

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

Anonymous Referee #2

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The manuscript "Long-term trends in ocean plankton production and particle export between 1960-2006" by Laufkötter et al. is a relevant contribution to the understanding and modeling changes in marine productivity. The paper has, however, some weaknesses, errors, and inconsistencies that must be addressed (see comments below). I recommend the publication of this paper in Biogeosciences after these issues have been addressed satisfactorily.

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1 General comments

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 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).

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

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 presented results to assumptions for aeolian iron fluxes?

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 be condensed

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to describe only the most essential points with respect to this study and refer to the original publication for details.

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)?

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.

2 Specific comments:

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)

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 CO₂ by the oceans. Compared to preindustrial times, ocean pH at present...".

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.

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.

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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.

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).

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

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?

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

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

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

P5932,L12: "All other areas display changes of less than 5

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.

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."

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

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indicates that there the trend in EP is weaker than in NPP.

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.

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

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.

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.

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.

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

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.

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

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

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

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

Table 1: Description is missing for the last parameter ($Q_{\text{diat,coeff}}^{\text{Si}}$).

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

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 as written in the caption.

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.

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.

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

3 Minor editorial comments:

P5924,L25: This should probably read "...and reduce[s] the mixing...".

P5928,L11: Missing space between "using" and "a quadratic".

P5930,L13: I think this should read "...larger than 0.95, but [only] 0.56 for chlorophyll...".

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P5932,L8: I think the figure reference is wrong, it should be 2d instead of 2c.

P5932,L22: The figure reference should read "Fig. 1e,f".

P5933,L3: Remove parenthesis after "increases".

P5936,L9: Something went wrong here. Should it read "...have only a weak effect on phytoplankton growth compared to..."?

P5937,L9: I think this should read "...towards a lower diatom fraction."

P5940,L5: "through" instead of "throguh".

P5942,L5: Figure reference should be "2e" instead of "2d".

P5954,L7: '3' should be written as subscript in the section heading.

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