Response to referee reports

We would like to thank the referees for their time and useful comments towards the improvement of our manuscript.

Response to Anonymous Referee # 1 comments

General comments

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. 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.

I would like to bring to attention the following text that already appears in the paper which addresses the roles of various controlling mechanisms of productivity in the Southern Ocean.

In the Introduction:
"Numerous studies in the literature have addressed the factors governing phytoplankton distribution, diversity, biomass and production. These include both bottom-up controls of the physiological response of phytoplankton assemblages to physical and biogeochemical forcing (e.g. Martin et al., 1990; Cullen, 1991; Nelson and Smith, 1991; de Baar et al., 1995; Boyd, 2002) as well as top-down controls of grazing (e.g. Cullen 1991; Price et al., 1994; Smetacek et al., 2004; Behrenfeld, 2010). Moreover, given the known influence of temperature on phytoplankton growth (e.g Raven and Geider, 1988) and photosynthesis (e.g., review by Davidson, 1991) it is not surprising that the cold temperatures of the Southern Ocean also effect phytoplankton biomass and the seasonal cycle. The aim of this paper is not to investigate further the numerous controls of production in the Southern Ocean but rather to use remote sensing data at appropriate temporal and spatial scales to characterise regional differences in the Southern Oceans seasonal cycle."

In the discussion:
"The annual cycles of phytoplankton biomass in our results emphasize the role of bottom-up controls (light and nutrients) on increases in phytoplankton specific growth rates for determining bloom initiation (Sverdrup, 1953). The lack of information on growth rates does not allow us to quantify the role of grazing. In this study, we do not assume that the role of grazing is negligible, but rather that statistically significant seasonal increases in biomass can only occur when specific growth rates exceed loss terms and net population growth remains positive, despite potential increases in grazing pressure associated with increased encounter rates when the seasonal mixed layer shallows."

In order to include the potential role for other mechanisms (e.g. grazing and phytoplankton community composition) on the seasonal distribution of phytoplankton, the following paragraph has been added to the Synthesis section:

"This conceptual framework for characterising the response of the biological seasonal cycle to the underlying physics emphasizes the role of bottom-up controls of light and nutrients in the Southern Ocean. The lack of information on growth rates and community composition does not allow us to quantify the roles of grazing and biogeography in determining the observed patterns of seasonal characteristics. Knowledge of the changes in biological factors (e.g. grazing and community structure) is however important if we are to understand the biological response to future climate change. For example, changes in climate may facilitate a shift in the species composition in a manner that can alter the elemental composition of particulate matter, cell size and the trajectory of primary production through the food web, influencing the proportion of the biomass exported to the deep sea (Finkel et al., 2010). Looking to the future, the development of ecosystem appropriate functional type algorithms for the Southern Ocean will allow us to use satellite remote sensing data to provide information on the response of phytoplankton community composition and physiology to physically distinct seasonal regimes and ultimately to climate change. "

Although we do not know the role of biogeography in determining the observed patterns of seasonality, we do have some information on the potential role of different seasonal regimes on phytoplankton diversity. The following text from Barton et al., (2010) was added to the manuscript:

To section 3.2 on the seasonal cycle of chlorophyll in the Southern Ocean:
"Barton et al., (2010) developed a large-scale ocean model to investigate phytoplankton diversity across the global ocean and found that temporal variability of the environment played a significant role on the ecological control of phytoplankton diversity. Regions with relatively steady environmental conditions and high seasonal cycle reproducibility (e.g. the subtropics) enable the coexistence of multiple phytoplankton species and enhanced diversity (Barton et al., 2010)."

"In regions such as these, where there is a low degree of seasonal reproducibility, environmental variability (through changes in the MLD which regulates light and nutrient availability) may lead to competitive exclusion and a reduction in phytoplankton diversity (Barton et al., 2010)."

To the synthesis:
"According to ecological explanations for modelled diversity gradients, such stable environments are said to favour multiple phytoplankton with comparable fitness and subsequent high diversity (Barton et al., 2010)."
"In regions such as these, where the seasonal variability of the environment is high, competitive exclusion of phytoplankton with slower growth rates may lead to lower phytoplankton diversity (Barton et al., 2010)."

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)).*

In order to include the role of aeolian dust as a dominant source of Fe in the Southern Ocean the following text has been added to:
Section 3.1 on the discussion of the zonal characterization of seasonal biomass variability.
"Atmospheric dust deposition downwind of dry continental areas (e.g. Patagonia, south and southwest of Australia, New Zealand and Africa) is also considered a dominant Fe source fertilising primary production in the Southern Ocean (e.g. Cassar et al., 2007)."

Section 3.2 on the discussion of the amplitude of the seasonal bloom depending on Fe supply from a list of various different mechanisms.
"and e) the delivery of soluble iron by aerosol deposition"

Section 4 in the synthesis and the discussion of high chlorophyll regions via Fe addition to surface waters
"and downwind of dry continental areas (Cassar et al., 2007)."
"or dust deposition"

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.

The definition of the Southern Ocean in this paper has remained as south of 30ºS and represents the region for which data is being presented for this study. However, the confusion generated by often referring in the text to the Southern Ocean as the region south of 40ºS has been removed. In these instances we have replaced 'Southern Ocean' with the region 'south of 40ºS'.

