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## ***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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### **Response to Reviewer Comment #2**

We wish to thank reviewer #2 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:

***Reviewer Comment: 1. The authors do not show convincingly that this paper presents novel results. Large parts of the paper (particularly sect. 4.2 and 4.3) is about reproducing results from previous studies without relevant new***

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**findings. The conclusion section is vague and largely an outlook on potential future studies. I recommend to rewrite the conclusions and strongly encourage the authors to highlight why this paper is novel and important (possibly already in the introduction).**

Author Response: We present the first analysis of changes in PFTs, NPP and EP in a model hindcast from 1960 onwards driven with prescribed atmospheric forcing. Previous efforts to estimate changes of the past decades have used fully-coupled models, forced with reconstructed CO<sub>2</sub> emissions. As a result, they produce their own internal variability, which does not correspond to the observed variability. Differences between our simulations and previously published results indicate that fully-coupled models might underestimate ongoing climate change. In addition, we have done a detailed analysis about the relationship between PFT structure, export efficiency and export production. We show a non-linear relationship between diatom fraction and export efficiency, contrary to previous findings. We compared our results in Sections 4.2 and 4.3 with several existing measurements. However, these measurements are either too short to estimate global trends (satellite data), are only available for localized areas and can therefore not be used to estimate global changes (timeseries at e.g. BATS, HOT) or base on very sparse data (results from Boyce et al. (2010)). We have restructured and shortened Sections 4.2 and 4.3 to put a stronger focus on our results. Furthermore we have rewritten the conclusions and parts of the introduction.

**Reviewer Comment: 2. One point I miss in this paper is a discussion of the analyzed trends with respect to internal variability. Natural climate variability such as ENSO cycles or the NPO can cause considerable variations in productivity and ecosystem structure (Masotti et al., Biogeosci., 2011; Patara et al., Ecol. Mod., 2012). In a paper that analyzes a hindcast simulation of less than 50 years I would have expected a discussion to what extent the results could be affected**

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***by natural variability, e.g. that trends could be indistinguishable from internal variability.***

Author Response:

We agree that the trends discussed in this paper 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 longterm oscillation in climate and rather reflect natural variability. We realized that our presentation of this topic is not very clear. We emphasize that we do not attribute the described changes to anthropogenic climate change. We adress the issue of natural variability in the introduction, discussion and caveats Section.

The introduction includes now the following paragraph:

"Our aim is to determine and analyze the trends over the 1960 through 2006 period, but we do not make any attempt to attribute these changes to anthropogenic forcing. The described trend over these nearly five decades could be due to anthropogenic climate change, but they equally could be part of a natural multi-decadal oscillation of the climate system. Regardless of whether the trends are due to natural or anthropogenic processes, they are indicative of how marine plankton responds to perturbations and hence help us to ultimately better understand and predict the future."

The discussion includes now the following paragraph:

"In the North Atlantic, future climate change predictions show robust increases in stratification and declines in primary production driven by increases in greenhouse gases (Capotondi et al., 2012; Steinacher et al., 2009). This contrasts with the reduced stratification and higher NPP we simulated in these regions over the past 5 decades, driven by stronger wind stress caused by by an overall positive trend in the North Atlantic Oscillation (Hurrell et al., 2001). In most climate simulations, the NAO does not continue

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to become even more positive, therefore permitting the stratification impact to emerge from the potential masking effect of the NAO trend over the last five decades.

For the next century, increases in NPP and EP are predicted for the polar Southern Ocean, but there is little agreement among models for the region between 40-60°S (Bopp et al., 2013). Over the past five decades, westerly winds increased in the Southern Ocean, most likely driven by ozone depletion above Antarctica and increases in greenhouse gases (e.g., Marshall (2003)). However, the NCEP reanalysis (which is the basis of the CORE-CIAF v2 forcing used in our simulation) is known to overestimate winds in the Southern Ocean (Marshall, 2003). Therefore wind-driven changes in our simulation might be overestimated in the Southern Ocean. In addition, the strong variability in the Southern Ocean makes it difficult to detect a climate change signal (Boyd et al., 2008). We conclude that caution is required when interpreting our trends in the Southern Ocean and possible consequences for future climate."

The caveats includes now the following paragraph:

"In this work we determine and discuss significant trends in ocean NPP and EP in the last 47 years, but without making an attempt to attribute the trends to anthropogenic climate change. Any trend detection is very sensitive to the level of variability, which is substantial, and potentially underestimated in our model relative to observations. Thus, our model maybe detecting significant trends too early (see also Henson et al. (2011)). Particularly in regions with weak trends but high variability (e.g. Southern Ocean) our trend estimates may be biased."

