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Biogeosciences
Discussions

Interactive comment on "The impact of global warming on seasonality of ocean primary production" *by* S. Henson et al.

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Response to Reviews

We would like to thank both reviewers for their very positive comments.

Reviewer 1

In response to reviewer 1's general comments, we accept that 'phenology' may have been used in the incorrect context in some parts of the manuscript. We've carefully gone through the manuscript and replaced 'phenology' with 'seasonality' where appropriate. We have added a line to the abstract to emphasise our finding that monthly resolution is inadequate for phenological studies.

Specific comments

1. We have replaced 'phenology' with 'seasonality'.

2. Done.

3. We prefer to be specific in the abstract and so retain mention of the 2 time scales.

4. We have altered the last lines of the abstract to now read: "Monthly resolution model output is found to be inadequate for resolving phenological changes. We conclude that analysis of phytoplankton seasonality is not necessarily a shortcut to detecting climate change impacts on ocean productivity."

5. We have added these references.

6. Done.

7. We have replaced 'phenology' with 'seasonality'.

8. We have replaced 'phenology' with 'seasonality'.

9. We have replaced 'phenological markers' with 'seasonality metrics'.

10. Done.

11. Done.

12. In the 1st sentence, we have replaced 'phenological markers' with 'seasonality metrics'. The 2nd sentence has been rephrased to "Previous work suggested that in order to distinguish a climate change trend from natural variability, a continuous PP record of $\sim$ 30-40 years in length was required (Henson et al., 2010)".

13. We have replaced 'phenology' with 'seasonality' in this sentence. We feel this is a valid sentence as we have demonstrated that monthly output is not sufficient to detect a trend in phenological metrics (i.e. it's not a shortcut). Furthermore, we do not specify that we analysed trends in phenology, we state that "analysis of phytoplankton seasonality is not necessarily…" – analysis does not indicate only calculation of trends.

Reviewer 2

In response to reviewer 2's general comments, we have added an additional sentence to the abstract that explicitly states that monthly output is too coarse to examine phenological changes. We did not intend to give the message 'that studying phenology is by no means an indicator capable of capturing change'. Rather, we wished to emphasise that the current norm of saving model output at monthly resolution is not sufficient to resolve trends. The last lines of the abstract now read: "Monthly resolution model output is found to be inadequate for resolving phenological changes. We conclude that analysis of phytoplankton seasonality is not necessarily a shortcut to detecting climate change impacts on ocean productivity." We discuss the requirements for higher resolution model output on page 14 of the revised manuscript.

Specific comments

1. The current RCP scenarios differ from preceding SRES scenarios largely in terms of underlying policy considerations and the practical aspects of scenario creation. For instance, SRES scenarios do not imply mitigation policies, while RCP scenarios factor this in. Also, the relationships between Integrated Assessment Models (IAMs), Climate Models (CMs) and Impacts, Adaptation and Vulnerability (IAV) analysis was more rigid in the SRES era, and is more interactive in the RCP era. To quote Moss et al. (2012), "To date, such scenarios have not adequately examined crucial possibilities, such as climate change mitigation and adaptation, and have relied on research processes that slowed the exchange of information among physical, biological and social scientists."

However, in pragmatic terms, the scenarios from different eras do not differ markedly during the 21st century. Henson et al. (2010) utilised simulations that were mostly using SRES scenario A2, a climate change projection whose warming is slightly less extreme than that of RCP8.5. Per Rogelj et al. (2012), the temperature impact of RCP8.5 (mean 4.6 °C, range 3.8-5.7 °C) is slightly greater than that of SRES scenario A2 (mean 3.9 °C, range 2.5-5.9 °C), and more closely resembles that of SRES scenario

A1F1 (mean 4.5 °C, range 2.9-6.9 °C), although it is slightly less extreme than this latter scenario. We have now included this last sentence on page 11 of the revised manuscript.