(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.

The following text has been added to the methods section to expand on the origin of the SeaWiFs data set and how we dealt with the lack of valid data points in the winter months:
"The SeaWiFS Chl-a estimates for Case-1 waters comes from the OC4V4 processing algorithm from the NASA, Level 3 product (binned and mapped)."

“When computing averages, a criteria of at least ¼ of the maximum possible number of observations available was used. Grid-points where this criteria is not satisfied (i.e. more than ¾ of the data is missing) are discarded and appear in grey in the figures.”
“See supplementary material for available data used to calculate averages in July (Figure S5a) and January (Figure S5b).”

Following comments from the second reviewer, additional material has been added to the supplementary material in the form of maps presenting the number of observations available (i.e. number of 8-day composite periods for which data is valid) for the July, January and September (1998-2007: 46 “weeks” x 10 years).

A discussion of the implications of the presence of missing values on our results is also offered in the response to reviewer #2

(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?

The Std. Deviation here is the scale parameter sigma for a Gaussian function with which the time-series is convoluted. Here then it is dimensionless. It has been chosen as a good compromise between removing intra-seasonal (or intra-annual) variability without over-smoothing the seasonal cycle.

(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.

The incorrect definition of an Einstein as the energy equivalent to 1 mole of photons of monochromatic light has been removed from the manuscript.

(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).

Figure 3 discussed in this section (p. 4776, line 6 and further) presents the standard-deviation of the bloom initiation date over the 9 years (1998/99 – 2009/10), not of the chlorophyll concentrations. As such, it cannot be influenced by low chlorophyll concentrations.

As a date is not a quantity (here it is rather an index, going from 1 (first week of the year, starting 1st of Januray, ending 8th of January included), to 46 (last week of the year: 27 December to 31 December), there is no need for a normalization. A coefficient of variation is useful for comparing variability between distributions characterized by a large range in their location parameters (mean for example).

However, we discovered a mistake in the calculations that led to an overestimation of the standard-deviation. As we are dealing with dates, a bloom initiation determined as being in December is not far from one in January and the difference is small, because we neglected this property, the standard-deviation was greatly overestimated in the previous calculations. The new figure 3 now display the correct values. Note However that the spatial pattern is the same.

(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.

In order to prevent confusion between our definition of the term high and low seasonality and that of other readers, we have adjusted the term from seasonality to seasonal cycle reproducibility. The definition for high and low seasonal cycle reproducibility now reads as follows:

"The percentage of the variance explained by the mean seasonal cycle (Figure 4) defines how well the mean climatological seasonal cycle (from 9 years) represents the evolution of chlorophyll over each year. Areas where the seasonal cycle for each year is coherent with the 9 year mean ($R^2 > 0.4$) are defined as having high seasonal reproducibility. Regions where there is large variability from year to year in the timing and amplitude of the bloom and only a low percentage of the variance is explained by the mean seasonal cycle ($R^2 < 0.4$) are defined as having low seasonal reproducibility."

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

The role of community composition has been mentioned and the sentence now reads as follows:

"High chlorophyll concentrations coinciding with deep MLD's in the subtropics are likely enhanced by adjustments in community composition and photoadaptation of light limited cells in the deep winter mixed layer (Letelier et al., 1993)."

(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 of exactly how these anomalies were calculated. Is it log(Chl − Chlmean/Chlmean), or (logChl − logChlmean/logChlmean)?
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 101.5%?

Figure 8 has been changed and now presents the raw chlorophyll values in mg m^{-3}.

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

Figure 9 has been redone, this time using defined thresholds for high and low chlorophyll and seasonal reproducibility. The text has been updated to read as follows:
"The four regions result from a combination of high (> 0.25 mg m^{-3}) or low (< 0.25 mg m^{-3}) chlorophyll concentration and high (R^2 >0.4) or low (R^2 <0.4) seasonal reproducibility."

(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.

The paper by Allison et al (2010) use satellite ocean colour data to demonstrate variability in POC on seasonal and inter-annual time scales. In their paper, they define seasonality as the difference between summer and winter POC maxima and minima (what we would have termed the amplitude of the seasonal signal). The broad conclusions of their study of weak seasonality in the subtropical zone and high seasonality in the MIZ are at odds with ours due to a difference in the interpretation of the term seasonality. In our paper, we defined seasonality as the degree of reproducibility of the seasonal cycle. In our study, the subtropical zone exhibits a degree of seasonal reproducibility (i.e. low inter-annual and intra-seasonal variability) despite a small difference in amplitude between summer minima and winter maxima. In the MIZ on the other hand there is a large difference in amplitude between summer maximum and winter minimum (which according to Allison et al., 2010 amounts to high seasonality) however, according to our definition of seasonality, large intra-seasonal and inter-annual variability chlorophyll distribution in the MIZ results in a low degree of seasonality.

In order to prevent confusion between our definition of the term high and low seasonality and that of other readers, we have adjusted the term from seasonality to seasonal cycle reproducibility.