***Reviewer Comment: 3. Iron is an essential nutrient and an adequate representation in models is required to be able to simulate the observed variations in marine productivity (Schneider et al., Biogeosci., 2008; Misumi et al., Biogeosci. Discuss., 2013). This is not discussed explicitly in the present paper and no information about the assumed dust input is provided. How sensitive are the***

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## ***presented results to assumptions for aeolian iron fluxes?***

Author Response: We use a constant climatology to prescribe iron fluxes using data from Mahowald et al. (2009). We conclude that our trends cannot be driven by changes in iron forcing. We agree that an analysis of the sensitivity of NPP to aeolian iron fluxes would be very interesting. An extended analysis using different iron fields is unfortunately beyond the scope of this work. However, we revised the forcing Section to give more information on prescribed iron fluxes and the forcing in general. We cite the complete new forcing Section here, as this is also part of our answer to next comment:

"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.

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

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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: 4. The model description (sect. 2.1) is very detailed and includes an extensive set of equations in the appendix. Although I appreciate well documented methods, it is rather unusual for a model that already was described in previous papers. It is not clear if the model has been modified for this study or not. I suggest that the authors point out to the reader which parts of the model have been modified (or not) and focus on these modifications. For model components that are the same as described in Moore et al. (2002, 2004) and Doney et al. (2006) the text and appendix can probably condensed to describe only the most essential points with respect to this study and refer to the original publication for details.***

Author Response: We have shortened the model description 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: 5. In contrast to the model description the description of the model setup and forcings (sect. 2.2) as well as the trend analysis is rather short. I think this should be extended somewhat. Do you prescribe dust/iron fluxes to the ocean, and if so how? How is the input of freshwater and nutrients from river runoff modeled? How are trends calculated exactly (see also specific comment below)?***

Author Response: We extended both the forcing Section (see response above) and also the "Calculation of Trends" Section. The calculation of trends Section states now: 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 von Storch (1995). Typically, this reduced the degrees of freedom by 36%. We report primarily percent changes, which we obtained by 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: 6. In parts I find the paper somewhat long-winded. I think the text could be tightened somewhat by using a more precise language and shorter, clearer phrases.***

Author Response: Done.

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## Specific Comments Reviewer #2

**Reviewer Comment: P5927,L22: P5924,L24: I would phrase this a bit more carefully. Changes in wind patterns or salinity can outweigh the temperature effect. E.g. "Warmer surface waters generally increase ocean stratification and reduce..." You might also point out here that temperature is particularly important at low to mid latitudes while salinity effects are more prominent at high latitudes, and refer to Capotondi et al. (2012, JGR-Oceans)**

Author Response: Done. We have rephrased the sentence and cited the Capotondi paper in the discussion.

**Reviewer Comment: P5925,L1: This sentence is awkward and I had troubles to understand it. I suggest to reformulate it. For example: "In addition, seawater has become more acidic due to the uptake of anthropogenic CO<sub>2</sub> by the oceans. Compared to preindustrial times, ocean pH at present..."**

Author Response: Done

**Reviewer Comment: P5925,L20: I suggest to remove "still". The use of this word implies an earlier expectation that this should have changed until now or that is about to change soon, which is not a qualified statement.**

Author Response: Done

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**Reviewer Comment: P5927,L22: Are the C/N/P ratios fixed to the well-known Redfield ratios? If this is the case, the authors might want to refer to it here.**

Author Response: N/C is fixed to 0.137 for all phytoplankton and zooplankton, and P/C is fixed to 0.00304 for diazotrophs and 0.00855 for all other plankton. As this is described in Moore et al. (2004), we removed the sentence to shorten the section on model description as suggested earlier.

**Reviewer Comment: P5929,L12: I suggest to write "The ocean component of CCSM3 was forced with..." in order to make clear that ocean-only model simulations are performed and not fully coupled simulations with CCSM3.**

Author Response: Done

**Reviewer Comment: P5930,L6: The statement about parameters that have been modified should go to the model description in section 2.1. It should be explained why these parameters have been changed when discussing the differences to the original BEC model described in Moore et al./Doney et al. (see also general comment above).**

Author Response: The model description Section includes now the following paragraph:

"Some parameters of the BEC were modified relative to those used by Doney et al., 2009, with a list with the differences given 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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Table 1: Modified parameters of the ecosystem model BEC. Bold numbers in brackets denote values that have been used in the version of Doney et al., 2009.

