. lines 6-10), yet it is Chl “anomalies” that are plotted in the figure. It is difficult to interpret this figure, as there is no description

C1521

of exactly how these anomalies were calculated. Is it $\log(Chl - Chl_{mean}/Chl_{mean})$, or $(\log Chl - \log Chl_{mean})/\log Chl_{mean}$? Why use a mean value instead of the median value which was used for defining blooms? What are the units of the color scale on the figure; does a value of 1.5 indicate a change in Chl of 1.5%, or $10^{1.5}\%$, or ?

(p4785) Please be more specific in the criteria you used to develop the zonal classification, for example what is considered “low” and “high” Chl.

(Synthesis section) The broad conclusions of this study seem to be generally consistent with a recent similar analysis of satellite-derived POC concentrations in the S. Ocean by Allison et al. (JGR 2010, doi: 10.1029/2009JC005347), who also note a weak seasonal signal in surface POC for waters 35–45°S and higher seasonality associated with higher latitudes. I think it important to include a few lines of comment comparing your results with theirs in this section.

Technical corrections

There are a large number of acronyms used throughout the paper to denote geographic regions, data products, and sensors. Even when defined upon first use, it's annoying to have to go back searching in the text for the definition when encountering an abbreviation several pages later. I would strongly recommend providing a table listing the most-commonly used acronyms, to which a reader could quickly refer to when reading the text.

In general I find much of the paper over-referenced. An idea or concept should require at most 3 references; a seminal reference to acknowledge first credit, a recent review, and perhaps a later reference which has new important information. If a good review paper is available, that is generally sufficient. Please be more selective in the references you choose.

Please further increase the font sizes used in your figures. Although they may appear to be fine on your monitor at 300X actual size, I challenge you to print a hardcopy and

C1522

read them without some sort of magnification.

(p4755, 26) "effect" should be "affect"

(p4769, 20) It seems that you are referring to a figure from a previous version of the manuscript and is no longer present.

(p4777, 4) and numerous other places throughout the text. R^2 values are missing decimal points (i.e. $R^2 = 91$ should be $R^2 = 0.91$).

(p4781,12) Superfluous "of"

(p4790, 5) Behrenfeld is misspelled.

Fig. 3: The units of std. dev. used in the color bar need to be specified. Does the scale represent days, or weeks? I assume these are absolute values of the std. dev., since there are no negative values.

END OF REVIEW

Interactive comment on Biogeosciences Discuss., 8, 4763, 2011.