The study by Allison et al., 2010 is however still considered relevant to this manuscript and their work has been added as a reference to our manuscript where appropriate:

Section 3.2. describing the seasonal cycle of chlorophyll in the Southern Ocean:

"Despite the amplitude of the seasonal cycle in the STZ being weak (~0 to 0.5 mg m-3) between summer minima and winter maxima, (Figure 1a, b) (see also Allison et al. [2010]) the overall variance of the chlorophyll signal is strongly phase locked to the mean seasonal cycle (>70% of variance explained) (Figure 4)."
"In the ACZ and MIZ, the amplitude of the seasonal signal is much higher (see also Allison et al., [2010])"

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 acronymns, to which a reader could quickly refer to when reading the text.

The following table of all acronyms used in the text has been added to the manuscript

Southern Annular Mode	SAM
Subtropical Zone	STZ
High Nitrate Low Chlorophyll	HNLC
Iron	Fe
Sea-viewing Wide Field of view Sensor	SeaWiFS
Maps of Absolute Dynamic Topography	MADT
Antarctic Circumpolar Current	ACC
Subantarctic Front	SAF
Polar Front	PF
Southern Antarctic Circumpolar Current Front	SACCF
Subtropical Front	STF
Mixed Layer Depth	MLD
Photosynthetically Active Radiation	PAR
Subtropical Zone	STZ
Transition Zone	TZ
Antarctic circumpolar zone	ACZ

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.

The following references have been removed from the text in an attempt to be more selective and reduce over referencing. In addition, and idea or concept has been reduced to a maximum of three references.
Böning et al., 2008;
Cullen, 1991
Nelson and Smith, 1991
Price et al., 1994
Arrigo et al., 1998;
Coale et al., 2004
Gervais et al., 2002
Veth et al., 1997

Smith et al., 2000
Perissinotto et al., 1992
Pollard and Regier et al., 1992
Korb and Whitehouse 2002
Venables and Meredith 2010
Smith and Gordon, 1997
Hense et al., 2000
Johnson et al., 1997

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 read them without some sort of magnification.

We have increased the size of the fonts for most labels. Hopefully the figures are now easier to read. Some additional information has also been added in the figure captions to help interpretation.

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

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

This figure is correctly referred to in the methods section of the manuscript (p4769, 20). "Inter-annual variability in the bloom initiation date was calculated as the standard deviation of the bloom initiation dates for each of the 9 years."
The figure first appears in the discussion section 3.2 as Figure 3 and is referred to on numerous occasions throughout the text. The figure legend is as follows:

"Figure 3: Inter-annual variability (standard deviation over the 9 years) in phytoplankton bloom initiation dates in the Southern Ocean south of 30ºS."

(p4777, 4) and numerous other places throughout the text. R2 values are missing decimal points (i.e. R2 = 91 should be R2 = 0.91).
Corrected

(p4781,12) Superfluous "of"
Corrected

(p4790, 5) Behrenfeld is misspelled.
Corrected

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.
Standard deviations are expressed in the units of the data (8 days periods as we are dealing with bloom initiation dates). It is here positive (square-root of the variance, itself a positive value).
The figure legend for figure 3 now reads as follows:

“Figure 3: Inter-annual variability (standard deviation over the 9 years) in phytoplankton bloom initiation dates (units in 8 day periods) for the Southern Ocean south of 30ºS”

END OF REVIEW

Response to Anonymous Referee # 2 comments

General Comment

This manuscript examines the seasonal variability in satellite derived chlorophyll in the Southern Ocean and describes the potential physical/chemical factors underlying the seasonality. Overall I found the manuscript a solid piece of work and not uninteresting.

Major comments

My major comment is that the authors use the correlation of the mean seasonal cycle with the ‘raw’ data to define ‘seasonality’. If there is a high correlation, then by the authors’ definition the region has high seasonality. However, a high correlation only means that there is a relatively stationary seasonal cycle that doesn’t change much interannually. This doesn’t necessarily mean it doesn’t have a strong seasonal cycle, just that interannual variability in the seasonality is low. Similarly a low correlation is described by the authors as low seasonality. In fact, it just means that a stationary seasonal cycle does not capture the seasonality in chl, even though there could be a very strong seasonal cycle. It just means that there’s large interannual variability in that seasonal cycle. I would prefer it if the authors find some other terminology because their definition of low/high seasonality probably doesn’t match most readers understanding of low/high seasonality.

We have defined the seasonal cycle as a simple repetition of itself, i.e. the seasonal cycle (averaged over the period available) it is neither amplitude nor phase-modulated. We have chosen this definition in order to illustrate how reproducible (or “predictable”) the evolution of chlorophyll is and to delineate different regions based on this property. We are aware that there have been alternative definitions for the seasonal cycle, whereby phase and amplitude modulation is allowed. For example, Vantrepotte and Melin (2009) and Vantrepotte et al., (2011) used the Census X-11 iterative band-pass algorithm on time-series of Ocean Colour, and Particulate Organic Carbon estimates from the SeaWIFS record. In climatology, theoretical arguments and implementation methods have been proposed by e.g. Pezzuli et al (2004) and Wu et al (2008).