2 & 3. The VGPM does not use output from a general circulation model. The inputs to the algorithm are satellite-derived chlorophyll, sea surface temperature and PAR. We take the reviewer's point though that the VGPM itself has errors associated with it. We have changed the wording in section 3.1 to refer to 'satellite derived PP' rather than observations and have included mention of the round robin results on page 6 of the revised manuscript: "Although the satellite-derived PP fields themselves have errors potentially as large as biogeochemical models (Friedrichs et al., 2009; Saba et al., 2010) they provide an indication of the large-scale features in PP seasonality."

4. Figure S2 has been re-plotted with a discrete colour bar.

5. We agree with the reviewer that Figure 2 was difficult to read. We have now split the figure into 3 separate figures (new figures 2-4) and have also introduced a discrete colour bar to improve clarity. The statistical significance of the trend is explicitly included in the figures, as stated in the original figure caption, "White areas in a) and b) indicate where the trend is not significantly different from zero and in c) where a 1-way ANOVA analysis showed no significant difference in the means of the 2 periods (significance at 5% level in both cases)." Hopefully the new discrete colour bar will make distinguishing between areas of no significance and those with weak trends easier.

6. In our discussion of the model predictions, we limited ourselves to those features which were displayed consistently by the majority of models. In the case of the increase in the equatorial Pacific highlighted by the reviewer, only one model shows a substantial increase on the flanks of the upwelling region (CCMA), with another showing a possible weak increase on one side of the upwelling region (IPSL).

7. We have expanded this section, which now reads: "Unlike the situation with the macronutrient nitrate (Fig. 4e) there are trends towards higher surface dissolved iron

concentrations across all biomes (Fig. 4g). These are typically stronger in regions where iron is supplied by aeolian dust, and lower (to near-zero) in regions, such as the Southern Ocean and Equatorial Pacific, where aeolian dust supply is limited. Note that none of the models include time-variant atmospheric dust deposition, so any increase in surface iron is not a consequence of increased desertification. In the case of dust-affected regions, since the seasonal amplitude of vertical mixing broadly decreases in response to climate change, macronutrient supply from deep, nutrient-rich waters also declines. As a result, production declines in these regions, and the iron deposited via dust is unable to be utilised by phytoplankton and so accumulates. As such, what might appear a potential source of enhanced growth - extra iron - cannot support enhanced PP. By contrast, in iron-limited HNLC regions such as the Southern Ocean, the supply of nitrate and iron is largely from below, so this decoupling of surface nutrient availability is absent. These results emphasise the importance of the processes by which nutrients, macro- and micro-, are supplied to the surface ocean."

8. Differences between the previous results (Henson et al., 2010) and those presented here are small (38 years vs 32 years), and are not only due to different climate change scenarios. In many cases, the physical and/or biogeochemical formulation of the models has changed, not just the forcing. In addition, the IPCC AR4 simulations used a CO2 emissions scenario, whereas the AR5 simulations use a representative concentration pathway (see response to point 1 above). There are too many possible factors that changed between the 2 sets of simulations to ascribe any differences (if, that is, they are even significant) to a change in the climate scenario.

9. This should read just 'nutrient', rather than 'macronutrients', and has been changed in the revised manuscript.

10. We have added into the discussion reference to previous work that examined future changes in export production. No IPCC-class model is sufficiently sophisticated to simulate changes in particulate sinking rates, to the best of our knowledge. This section now includes: "Earlier studies focusing on climate change effects on export

production confirm this hypothesis, with a projected increase in the abundance of small phytoplankton at the expense of diatoms, and a subsequent decrease in global carbon export (Bopp et al., 2005; Steinacher et al., 2010)."

11. By 'natural variability' we mean any temporal variability that is not associated with global warming. We've attempted to clarify this by adding, on page 14 of the revised manuscript, "An additional confounding factor in detecting trends in phenology is strong natural variability, whether on interannual timescales driven by local changes in forcing, or on multi-year timescales driven by basin-wide modes of variability, such as the North Atlantic Oscillation or Pacific Decadal Oscillation."

Interactive comment on Biogeosciences Discuss., 10, 1421, 2013.