

## ***Interactive comment on “Long-term trends in ocean plankton production and particle export between 1960–2006” by C. Laufkötter et al.***

**C. Laufkötter et al.**

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We wish to thank reviewer #1 for the detailed analysis of our paper and his/her thoughtful comments, which have been very helpful and greatly improved the quality of this manuscript. A detailed reply to each point follows below:

### **Response to Reviewer Comment #1**

***Reviewer Comment: The authors investigate the impact of climate change on primary production and export of particulate organic carbon (POC) in the ocean***

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*for the period 1960-2006 using a hindcast simulation from a global coupled physical-biogeochemical model. The authors perform a thorough analysis of how changes in the physical environment affect ecosystem structure and lead to changes in primary production and POC export in different regions of the ocean, and present some interesting results. However, there are some issues with clarity and specificity in the methodology and presentation of the results that make the manuscript hard to follow in some places, and potentially raise questions about the validity of some of its results and conclusions. More detailed comments follow below.*

***Reviewer Comment: The CCSM BEC model has been extensively described in Moore et al 2004 and Doney et al 2009a, 2009b. So the first part of Section 2.1 (Model description, page 5927) can probably be shortened. It would suffice to include a basic description of the model, cite the previous papers and comment on the differences between the version used in this study and the versions used in those papers. I also do not think it is necessary to include the full equations and parameter values for the BEC model in the Appendix.***

Author Response: We have shortened the Section 2.1 accordingly. Although Moore et al., 2002 and Doney et al., 2009 provide a full description of the equations and parameters of the model, we repeat the most important ones describing phytoplankton growth, zooplankton grazing, and the production and sinking of particles in the Appendix. This permits us to better connect our results with the particular implementation of the respective processes in the model. We also give a list with the differences between our version and the one used in Doney et al., 2009 in Table 1. These changes were incorporated to improve the model results relative to observations, but were relatively small and without major consequences.

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***Reviewer Comment: In Section 2.2 (Forcing), the authors state that the 3000-year spin-up run was forced with "climatological means" of the inter-annual forcing (CIAF). Was the spin-up run forced with the averaged CIAF fields or was it forced with with the CORE CNYF v2 (Corrected Normal Year Forcing version 2)? This is a very important distinction. Averaging inter-annual forcing into an annual climatology removes high frequency variability, and that has severe negative effects on the quality of the simulation and model skill. The CNYF v2 is a reconstructed "normal year" that maintains high frequency variability. Another point is that 3000 years is a fairly long time for a spin-up. There could be significant model drift. Was there any attempt to quantify model drift? This is particularly important given how small the changes in global NPP and export production are. The authors should elaborate more and be more explicit on the forcing used for the spin-up run and the control runs that were made, so these issues can be clarified.***

Author Response: The forcing Section now states:

A two step-procedure was used to generate our hindcast simulation. First a 3000 year preindustrial spin-up simulation forced at the surface with CORE CNYF v2 (Common Ocean-Ice Reference Experiments Corrected Normal-year Forcing (Large and Yeager, 2004) was conducted. This long spin up resulted in a model with a negligibly small drift in surface nutrient concentrations and primary and export production. It also resulted in stable deep ocean radiocarbon values and a minimal net air-sea flux of CO<sub>2</sub> (Graven et al., 2012). Second, in 1950, the physical forcing was switched to CORE CIAF version 2 (Common Ocean-Ice Reference Experiments Corrected Inter-Annual Forcing (Large and Yeager, 2004) and we ran the model forward in time through the end of 2006. This transient forcing data set was calculated using the NCEP reanalysis dataset (Kalnay et al., 1996) and satellite based estimates of radiation, sea surface temperature, sea-ice concentration and precipitation (Large and Yeager, 2009) and hence represents the full suite of physical forcings affecting the ocean.

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Although the two CORE forcings were constructed in order to minimize the model “shock” when switching the forcing from the normal year forcing to the interannually varying one, we nevertheless do not consider the first 10 years of the transient simulation. This results in 47 years of model data for analysis, i.e., from Jan 1960 through Dec 2006. To ensure the absence of drifts over this period, we conducted also a control simulation forced with the normal year forcing (CORE CNYF v2) over the same period. As was the case with the spinup, plankton biomass and export exhibited a negligibly small drift, with changes of less than 0.1% over these 47 years.