In order to prevent confusion between our definition of the term high and low seasonality and that of other readers, we have adjusted the term from seasonality to seasonal cycle reproducibility. The definition for high and low seasonal cycle reproducibility now reads as follows:
“The percentage of the variance explained by the mean seasonal cycle (Figure 4) defines how well the mean climatological seasonal cycle (from 9 years) represents the

evolution of chlorophyll over each year. Areas where the seasonal cycle for each year is coherent with the 9 year mean ($R^2 > 0.4$) are defined as having high seasonal cycle reproducibility. Regions where there is large variability from year to year in the timing and amplitude of the bloom and only a low percentage of the variance is explained by the mean seasonal cycle ($R^2 < 0.4$w) are defined as having low seasonal cycle reproducibility."

Also, it's not clear how the authors define intra-seasonal variability (perhaps intra-annual would more appropriately describe what they mean?). I think improving the terminology used would make the paper much less confusing once the authors start to discuss intra-seasonal variability and seasonality in detail (earliest example I marked on the m/s was at P4779, L12-14). `

The following text in parenthesis was added to the first paragraph of section 3.2 in order to assist the reader in our definitions of inter-annual and intra-seasonal variability.
"The characteristics of the seasonal cycle of phytoplankton biomass in the Southern Ocean are examined in terms of the timing of the bloom initiation, its amplitude, inter-annual variability (variability from year to year) and intra-seasonal variability (variability of phytoplankton biomass within a season) and the importance of the climatological seasonal cycle in explaining the overall variance."

Specific comments

Missing data: Cloudiness and winter darkness must seriously affect the amount of chlorophyll data retrieved in this region. A 'cloud atlas' showing % of days with missing data would be very useful (perhaps in the supplemental info). I would like the authors to include a critical assessment of how missing data affects their ability to accurately determine the start of the spring bloom, to assess the interannual variability in bloom timing, and to assess whether the seasonal cycle is stationary or not. One example of where persistent cloudiness affects bloom estimation might be (P4778, L13-14) south of Australia? In relation to above, Figure 1b shows the chlorophyll in winter – but how much data is there in winter? Do some of the pixels in this plot represent just one winter retrieval in the entire SeaWiFS time series?

Ice-cover, cloudiness and low solar angles characterize the Southern Ocean south of ~50ºS during the winter months. It is exemplified in figure 1a, where south of 50ºS more than ¾ of the data were missing and consequently masked in grey (see also supplementary figure S5a).

The following text has been added to the methods section to highlight this data criteria:
"When computing averages, a criteria of at least ¼ of the maximum possible number of observations available was used. Grid-points where this criteria was not satisfied (i.e. more than ¾ of the data is missing) are discarded and appear in grey in the figures."
"See supplementary material for available data used to calculate averages in July (Figure S5a) and January (Figure S5b)."

The following text has been added to the Methods section pointing to the supplementary material for an assessment of the effects of missing data on accurately calculating the bloom initiation dates and inter-annual variability:

“An assessment of how missing data affects the ability to accurately determine the start of the spring bloom appears in the supplementary material.”

The following text on the assessment of the effects of missing data on accurately calculating the bloom initiation dates and inter-annual variability now appears in the supplementary material:

Accurately assessing bloom initiation dates in regions of poor data coverage

Significant problems are known to exist in ocean colour retrievals at high latitudes that are related to low solar angle, sea ice cover and clouds. If the effects of poor data coverage were significantly impacting the calculation of bloom initiation dates, one would expect a systematic delay in bloom initiation date with an increase in latitude. This however is not what we observe, south of Australia for example, regions of late bloom initiation (November/December) are found north (40-50°S) of early bloom initiation regions (August/September) further south (50-50°S) (Figure 2). The transition between late and earlier bloom initiation south of Australia appears to be delineated by the Polar Front (Figure 2), which implies that the differences in bloom initiation date are physically driven rather than a poor data-related artefact.

Although it is no possible to directly determine the effects of poor data coverage on the inter-annual variability in bloom initiation dates, one would similarly expect that the variability in bloom initiation date would systematically increase with latitude if it were dependant on the number of observations. This however was not the case and instead, the pattern of variability in bloom initiation date shows the highest degree of variability in the TZ between different seasonal regimes centred around ~40°S (Figure 3).

Furthermore, the bloom initiation date north of the MIZ and south of ~40°S and is found to occur mainly between September and November. The number of available SeaWiFS data observations (8-day composites) for September (Figure S5c) shows that outside the MIZ, at least half of the total number of potential observations are generally available during spring. Missing values are thus unlikely to bias the determination of the bloom initiation dates north of the MIZ. As we cannot detect a phytoplankton bloom in an ice-covered grid-point, one can expect that there will be a delay in the bloom initiation date related to the MIZ. This was found to be the case with bloom initiation dates in the MIZ occurring in summer (December to February) reflecting the time it takes for phytoplankton blooms to respond to the newly created ice-free waters. This was similarly shown to be the case in the Arctic where phytoplankton blooms took ~20 days to respond to sea-ice melt (Perrete et al., 2011).