Parameter	Value	Units	Definition
$\alpha$	0.3 <b>(0.25)</b>	$\frac{mmolCm^2}{(mgChlWd)^{-1}}$	initial slope of P - I curve
$K^{NH_4}_{diat}$	0.08 <b>(0.1)</b>	mmol N m <sup>-3</sup>	diatom NH <sub>4</sub> half saturation coefficient
$e^{POC}_{sp}$	0.18 <b>(0.22)</b>	(mmol C) <sup>-1</sup>	small phyto. grazing factor
$u^{max}_{diat}$	2.07 <b>(2.0)</b>	d <sup>-1</sup>	max. zoo. growth rate on diatoms at T <sub>ref</sub>
$f^{small,POC}_{graz}$	0.22 <b>(0.24)</b>		upper limit on fraction of grazing on small phyto. routed to POC

**Reviewer Comment: P5931,L5: Should this read "48 PgC/yr" instead of "4.8 Pg/yr"? Or are the units wrong?**

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

**Reviewer Comment: P5931,L20: Please specify the temporal resolution of your data for calculating the linear regression. Do you calculate the regression from monthly data for each month of the year or for annual mean values?**

Author Response: Done. The trends are calculated for annual mean values.

**Reviewer Comment: P5931,L24: Are the first ten years 1950-1960 or 1960-1970? Please clarify**



Author Response: Done. The first ten years here are 1960-1970.

***Reviewer Comment: P5932,L7: Looking at Fig. 2d it seems that EP decreases to 94***

Author Response: The decrease in export is -8% over the 47 years and was calculated from the slope of the regression line. Another option would have been to calculate the mean of the last 10 years minus the mean of the first 10 years to describe the change. We have chosen to use the slope of the regression line to calculate the changes as this is more robust. We have attached regression lines to the timeseries in Figure 2 to make this more clear.

***Reviewer Comment: P5932,L10: "Export production increases in ... and in the Polar Southern Ocean > 60S.": Is this true? In Fig. 2d I see no significant trend in the Southern Ocean until about year 2000 and then a decrease of 10***

Author Response: We have changed this section in order to increase clarity. The part reads now as follows:

"Export production increases in the North Atlantic ( $+0.6 \text{ mol POC m}^{-2} \text{ yr}^{-1}$  resp.  $+30\%$ ) and in parts of the Southern Ocean. . However, the Southern Ocean shows areas with increases and decreases that add up to no significant overall trend in export."

***Reviewer Comment: P5932,L12: "All other areas display changes of less than 5 %***

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Author Response: This comment is unfortunately incomplete. However, we have revised this part of the results and the sentence has been removed.

**Reviewer Comment: P5932,L20: *It should be mentioned here that the trends in NPP are weaker only in relative terms, in absolute values, the changes in NPP are larger in many regions.***

Author Response: Done. The sentence reads now: "In relative terms changes in NPP are between 2 - 20 % weaker than changes in EP."

**Reviewer Comment:P5932,L21: *"The amplification of trends between NPP and export...": This sentence is somewhat awkward. I suggest something like "The trends in EP are amplified due to changes in export efficiency."***

Author Response: Done.

**Reviewer Comment: P5932,L23-26: *The line of argument provided here is not very convincing because the export efficiency is defined as EP/NPP and thus correlated with EP and NPP by definition. Further, in some regions the export efficiency decreases (Fig. 1f) which indicates that there the trend in EP is weaker than in NPP***

Author Response: We used the correlation here only to avoid describing a very similar pattern several times. We have changed the description to avoid confusion. The sentence states now: "The changes in export efficiency show a pattern similar to the

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changes in export production ( $S_c = 0.88$ ) and also NPP. "

**Reviewer Comment: P5933,L1: The reference to Fig. 3 is ambiguous here. Does Fig. 3 show the correlation for the global mean (which is not discussed in the text) or for the low latitudes? If the latter is the case, why don't you also show the high latitudes in a different color? Further, the reference should then go after "low latitudes" to make it clear.**

Author Response: We have changed the figure and added points for high and low latitudes in a different color. The text referring to the figure now reads 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.

The full caption of Figure 3 does not fit into the input mask, therefore we cite it here:

**Figure 3** Relationship between anomalies of annual mean depth-integrated NPP [ $\text{mol C/ yr}^{-1}$ ] 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 study period mean of SST (NPP) in the respective regions.

**Reviewer Comment: P5934,L16: I think temperature should also be mentioned as a controlling factor here.**

Author Response: Done.

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**Reviewer Comment: P5936,L2: From Fig. 5 it is not clear that there are substantial changes in both light and nutrient limitations as only the combined effect is shown. I suggest to add four additional panels where the individual effect of light and nutrient limitation are shown.**

Author Response: We agree that light and nutrient limitation should be shown separate. We have replaced the figure and show zonal means of light and nutrient limitation for small phytoplankton and diatoms instead.