The CORE forcings include annual mean river runoff, but we considered this flux in the freshwater forcing only, but not in the input of nutrients. There is also no atmospheric deposition of macro-nutrients in this version of BEC. We use a constant climatology to prescribe atmospheric iron fluxes on the basis of the data from Mahowald et al., 2009.

***Reviewer Comment: On page 5931 line 5, the authors state that global NPP is 4.8 Pg/y. Either the value or, most likely, the unit is incorrect. It should probably be Pg/month.***

Author Response: Thank you for pointing out this typo. It should have been 48 Pg/year.

***Reviewer Comment: On page 5931 lines 17-18, regarding the BEC estimates of total export being significantly lower than other models. More recent studies (Henson et al 2012 and Lutz et al 2007) have total export estimates of the order of 5 PgC/y, which are consistent with the CCSM BEC results presented in the manuscript.***

Author Response: Thank you for this indication, we have included numbers from Henson et al. and Lutz et al. in our manuscript.

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***Reviewer Comment: In Section 2.4 (Calculation of trends), it would be helpful if the authors stated explicitly the temporal frequency of the model data used to compute the trends. Did they use weekly, monthly or annual model output? The mention of specific months and seasons in the text suggests that they used monthly data. But it's not clear. If they used monthly or weekly data, was the seasonal cycle removed before computing the trends? The authors should provide more detail on how the trends were computed so the reader can better evaluate the results presented. Do the time-series plots (Figures 2 and 9) show annual means or deseasonalized monthly means? It would also be helpful if, in addition to the definitions of export production, NPP and phytoplankton and zooplankton biomass, the authors also included the units used for these quantities (between parentheses, perhaps).***

Author Response: We use annual mean model output for the calculation of trends, as annual means are more robust than monthly means for calculation of long-term trends. All time-series show annual means. We mentioned in the manuscript that our trends persist in all seasons, but we did not show these results. We have revised this part of the manuscript for clarity. In addition we added the units for NPP, EP and plankton biomass and revised the Section "Calculation of Trends", which reads now as follows: "All trends presented in this work were computed using a linear regression on annual mean model output. Changes were obtained by multiplying the slope of the linear regression with the number of years. We tested for the significance of all trends with a two-sided Student t-test (requiring a level of significance  $\alpha = 0.05$  or alternatively requiring a p-value of the regression of less than 0.05). To account for autocorrelation in the timeseries, we reduced the degrees of freedom when performing the t-test as described in e.g. Zwiers and van Storch, 1995. Typically, this reduced the degrees of freedom by 36%. We report primarily percent changes, which we obtained by

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normalising all results to the decadal mean of the first ten years (1960-1969). For the maps, the changes were calculated for each grid cell and non-significant changes are shown in white."

**Reviewer Comment: The units in Figure 1 are a bit confusing. Panels a, c and e show percentages and panels b and d show mol C/m<sup>2</sup>/yr. So are the trends/percentages in panels a, c and e %/year or percent over the 47 years of the simulation? In the caption, the authors should be more explicit about what is being shown in the figure. I would also change the color of the land to something other than light blue. This makes it look like the trends in the light blue regions in the ocean are not significant. The same comments apply to Figure 4. The authors use "PP" in Figure 1a,b and "NPP" in the text. The same acronym should be used everywhere.**

Author Response: Done. Both figures show changes over the 47 years of simulation, we have revised the description of the Figures accordingly.

**Reviewer Comment: On page 5932 line 16, the authors state that the decline in NPP and EP in the Southern Ocean is greater than the inter-annual variability. Looking at Figure 2, that is not very clear. There is considerable inter-annual variability in the Southern Ocean and other regions as well. But the authors do not quantify nor show any estimates of the model's inter-annual variability to contrast with the computed trends. Given the model's inter-annual variability, are the observed trends and changes significant? This is a very important issue given how small the changes in NPP and export production (6% and 7%, respectively) are in that 47-year period. For example, Henson et al. (2010) and Yoder et al. (2010) argue that longer time-series of at least 40 years are needed to distinguish a climate change signal from natural variability. So**

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**according to Henson et al. (2010), the 47-year hindcast run is barely long enough to detect a climate change signal.**

Author Response: All trends and changes shown in our manuscript have been tested for significance with a t-test ( $\alpha = 0.5$ ). Degrees of freedom have been reduced when performing the t-test as described in e.g. Zwiers and von Storch (1995) to account for temporal autocorrelation. Regarding the maps of trends, trends have been calculated for each grid cell and non-significant trends are shown in white. There is no strong ENSO event at the beginning or end of our simulation period that could possibly bias our trends.