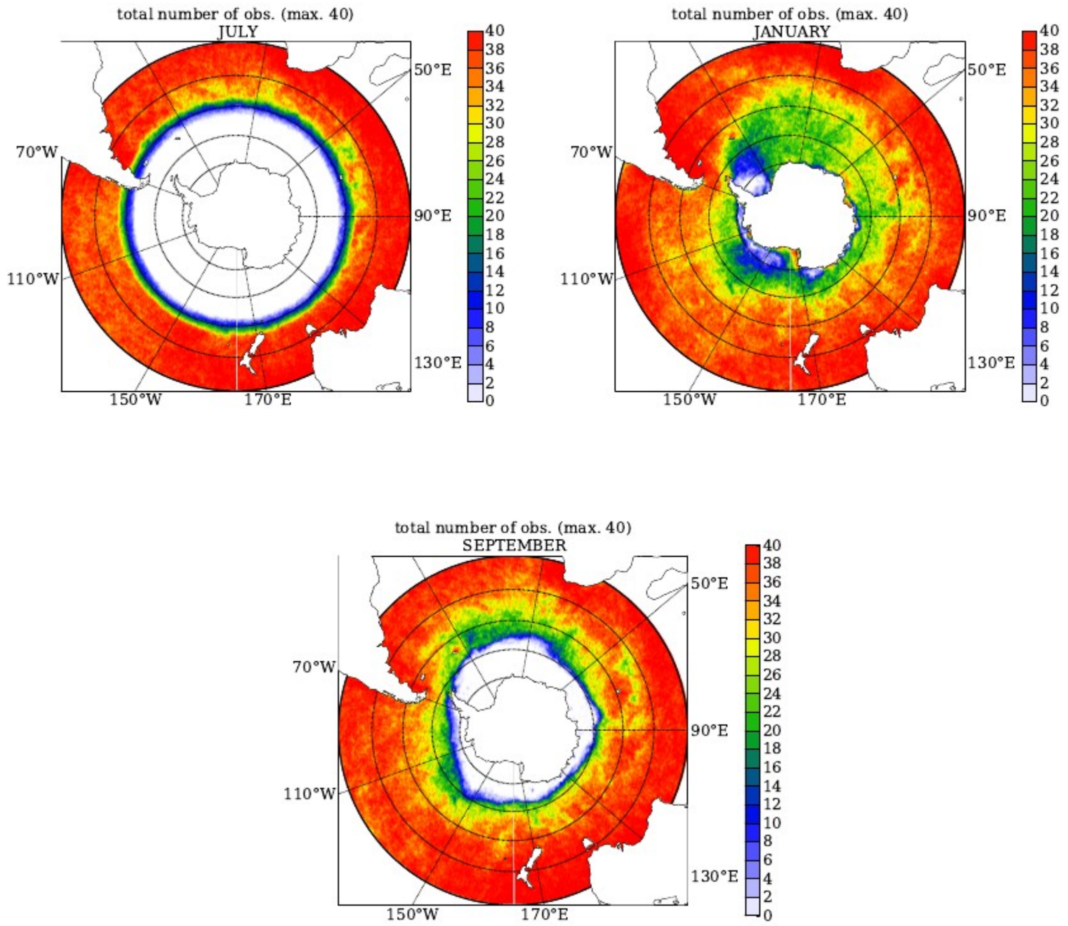

Figure S5. Maps presenting the total number of available observations of SeaWiFS data (8-day composite periods) from 1998 to 2007 for a) July and b) January and c) September

Abbreviations: are numerous and after putting down the m/s halfway through it, I was completely lost when I picked it back up a few days later! A list of abbreviations would be helpful for people with a bad memory and short attention span, like me.

The following table of all acronyms used in the text has been added to the manuscript:

Southern Annular Mode	SAM
Subtropical Zone	STZ
High Nitrate Low Chlorophyll	HNLC
Iron	Fe
Sea-viewing Wide Field of view Sensor	SeaWiFS
Maps of Absolute Dynamic Topography	MADT
Antarctic Circumpolar Current	ACC
Subantarctic Front	SAF
Polar Front	PF
Southern Antarctic Circumpolar Current Front	SACCF
Subtropical Front	STF
Mixed Layer Depth	MLD
Photosynthetically Active Radiation	PAR
Subtropical Zone	STZ
Transition Zone	TZ
Antarctic circumpolar zone	ACZ

Correlation coefficients: throughout m/s these should be r2 = 0.91 etc.(rather than r2 = 91).

Corrected

Also, please report 'p-values' or levels of statistical significance.

P-values are systematically smaller than 0.01 (maximum p-value given for region E is e.g. 2*10-32). Thus all the correlations between mean seasonal cycle and raw-time-series are statistically significant at the 99% confidence level. It is now reported in the figure captions.

Non-seasonal regions: The authors suggest that some regions have low chlorophyll and weak seasonal cycles (e.g. P4778, L8-9 and P4786, L18-20). Is it appropriate to talk of 'blooms' in these regions? Or instead of a well-defined seasonal cycle, does chl just oscillate about some mean on much shorter time scales?

Although chlorophyll concentrations in these regions remains low throughout the year, we use the word "bloom" in reference only to the bloom initiation date which is defined statistically in the methods section 2.3.

"We use "bloom" to refer to events to refer to events of elevated chlorophyll concentration, without reference to a particular concentration threshold. The initiation of the bloom (or the date of bloom onset) is understood here as the period of the year registering a relative increase in chlorophyll concentration, irrelevant of the actual value. The chlorophyll bloom is defined statistically, as in other studies (Henson and Thomas, 2007; Follows and Dutkiewicz, 2002; Siegel et al., 2002)."