The full caption of Figure 5 does not fit into the input mask, therefore we cite it 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 limitation 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: P5936,L13: Doney et al. (2007) is the wrong reference here as this paper only deals with ocean physics and not with NPP. Also, if the findings are confirmed by several authors as you write, I suggest to pick a study that was performed with a different model brand than NCAR.**

Author Response: Thank you for pointing this out. The sentence reads now:  
"The trend towards increased stratification in the low latitudes and related decrease in NPP in our simulation proceeds in several climate model studies that simulate

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anthropogenic climate change in the next 100 years (Steinacher et al., 2009; Bopp et al., 2001; Boyd and Doney, 2002; Le Quéré et al., 2003)."

***Reviewer Comment: P5937,L4: I don't see why warming of the surface ocean can directly lead to nutrient stress. Warming can lead to increased stratification but as it is stated here it implies an additional direct effect of warming on nutrient availability. Please clarify.***

Author Response: Done. The sentence now reads: "The increase in stratification and lower wind stress lead to higher nutrient stress for both phytoplankton PFTs and reduce the growth rate of both of them."

***Reviewer Comment: P5942,L16: This sentence is awkward. I suggest to just write "..., we use chlorophyll measurements only."***

Author Response: Done.

***Reviewer Comment: P5945,L20: The models in Steinacher et al. (2009) are not forced with CO2 concentrations but with anthropogenic CO2 emissions for both the historical period and the future scenario. Please clarify.***

Author Response: Done.

***Reviewer Comment: P5947,L5: In addition to the ecosystem representation in models the temperature dependency of phytoplankton growth plays also a role. Schmittner et al. (2008) find an increase in global NPP due to an intensified***

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***microbial loop although the export is decreasing. The different effects of temperature on NPP and EP should be discussed here.***

Author Response: Done.

***Reviewer Comment: P5948,L9: I think this should read "decreased light limitation" or "increased light availability".***

Author Response: The "increase in light limitation" refers to the high latitudes. We have changed the sentence to make it more clear. It reads now: "The trends in our simulation are mostly driven by bottom-up controls, with decreased nutrient concentrations being the main driver in the low latitudes and decreased light availability the main driver in the higher latitudes."

***Reviewer Comment: P5948,L20: "...climate change as been weaker...": Weaker than what? Please clarify.***

Author Response: Done. The sentence reads now: "It is assumed that climate change will accelerate in the upcoming century."

***Reviewer Comment: P5949,L4: I cannot distinguish model parameters that should be bold from others.***

Author Response: Done. Model parameters are now in lowercase letters.

***Reviewer Comment: Table 1: Description is missing for the last parameter (QSi***

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*diat,coeff).*

Author Response: Thank you for pointing this out. Done

***Reviewer Comment: Fig. 2: Panel labels are wrong (a) should be (b) etc. Description of panel (a) is missing.***

Author Response: Done.

***Reviewer Comment:Fig. 3: This caption is too brief. Are this global mean values or for a specific region? Further, the plot shows NPP as a function of absolute SST rather than changes in SST***

Author Response: We have changed Figure 3 and the caption, please see response above.

***Reviewer Comment: Fig. 5: Please indicate whether negative values indicate less limitation (more production) or less production (more limitation) to make the figure easier to read. Also, a period is missing at end of the caption.***

Author Response: Done, please see response above.

***Reviewer Comment: Fig. 6: The axis labels are confusing here. Please move them to the left (y-axis) and below (x-axis) the tick labels. Further, superscripts should be used for m2 and m3 and the labels (a) and (b) for the panels are missing in the figure. Please also add units to the growth rate color bars.***

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Author Response: Done.

***Reviewer Comment: Fig. 7: I don't fully understand this figure. Please explain what the values on the y-axis mean.***

Author Response: We have extended the caption of Figure 7 in the manuscript. The caption states now: "Biomass structure of phytoplankton types and zooplankton at different regions. Each panel shows the biomass structure for the first ten years average (1960-70) and for the last ten years average (1996-2006). The ratio between total phytoplankton and zooplankton is influenced by temperature, with higher temperatures leading to higher zooplankton quota. Moreover diatoms can sustain a lower zooplankton fraction as the zooplankton growth rate is lower when feeding on diatoms."

Minor editorial comments Reviewer #2

Thank you very much for these detailed comments. We corrected all sentences in our manuscript.