However, significant changes between 1960 and 2006 do not necessarily show climate change caused by increase of antropogenic CO<sub>2</sub> in the atmosphere. The trends could as well be part of a decadal-scale oscillation in climate and rather reflect natural variability. We realized that our presentation of this topic is not very clear and we added a paragraph on trends vs variability in the introduction and also discussed this issue in the discussion and caveats Section. In addition we expanded our explanation of how the trends are calculated and refer to this throughout the manuscript.

***Reviewer Comment: Figure 2 lacks units for the variables shown in panels c, d and e. It's also not clear what is being shown in the time-series plots. Are these annual means or deseasonalized monthly means? More information about the plots should be included in the caption.***

Author Response: Done.

***Reviewer Comment: On page 5932 line 22, it looks like it should be Fig. 1e NOT Fig. 1c.***

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Author Response: Thank you for pointing out this typo. Done.

***Reviewer Comment: On page 5933 lines 1-4, the authors talk about the relationship between "changes" in NPP and "changes" in SST and refer to Figure 3. What exactly is being shown in Figure 3? From the magnitude of the values and number of data points, it looks like a plot of global NPP vs global mean SST from a series of model runs. If so, where do these model runs come from? How were they made? If these are not model runs, how did the authors obtain the global NPP and SST values in the plot? I did not find any mention of it in the methodology or figure caption. In the text (lines 1-4), the authors refer to "changes" in NPP being correlated with "changes" in SST, which implies the figure shows delta NPP vs delta SST. But the figure caption says "annual NPP as a function of changes in SST", which implies NPP vs delta SST. And yet the magnitude of the values suggests that these are global integrals of NPP vs global averages of SST. In addition, in the text the authors mention the relationship/correlation changes with latitude, but I don't see any information on latitude in the figure. The reader cannot properly evaluate the results and arguments presented without knowing exactly what is shown in Figure 3.***

Author Response: We have changed both the Figure 3 and the text referring it. The new text reads now as follows:

"NPP (and EP) are negatively correlated with SST (Fig. 3) in the low latitudes and show weak correlation in the high latitudes. The global average NPP is negatively correlated with SST, reflecting mostly the changes in the low latitudes."

We uploaded the new Figure with the caption "Figure 3: Relationship between anomalies of annual mean depth-integrated NPP and anomalies of annual mean SST"

As the full caption does not fit into the input mask, we cite the full caption here:

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**Figure 3** Relationship between anomalies of annual mean depth-integrated NPP [ $\text{mol C/m}^2/\text{yr}^{-1}$ ] and anomalies of annual mean SST [ $^{\circ}\text{C}$ ] for three regions. Each dot represents one year, with the low latitudes average ( $30^{\circ}\text{ S} - 30^{\circ}\text{ N}$ ) shown in dark blue, high latitudes average shown in light blue and the global average shown in red. Anomalies are defined here as the deviation of annual mean SST (NPP) from the 1960–2006 mean of SST (NPP) in the respective regions.

**Reviewer Comment:** *In Figure 4, the labels in the panels say "small phyto trends", "diatom trends" but the caption says "changes in small phyto NPP" and "changes in diatom NPP". The labels in the panels are misleading because they suggest that the trends are in biomass not NPP. On page 5935 line 1, there is an extra "zooplankton biomass".*

Author Response: Thank you for pointing this out. Figure 4 shows changes in small phyto and diatom NPP, and changes in zooplankton biomass. We have corrected the labels.

**Reviewer Comment:** *In Figure 5, how were these changes computed? Are these trends (slope of linear regressions) or differences between annual or decadal means? The authors should be more explicit about what is shown in the figure in the caption.*

Author Response: In general, the map of trends in our manuscript show changes over the simulation period, calculated as slope of linear regressions multiplied with the number of years. However, we decided to change the Figure 5 and show the zonal mean of changes in limitation factors for both PFTs. We uploaded the new Figure with the caption "Figure 5: Changes in a)light and b)nutrient limitation of diatoms and c)light

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and d)nutrient limitation of small phytoplankton on a zonal average."

As the full caption does not fit into the input mask, we cite the full caption here:

**Figure 5** Changes in a)light and b)nutrient limitation of diatoms and c)light and d)nutrient limitation of small phytoplankton on a zonal average. The limitation factor is a unitless value between zero and one, with zero representing maximal limitaion and one representing unlimited growth. An increase in limitation factor leads therefore to increase in growth. For this plot, changes in limitation have been calculated for each cell as described in Section "Calculation of Trends", and the zonal mean of the resulting map of changes is shown.