Both examples that the referee refers to (P4778, L8-9 and P4786, L18-20) are examples where the authors have implied a lack of a distinguished seasonal cycle "chlorophyll concentrations are so low that the seasonal cycle is indistinguishable from intra-seasonal noise". These statements are misleading and we have corrected them to instead read as follows:

P4778, L8-9
"This region is particularly extensive in the Pacific (~40-50oS), where chlorophyll concentrations are so low and intra-seasonal noise so high that only a low percent of the variance can be explained by the mean seasonal cycle (e.g. Figure 5d; R2 =0.27)."
P4786, L18-20
"These regions are characterised by chlorophyll concentrations that are so low and intra-seasonal variability so high that only a small percent of the variance (<20%) is explained by the mean seasonal cycle."

Transition zones: Might be worth mentioning similar work done in the North Atlantic on bloom timing, transition zones, links to physics etc. e.g. Dutkiewicz et al. (2001), Deep Sea Res; Henson et al. (2009), J Geophys Res.

The following additional text and / or references have been added to the paper highlighting the relevant studies by Dutkiewicz et al. (2001) and Henson et al., 2009:
"The amplitude of the seasonal bloom would depend on the supply of Fe via mechanisms that include a) the seasonal re-supply of Fe through winter overturning (wintertime convective mixing sets the available nutrients for new production [Dutkiewicz et al., 2001],"
"These findings are supported by a study in the subtropical North Atlantic where anomalously strong convective mixing was shown to result in enhanced chlorophyll concentrations (Dutkiewicz et al., 2001)."
"(see also the subpolar regions of the North Altantic where deep mixing led to lower phytoplankton abundances [Dutkiewicz et al., 2001]."
"Similar high variability in bloom initiation dates was found in the transition zone separating regions of winter bloom initiation in the subtropical North Atlantic from May bloom initiations in the subpolar North Atlantic (Henson et al., 2009)."
"Transition regions of low seasonal reproducibility are thought to be driven by a combination of multiple limiting factors and forcing mechanisms from both seasonal regimes (e.g. nutrient and light limitation, see Dutkiewicz et al., 2001 and Henson et al., 2009)."

Figures: All of the figures had miniscule axis labels, colour bars and contour labels. Please enlarge.
We have increased the size of the labels on figures, colour bars and countours

Supplementary material: I found this to be superfluous. Much of the text is repeated in the main manuscript. However, if the authors choose to retain it. . ..The caption didn't supply me with enough information to deduce what the many subplots in the figures were. Presumably different latitude bands??

The supplementary figure legend has been expanded and now reads as follows:

Figure S1-4: 8 boxes were extracted, each spanning 5 degrees in latitude (from 30ºS to 70ºS) for a 10º longitude transect in the Atlantic (0 – 10ºE), the Indian (85 – 95ºE) and the Pacific (110 – 100ºW). The upper panel of each box presents the MLD (black curve, depth in meters, y-axis on right) and the mean annual cycle of PAR (red curve, Einstein m^{-2} day^{-1}, left-hand y-axis) with a minima / maxima envelope in yellow. The middle panel presents the mean annual cycle of chlorophyll (mg m^{-3}). The lower nine panels present the chlorophyll concentration for each year (y-axis) and the time of the year (x-axis). Years are defined from May 0 to April +1. Bloom initiation dates are marked by a black circle.

Figure S1: Subtropical Zone (STZ) boxes from longitudinal transects in the Atlantic (0 – 10ºE) a) 30-35ºS, the Indian (85–95ºE) b) 30-35ºS, and the Pacific (110–100ºW) c) 30-35ºS, d) 35-40ºS.

Figure S2: Transition Zone (STZ) boxes from longitudinal transects in the Atlantic (0–10ºE) a) 35-40ºS, the Indian (85–95ºE) b) 35-40ºS, and the Pacific (110–100ºW) c) 40-45ºS, d) 45-50ºS.

Figure S3: Antarctic Circumpolar Zone (ACZ) boxes from longitudinal transects in the Atlantic (0–10ºE) a) 40-45ºS, d) 45-50ºS, g) 50-55ºS, the Indian (85–95ºE) b) 40-45ºS, e) 45-50ºS, h) 50-55ºS, j) 55-60ºS, and the Pacific (110–100ºW) c) 50-55ºS, f) 55-60, i) 60-65ºS.

Figure S4: Marginal Ice Zone (MIZ) boxes from longitudinal transects in the Atlantic (0–10ºE) a) 55-60ºS, d) 60-65ºS, g) 65-70ºS, the Indian (85–95ºE) b) 60-65ºS, e) 65-70ºS, and the Pacific (110–100ºW) c) 65-70ºS.

Technical comments:

P4764, L1: The first line of the abstract made little sense to me.
Sentence has been adjusted to read as follows:
"The seasonal cycle is the mode that couples the physical mechanisms of climate forcing to ecosystem responses in production, diversity and carbon export."

P4764, L10: What do the authors mean by 'the more dynamically linked characteristics'?
Sentence adjusted to read as follows:
"The study highlighted important differences between the spatial distribution of satellite observed phytoplankton biomass and the more dynamic characteristics of the seasonal cycle."