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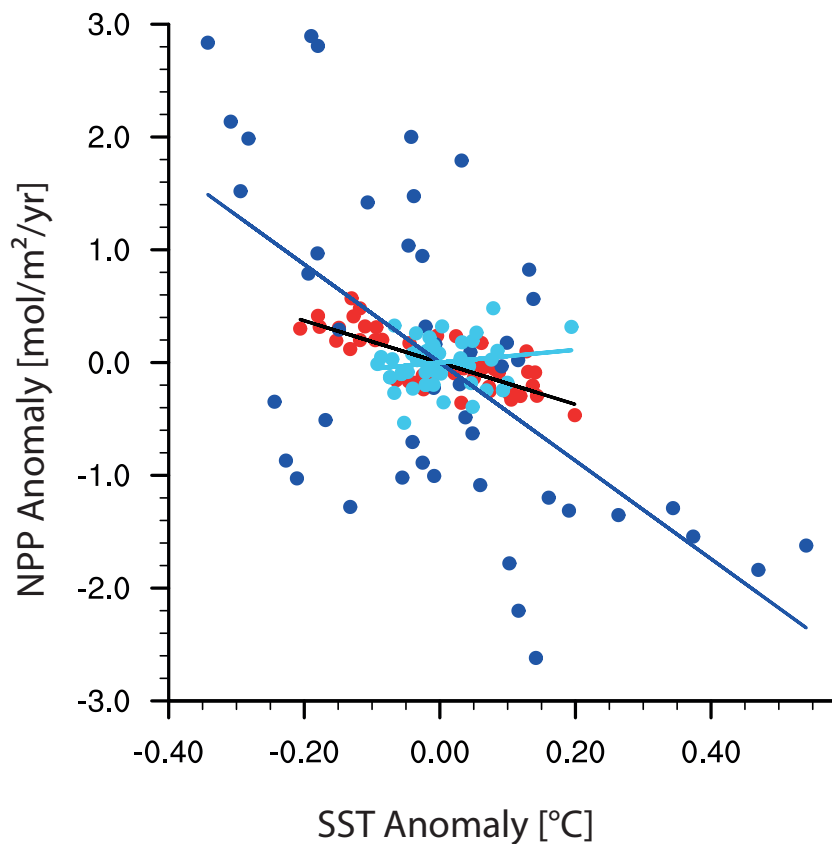
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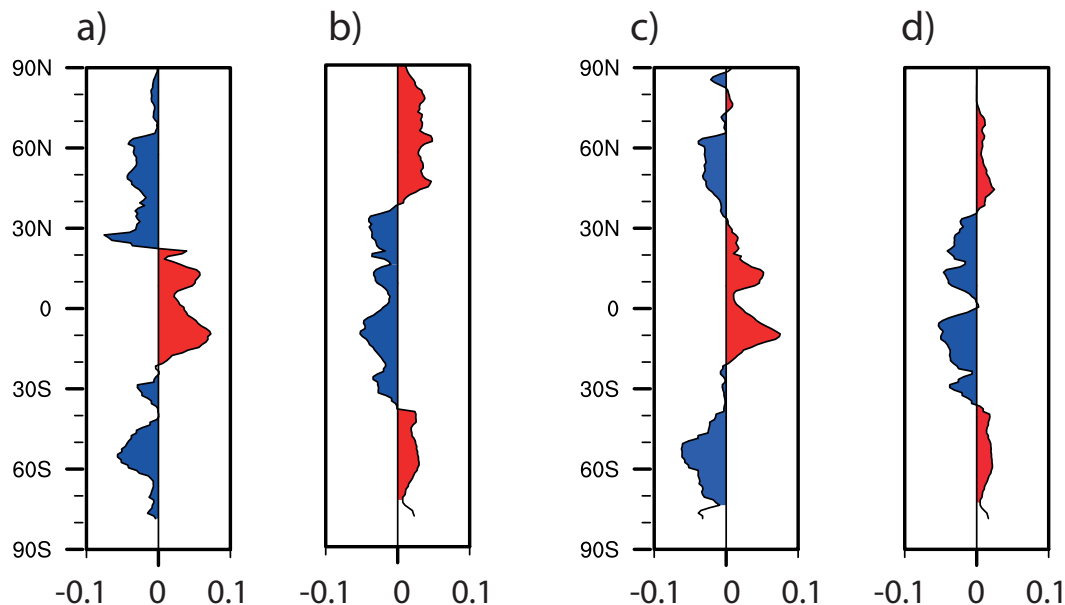
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**Fig. 1.** Figure 3: Relationship between anomalies of annual mean depth-integrated NPP and anomalies of annual mean SST



**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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