**Reviewer Comment:** *On page 5936 line 9, there is a typo "...weak oh phytoplankton..."*.

Author Response: Thank you for pointing this out. Done.

**Reviewer Comment:** *On page 5936 lines 24-25, the authors state that small phytoplankton have higher light requirements than diatoms. Are they referring to the small phytoplankton's lower max Chl:C ratio?*

Author Response: The light requirements of small phytoplankton are higher also at equal Chl/C ratio. Light limitation depends (among others) on nutrient limitation  $N_x$ :

$$L_x = 1 - \exp\left(\frac{-\alpha \cdot \left(\frac{Chl}{C}\right)_x \cdot I_{par}}{\mu_{max} \cdot N_x \cdot T_f}\right),$$

Therefore a lower value for nutrient limitation (= this means strong nutrient limitation) leads to a higher value for light limitation (note that the term inside the brackets is

negative). As the nutrient requirements of small phytoplankton are lower than for diatoms, they are usually less nutrient limited and therefore more light limited.

**Reviewer Comment: The caption in Figure 6 has "a" and "b" labels but I don't see any "a" or "b" labels in the panels. Figure 7 does not have any units for the biomass shown in the "y" axis of the plots.**

Author Response: Done.

**Reviewer Comment: Figure 8 shows a distribution map of the different sources of POC. Is this an average for the period 1960-2006? How exactly was this computed?**

Author Response: We changed the discription of Fig. 8 to: "Map showing which particle production mechanism is most dominant over the study period. The particle production mechanisms in the BEC are as follows: aggregation by diatoms (blue), aggregation by small phytoplankton (yellow), zooplankton grazing of diatoms (green) and zooplankton grazing of small phytoplankton(red). We calculated the study period mean (1960-2006) of all fluxes to determine which mechanism contributed strongest to the sinking particle pool."

**Reviewer Comment: The word "through" is misspelled on page 5940 line 5.**

Author Response: Thank you for pointing this out. Done

**Reviewer Comment: On page 5940 lines 25-27, there is not much of a trend**

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***in the sources of POC, particularly in the Southern Ocean and North Atlantic. Perhaps adding the regression line would help see the trends. This also relates to my previous comment on inter-annual variability and the climate change signal.***

Author Response: We realized that the changes are difficult to see on the scale of the original figure. We replaced the figure and also the section in the manuscript referring to it. The text reads now as follows:

The composition of POC reflects the changes in PFT structure (Figure 9). Between the beginning (1960-1970) and the end (1996-2006) of the simulation period, the fraction of POC that can be attributed to small phytoplankton grazing changed from 45% to 38% in the North Atlantic, in favor of diatom aggregation. Note that total small phytoplankton aggregation and grazing shows little changes, but total POC production increased driven by higher diatom aggregation. In the Southern Ocean, the fraction of POC produced by diatom aggregation increased from 29% to 33% at the expense of the fraction produced by small phytoplankton grazing (-3%) and aggregation (-3%). This reflects increases in diatom biomass and decreases in small phytoplankton and zooplankton (Fig 8). In the tropical Pacific, the fraction produced by small phytoplankton grazing increased from 73% to 79% at the expense of diatom grazing (-4%) and small changes in aggregation. These changes are associated with decreases in all PFTs and decreases in diatom fraction.

We uploaded the new Figure with the caption "Figure 9: Changes in POC composition in % in the Southern Ocean, Tropical Pacific and North Atlantic between beginning (1960-1970) and end (1996-2006) of simulation period."

As the full caption does not fit into the input mask, we cite the full caption here:

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**Figure 9** Changes in POC composition in % in the Southern Ocean, Tropical Pacific and North Atlantic between beginning (1960-1970) and end (1996-2006) of simulation period. We obtain the changes by calculating the percentage of each mechanism's contribution to total POC and then computing the difference between the percentaged fraction of POC at the beginning and the end of the study period. The changes are significant within the 95% confidence interval.

**Reviewer Comment:** *On page 5942 line 15, the authors state that they also see a "global decline in chlorophyll". Is this surface chlorophyll or an average for the upper 100 m or mixed layer? The authors should be more specific.*

Author Response: This sentence refers to an average of the mixed layer, we have revised the sentence in the manuscript.

