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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.

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Response to Reviewer Comment #3

We wish to thank reviewer #3 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: I found the analysis to be useful and interesting. I suspect, however, that a significant part of the contrast between the Bopp et al. (2005) results and those herein is due to the fact that the 1960-2007 trends in the North

Atlantic and Southern Ocean are due to low frequency climate variability and not change. In the North Atlantic, century-scale climate change predictions show robust increases in stratification and declines in primary production as the effects of continued increases in greenhouse gases take hold (e.g., Capotondi et al., 2012; Steinacher et al., 2010). This contrasts sharply with the reduced stratification in these regions over the contemporary period. This doesn't nullify the statement concerning export efficiencies being sensitive to biology (see also, for example, Tauscher and Oschlies, 2011), but the results herein do not imply similar divergence in mechanisms under climate change. This needs to be discussed in the "implications for future change" and "caveats" sections.

Author Response: We agree that our trends in the North Atlantic might be the result of climate variability (e.g. increase in NAO) and might not continue in the future. We have discussed this issue in the "Implicatios for future change" and also in the "caveats" section.

The "Implications for future change" section 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 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: More generally, the potential role of climate variability in generating the trends analyzed needs to be discussed more prominently. It seemed implicit that any trend between 1950-2007was "climate change", yet this is not the case - particularly at regional scales (e.g., see Deser et al., 2012). This should be clearly stated in both the introduction and in the caveats.

Author Response: We have included a discussion of the possible effects of climate variability on our trends in the introduction, the "implications for future change" and the "caveats" sections. The respective paragraph in the introduction states: "Our aim is to determine and analyze the trends over the 1960 through 2006 period, but we do not

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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 respective paragraph in the "Implications for future change" section now states:
"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 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.

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Reviewer Comment: There was an extended discussion of the roles of top-down versus bottom-up perturbations in generating trends over the 50 year hindcast, leading to the conclusion that the trends primarily reflect a bottom-up influence. While this discussion was carefully caveated by pointing out that the run considers only one minor element of the potential top-down forcing, one has to wonder whether the experimental design warrants an extended discussion of this issue. The external forcing in the experiment is essentially bottom-up, assessing the relative roles of top-down versus bottom-up would seemed to require a more comprehensive treatment of top-down forcing (changes in fishing/fish populations?). I have no issues with including some carefully caveated discussion of this issue, but I'm not sure it should be a central thread of the paper given the experimental design. Of course, as the authors point out, the ultimate response of phytoplankton to a bottom-up perturbations depends dynamics adjustments of all relevant growth and loss processes.

Author Response: We fully agree that our findings on bottom-up vs top-down control have to be carefully caveated and are therefore not a central result of our work. We have shortened the respective part of the discussion and also the conclusions to change the focus towards our main results. The conclusion states now:

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We present the first analysis of changes in PFTs, NPP and EP in a model hindcast from 1960 onwards driven with prescribed atmospheric forcing, i.e., winds, and fluxes of heat and freshwater. Our results suggest a significant global decline in ocean chlorophyll over the last five decades of 3%. Our downward trend supports the conclusions drawn by Boyce et al. (2010), but our simulation suggest a tenfold smaller trend. We could not resolve this large discrepancy, but tend to question the robustness of the observational trends. Our simulated changes in chlorophyll go along with global decreases in NPP and export of 6.5% and 8%. Our changes over the past 5 decades are more pronounced than those on the basis of simulations with fully-coupled atmosphere-ocean models, forced with reconstructed CO_2 emissions. A possible reason could be that fully-coupled models produce their own internal variability which does not correspond to the observed variability.

We identified increased stratification in the low latitudes and increased wind stress and increased mixing in the North Atlantic and Southern Ocean as the main drivers for changes in PFTs and NPP. 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 one in the higher latitudes. However, the representation of top-down control is restricted to temperature effects on grazing of one generic zooplankton type in our model. Adding a representation of higher trophic levels and their sensitivity to physical changes might emphasize the role of top-down control.