P4768, L15-20: the authors state that they use bloom without reference to a particular threshold. But then they go on to use a threshold method (the 5% above median approach).
Sentence adjusted by adding the word concentration before threshold and now reads as follows:
"We use ''bloom'' to refer to events of elevated chlorophyll concentration, without reference to a particular concentration threshold."

P4772, L14-16: Figure 1a doesn't show a rapidly-forming bloom.
The word 'rapidly' has been deleted

P4772, L26: Reference to ACC which is not plotted on the figures.
This specific comment is unclear to me, the reference on P4772, L26 is to the major fronts of the ACC which is plotted on Figures 1a and b and labelled in the figure legend as the STF (red), the SAF(black), the PF (orange) and the SACCF (blue).

P4774, L18-P4775, L10: Is this paragraph necessary? As the authors aren't able to address grazing or growth rate with their dataset and furthermore use Sverdrup's arguments in their later discussions of physics-biology interactions, this whole section seems out of place.

Indeed, this paragraph never appeared in the original manuscript but was added in the revised submission of our manuscript based on the editors recommendations. The specific comment that this paragraph is a response to reads as follows:

This paper clearly espouses a bottom-up control of phytoplankton dynamics by light or nutrients. Given that there is no analysis of growth rates to suggest that grazing is negligible in this part of the ocean, this is a growth simplification. A recent paper (similarly based on remote sensing ocean color) by Behrenfeld (in Ecology) suggests that in the North Atlantic one cannot explain phytoplankton annual cycle w/o understanding the role of the loss processes (which are likely dominated by grazing). This idea is not new (e.g. work by Evans and Parslow and Banse among many others). I don't see why it will be different in the Southern Ocean. That said, nutrients and light are likely to play an important role as well. What we observe in the chlorophyll signal is the balance of growth and loss processes not simply just growth processes. Behrefeld's paper results have been found to be consistent with in-situ data (Boss and Behrenfeld, 2010, GRL).

As this paragraph has been added to the original manuscript in response to the editors comments it remains in the manuscript.

P4779, L1-2: Have the authors already stated somewhere what their hypothesis is? Must have missed it. . .

The first line of the first paragraph has been adjusted so that it now starts with "We hypothesize that …."
"We hypothesize that low seasonal reproducibility related to the transition between regions of different bloom initiation is potentially related to the diversity of conditions encountered at the confluence between contrasting seasonal regimes."

P4781, L12-14: Could the increase in chl be partly due to relief of nutrient limitation by mixing up of new nutrients, combined with sufficient light for growth (as this is relatively low latitude region)?

Yes, this is exactly what we meant. In order to be more clear, I have added the words "and associated nutrient replenishment" to that particular sentence which now reads as follows:
"Consistent with our understanding of the subtropics as a nutrient-limited regime, the correlation in the STZ is uniformly positive (>0.8), with increased chlorophyll coinciding with deep winter MLD's and associated nutrient replenishment.

P4781, L28-29: I didn't understand what the authors meant by 'a simple overturning threshold initiates the seasonal cycle'.

In order to expand on what is meant, but without making the sentence too cumbersome I added the 'net heat loss in winter' in parenthesis. The sentence now reads as follows:
“Unlike in the subtropics, where a simple overturning threshold (net heat loss in winter) initiates the seasonal cycle, the physical forcing mechanisms of the mixed layer dynamics south of 40oS are more varied and complex, as are the requirements to promote phytoplankton growth.”

P4784, L12: Where did these % figures come from?
Percentages come from Lefevre and Watson, 1999 and Archer and Johnson, 2000 as referenced in the text.
Lefevre and Watson, 1999 state that: “more than 99% of the iron supply to the surface in the present day comes from upwelling and not from the local atmospheric flux”
Archer and Johnson, 2000 state that: “a majority (70-80%) of the global carbon export production can be sustained by upwelling of dissolved iron in seawater rather than by atmospheric deposition.”
Rather than lumping the percentages and references together into a single percentage range of 70 – 99% with both references together, I have rewritten this sentence to separate out the individual percentages that belong to each reference.

The sentence now reads as follows:
“If the main source of Fe to the surface waters of the Southern Ocean is through upwelling; 70 – 80% (Archer and Johnson, 2000), >99% (Lefevre and Watson, 1999), one can deduce that in the Pacific, where Fe supply to surface waters through deep water entrainment is limited, shallow winter mixed layers are unlikely to be deep enough to entrain sufficient Fe into the surface waters for stimulating and maintaining high production rates through the summer.”

P4785, L17-18: What is the definition of ‘high’ and ‘low’ chlorophyll and ‘high’ and ‘low’ seasonality used in Figure 9?
Figure 9 has been redone, this time using defined thresholds for high and low chlorophyll and seasonal reproducibility. The text has been updated to read as follows:
“The four regions result from a combination of high (> 0.25 mg m^{-3}) or low (< 0.25 mg m^{-3}) chlorophyll concentration and high (R^2 > 0.4) or low (R^2 < 0.4) seasonal reproducibility.”

P4787, L1-2: The authors do not demonstrate that the time series is entirely explained by light, etc. Please rephrase as a hypothesis, rather than fact.
This sentence has been corrected and now reads as follows:
“In these regions, we hypothesize that the annual time series is almost entirely explained by the seasonal forcing of light, heat flux and seasonal MLD (as in the subtropics).”