**Reviewer Comment:** *The studies by Henson et al 2010 and Yoder et al 2010 are particularly relevant to the statement made on page 5944 lines 22-25.*

Author Response: Thank you for the indication, we have included references to these studies.

**Reviewer Comment:** *In the different sections of the Discussion, the authors provide a very nice and thorough analysis of how changes in the physical environment impact the ecosystem dynamics and global NPP and export production and compare their results to other studies. However, questions remain regarding the significance of the observed trends given the model's temporal*

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***variability. In general, the authors should also be more explicit and include more information, including units and exact definitions, about what is being shown in each figure in the figures' captions. In many places not enough information is provided to properly evaluate the study's results and conclusions.***

Author Response: We thank the reviewer for the critical and very constructive review. In order to improve the manuscript, we have incorporated the individual comments into the paper as detailed above. We believe that these comments have lead to a greatly improved manuscript.

## References

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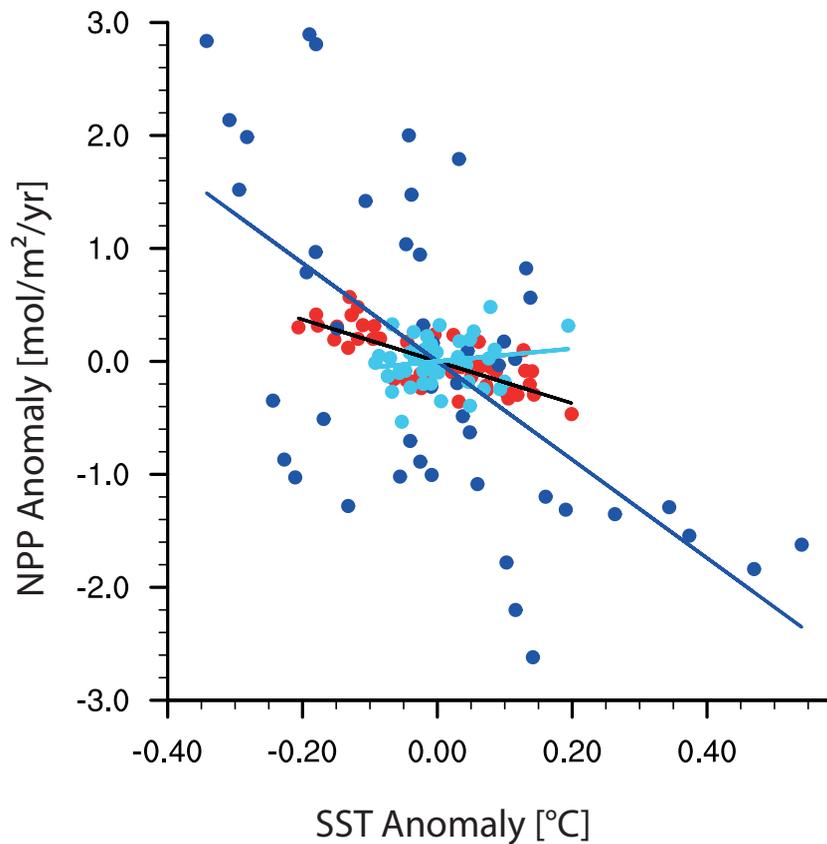
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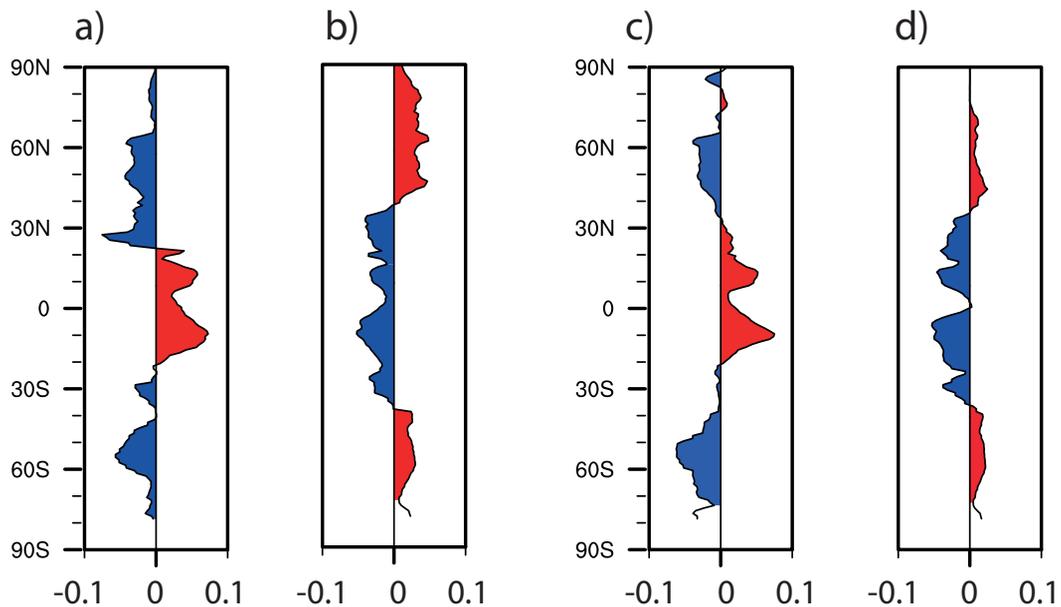
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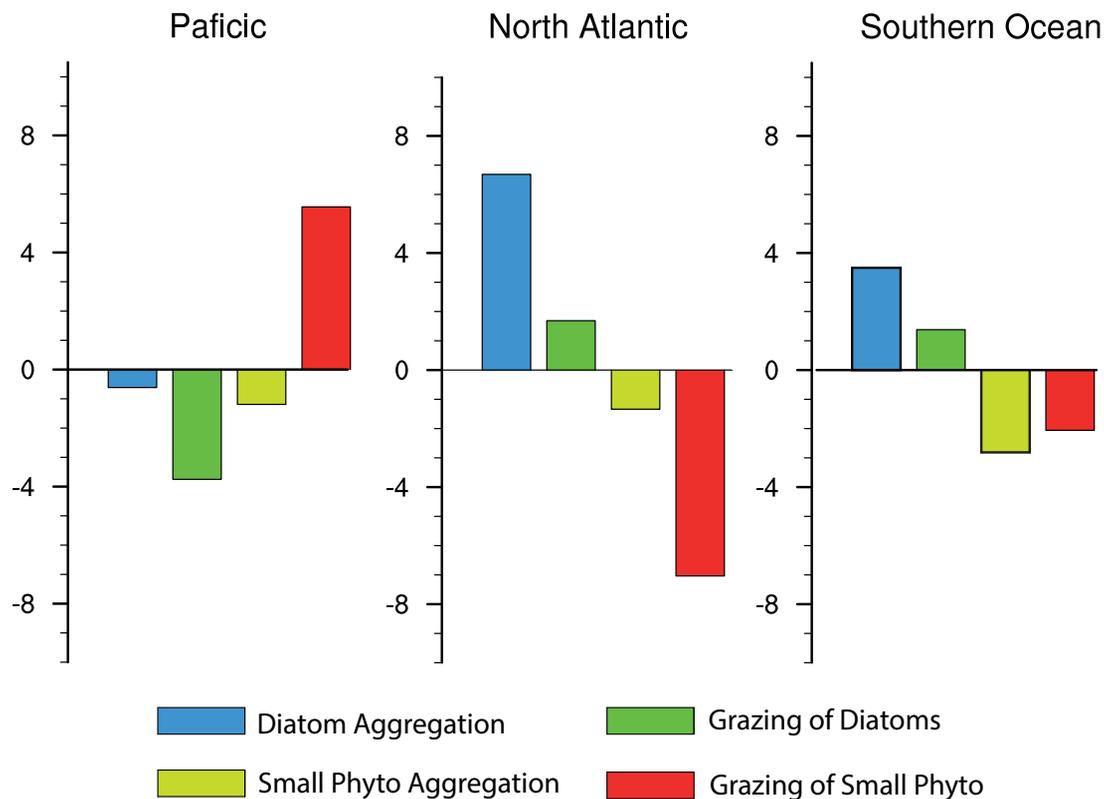


**Fig. 1.** Figure 3: Relationship between anomalies of annual mean depth-integrated NPP and anomalies of annual mean SST



**Fig. 2.** Figure 5: Changes in a)light and b)nutrient limitation of diatoms and c)light and d)nutrient limitation of small phytoplankton on a zonal average.

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**Fig. 3.** Figure 9: Changes in POC composition in \% in the Southern Ocean, Tropical Pacific and North Atlantic between beginning (1960-1970) and end (1996-2006) of simulation period.

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