Finally, our analysis reveals that a parameterization of PFTs including diatoms, coccolithophores and small phytoplankton of variable size impacts the dynamics of particle production in a non-linear way. This is in contrast to previous studies which show a linear relationship between diatom fraction and export efficiency (Bopp et al., 2005). The link between PFT distribution and export efficiency is currently not well understood. Further model studies should focus on determining the full range of possible responses of export efficiency to changes in PFT composition. Furthermore, an improvement of the representation of PFT distribution and their contribution to the particle flux is necessary. For this we need both better estimates of PFT distribution and measurements of the contribution of different PFTs to the particle flux. New datasets of PFT biomass (Buitenhuis et al., 2013) and pigment concentrations and more accurate satellite-based PFT estimates (Alvain et al., 2008; Hirata et al., 2011) provide a promising step towards improved model evaluation. It is assumed that climate change will accelerate in the upcoming century. Stronger perturbations of the marine ecosystems are likely to affect PFT distribution, primary and export production to a wider extent than shown in this work. Including improved representations of PFT distribution and behaviour in simulations of future climate change might reveal an important feedback of the biological pump to climate change.

Reviewer Comment: My last substantial comment pertains to the comparison of chlorophyll trends relative to those analyzed in Boyce et al. (2010). I was hoping for a Figure comparing the simulated trends here with those inferred by Boyce et al. (2010). Also, while trends in some areas may be consistent with those of Boyce et al. (2010), the magnitude of the trend here is considerably smaller. Boyce et al.'s 1%/year value translates to an alarming decline in chlorophyll of âĹij40% over 50 years and >60% over the last century. Through a more substantive comparison with the Boyce data and contrasting these trends, this paper could make an important contribution to the controversy that Boyce's alarming analysis has stirred.

Author Response: We agree that it would be desirable to have a more substantial comparison with Boyce et al. (2010). However, a more detailed comparison of the trends in chlorophyll is beyond the scope of this work, since we focus rather on the mechanisms influencing the changes in biomass, in community structure and in the fate of organic carbon on regional and global scales. However, we have stronger contrasted our trends to the ones reported in Boyce et al. (2010) and clearly stated that our trends

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are considerably smaller. The respective part of the manuscript now states:

"On the global scale, we compare our data only to that part of the Boyce et al. (2010) record that is based on direct chlorophyll measurements, omitting that part that is based on transparency observations. This avoids most of the controversial aspects of the Boyce et al. (2010) study (Rykaczewski and Dunne, 2011; Mackas, 2011). Even with this selection, their global decline in chlorophyll is nearly an order of magnitude larger than what we infer from our model. On the regional scale, our trends follow the direction (not the magnitude) of in situ chlorophyll trends described in Boyce et al. (2010) in the Equatorial Atlantic and Pacific, Northern Indian Ocean and parts of the Southern Ocean, i.e., in about half of the regions. Our simulated trends differ in several other regions. In particular, in the North Atlantic our simulation shows a strong increase in chlorophyll. Furthermore, Boyce et al. (2010) report increases in chlorophyll in the North Pacific, South Indian and South Pacific, where our simulation shows decreases (North and South Pacific) or no clear trend (South Indian).

However, one needs to take into consideration that the number of measurements on which Boyce et al. (2010) based their trends are sparse in general and extremely low in the southern hemisphere, where our trends differ most from their results. At the moment it is not possible to distinguish whether the model is seriously underestimating the trends, particularly in the southern hemisphere, or if the observational record is not reliable enough to derive robust trends."

Minor comments Reviewer #3

Thank you very much for these detailed comments, we have included them in the revised version of the manuscript. We like to thank the reviewer in general for the critical and very constructive review. The comments provided food for thought and have in our opinion lead to a greatly improved manuscript.

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Interactive Comment

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