P4787, L3-5: The meaning of this sentence eluded me entirely, I’m afraid.
I have rewritten this sentence and hope that it's meaning is now more clear:
“This is not to say that Fe or light is not a limiting factor but merely that their influence does not vary sufficiently on intra-seasonal or inter-annual time scales to impact the high reproducibility of the phytoplankton seasonal expression.”

P4787, L5-7: A low supply of new nutrients wouldn't prevent a bloom from starting. It just wouldn't last very long.

True, thank you for your comment, we have ammended the sentence to read as follows:

"In such instances sufficient winter preconditioning of the water column with limiting nutrients allows for a consistent initiation of the bloom, the duration and integrated seasonal amplitude of which depends on the amount of Fe made available; through winter overturning, the depth of the summer mixed layer relative to the nutricline, lateral advection of Fe into surface waters, upwelling at fronts or dust deposition."

P4787, L29-30: Is the seasonal cycle the most important mode of variability in climate change??? Please add suitable references here if so.

On addressing this comment it became apparent that the final paragraph of this manuscript was rubbish and it has thus been rewritten (this time without the incorrect statement that the seasonal cycle is the most important mode of variability in climate change). Thank you again to the reviewer for bringing these shortcomings to our attention. The final paragraph now reads as follows:

"The seasonal cycle is the mode that couples the physical mechanisms of climate forcing to ecosystem responses in production, diversity and carbon export. Accordingly, long term trends in Southern Ocean productivity will be mediated through changes to the characteristics of its seasonal cycle. Our dynamic characterisation of the Southern Oceans seasonal cycle may therefore be important in understanding regional differences in the response of the biological pump to climate variability. Future studies that better address the mechanisms governing this spatial variability will allow us to make more robust predictions of the Southern Oceans carbon cycle."

Figure 6: The regions are referred to by colour in the figure caption but by region A-D in the text. Please cross-reference them (my preference would be to add to the text Region A (light blue) etc.)

Done

Figure 7: Mark the contour of the 95% significance level for the correlation on this plot.

The 95% significance contour has been added as a black contour on the figure 7.

Figure 8: The contouring on this plot is just horrible (and the contour labels are unreadable). Could the contouring be made smoother somehow? Also please mark the latitude of the fronts on the figure. It would help to follow the related discussion.

The original dataset for the MLD (described in DeBoyer-Montegu et al, 2006) is available at the monthly time-scale and on a coarse (2 degree x 2 degree) grid, hence the appearance of the contours. We've reduced the number of contours, increased the font size of the labels, and highlighted the labels in the hope it makes them more readable. The position of the fronts (SubTropical Front, Sub-Antarctic Front, Polar Fronts and Southern ACC Front) has been added to the figure. Following remarks from the reviewer #1. We've modified the plot so that it doesn't display the anomalies in log(Chl-a) with respect to the annual mean but simply the raw values on a log-scale (see colorbar).

Figure 9: Add to caption which are Regions A-D.
Done

Figure 9: This plot had a lot of information on. I think I would have found it more useful if the authors had included some discussion of how this 'province' definition is of relevance to higher trophic levels or carbon export or defining future research priorities etc.

The following text discussing the potential impact on higher trophic levels was added to the discussion (but not the synthesis):
"Inter-annual variability in the timing of the bloom initiation can have significant impacts on he success of zooplankton and larval fish populations, e.g. if a mismatch in timing between food availability and critical life stages of higher trophic levels occurs, their survival rate is likely to be reduced (Henson et al., 2009)."
The following paragraph discussing the relevance of changes in community structure to carbon export was added to the synthesis:
"This conceptual framework for characterising the response of the biological seasonal cycle to the underlying physics emphasizes the role of bottom-up controls of light and nutrients in the Southern Ocean. The lack of information on growth rates and community composition does not allow us to quantify the roles of grazing and biogeography in determining the observed patterns of seasonal characteristics. Knowledge of the changes in biological factors (e.g. grazing and community structure) is however important if we are to understand the biological response to future climate change. For example, changes in climate may facilitate a shift in the species composition in a manner that can alter the elemental composition of particulate matter, cell size and the trajectory of primary production through the food web, influencing the proportion of the biomass exported to the deep sea (Finkel et al., 2010). Looking to the future, the development of ecosystem appropriate functional type algorithms for the Southern Ocean will allow us to use satellite remote sensing data to routinely provide information on the response of phytoplankton community composition and physiology to physically distinct seasonal regimes and ultimately to climate change."

The final paragraph of the manuscript has been rewritten and now touches of the importance of this 'province' characterisation to future studies:
"The seasonal cycle is the mode that couples the physical mechanisms of climate forcing to ecosystem responses in production, diversity and carbon export. Accordingly, long term trends in Southern Ocean productivity will be mediated through changes to the characteristics of its seasonal cycle. Our dynamic characterisation of the Southern Oceans seasonal cycle may therefore be important in understanding regional differences in the response of the biological pump to climate variability. Future studies that better address the mechanisms governing this spatial variability will allow us to make more robust predictions of the Southern Oceans carbon cycle."

END OF REVIEW

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