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Biogeosciences Discussions

Interactive comment on “Is global warming already changing ocean productivity?” *by* S. A. Henson et al.

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Replies to comments follow the numbering in the review:

1. In response to both reviewer’s comments, we have changed the title to, “Detection of anthropogenic climate change in satellite records of ocean chlorophyll and productivity”.

2 & 3. We had intended the phrase ‘natural variability’ to indicate all interannual, decadal and longer-term variability in productivity driven by physical forcing (e.g. changes in stratification, El Nino events, change of phase in the NAO etc.), as opposed to global warming- or climate change-related trends. We have added a definition on page 10315 of the difference between natural climate variability and global

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warming-driven trends, which should hopefully make clear the distinction we were trying to make. . .. “A note on terminology is warranted here to avoid confusion. Throughout the paper, we use the phrase ‘natural variability’ to refer to interannual, decadal or multi-decadal variability in PP or chl driven by oscillatory or transient physical forcing (e.g. El Niño events, shifts in the phase of the North Atlantic Oscillation etc.). This is contrasted with ‘trends’, which we use to indicate long-term (multi-decadal or longer) changes in PP or chl driven by persistent anomalous forcing (i.e. global warming).” We also recognise that the use of the word ‘real’ in the abstract might be confusing without the associated context, so we have therefore replaced it with ‘global warming-driven trends’. There have also been numerous small changes (and some more extensive ones, see responses 4, 16, 21) in the manuscript which hopefully emphasise the difference between climate variability and trends.

4. We hadn’t meant to imply that all of the cited papers had stated definitively that the observed trends were due to global warming. Obviously, the use of the word ‘several’ here was an over-statement. In most cases, the papers mentioned in the reviewer’s notes specifically comment on how the satellite ocean colour record is dominated by natural variability, driven by climate-scale processes, as represented by indices such as MEI, strength of stratification etc. (Martinez et al., 2009 is a good example of this). Our results, which demonstrate how decadal variability confounds the detection of global warming trends, are consistent with the earlier papers in this regard. This section has been altered to include reference to Martinez et al. (2009), Behrenfeld and Siegel (2007), Behrenfeld et al. (2008 and 2009). We’ve also attempted to clarify that (with the exception of Polovina et al. 2008) the cited studies found the SeaWiFS record to be correlated to various climate indices.

5. This sentence was meant to reflect the canonical or traditional view of how global warming will affect high latitudes, rather than a definitive statement on what the models or data show. We’ve altered this sentence to reflect this, and added an additional sentence, “In contrast, recent analyses by Behrenfeld et al. (2008 and 2009) suggest

that increasing SST corresponds to reduced PP in sub-polar regions (although the response is weaker than for the sub-tropics)."

6. Quite right – we've changed this sentence to 'SeaWiFS has been operating since. . .'

7. Changed.

8. We've added a sentence here to include a reference to Behrenfeld et al. (2008).

9. The reviewer is right that applying a linear fit to a short dataset, particularly one with a strong anomaly at the beginning, is not the best way to represent the trend in the time series - and this in fact is one of our key conclusions. We followed this method because it is how many of previous studies examining trends in SeaWiFS data have deduced those trends (e.g. in McClain et al., 2004; Gregg et al., 2005) and one of our aims here was to set those trends in a longer-term context (as we've attempted to do in Figures 2 and 3). Our aim in this manuscript was not to provide an analysis of the response in the SeaWiFS dataset to natural variability, i.e. the 97/98 ENSO and subsequent re-bound, in which case the regression method and time period over which it was applied would be very important, but rather to demonstrate that recent 10-year trends are not unusual in a longer-term context. Furthermore, as all of the models also simulate an El Nino event in 1997/1998, the most recent trends in the models are directly comparable to the SeaWiFS trends. The actual magnitude of the trend is less relevant to our aims than how the trend over the SeaWiFS record compares to previous 10-year time series. Early on in this project, we also performed the same analysis using the time series from January 1998 onwards with little difference in the results. There are of course more sophisticated approaches than a linear fit (the possibility of attribution and detection of trends using different methods is discussed on page 10326). One fairly simple approach might be to account specifically for the correlation between patterns of chl response to, e.g. ENSO, and then examine the residual for a trend (this is mentioned in the discussion on page 10334).

10. The fact that we found that the choice of MLD strongly affected the trends in the

CbPM says something significant about the sensitivity of the CbPM algorithm to choice of MLD (as in Milutinovic et al. 2009). It says nothing about the confidence we should have in the trends from the other 3 algorithms that do not require MLD estimates. The source of variability in the CbPM, and ignored by the other algorithms, should arise from taking into account chl:C variability, not from the choice of MLD product. However, we found that the estimated trends were substantially different between each of the CbPM-MLD combinations and from the estimates from the other 3 algorithms. The potential effects of variability in chl:C could be very important in the phytoplankton response to global warming, however because of the strong dependence of the 10-year trends in CbPM PP estimates on the MLD product used, we didn't feel confident including it in our analysis.

11. We've changed this to 'air temperature, wind stress and incoming fluxes of freshwater...'

12. We've substantially altered this paragraph to reflect the reviewer's comments.

13. We've removed the 2nd reference here.

14. Our point here was that the GFDL and NCAR models show similar variability as the SeaWiFS data, but that the IPSL model is different. I think it's better to point out both similarities and differences between model and data, rather than to ignore the differences. We mention the unusual parameterisation of iron in the IPSL model here as a potential explanation.

15. As noted in our response to comment 5, we had meant the statement on page 10313 to reflect the canonical expectations of high latitude response, not as a statement of our results.

16. Although Martinez et al. were able to demonstrate that the time series of the first principal component from an EOF analysis of CZCS and SeaWiFS chl showed similar interannual response to SST, they wisely didn't attempt to demonstrate that CZCS and

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SeaWiFS chl are directly comparable. I would still be very wary of directly comparing the magnitude of chl or PP across the two sensors in order to deduce decadal-scale changes. So, I would argue that it is still true to say that we cannot confidently assess long-term changes in the magnitude of chl/PP from the two sensors. However, on page 10330 we've added. . ."Although a recent study (Martinez et al., 2009) employed EOF analysis to demonstrate that variability in CZCS and SeaWiFS chl responded in a similar fashion to SST, directly comparing the two datasets remains challenging." The final sentence on page 10329 is still correct – we need 40 years of continuous data to distinguish a trend from natural variability. As pointed out in the next paragraph (page 10330), gaps in the time series extend the amount of data needed to detect the trend. We've changed line 26, page 10329 to reflect that a continuous time series is needed.

17. The spatial scale of ocean productivity's response to decadal variability is very large – as shown in Behrenfeld's own work that shows the relationship between MEI and productivity in the entire stratified ocean (> 70% of the global ocean). To test whether the number of years required is strongly dependent on the binning, we repeated the analysis by averaging chl or PP time series of the regions first, and then calculating the detection time. (In the manuscript the biome mean detection time is calculated by spatially averaging the individual pixel detection times). In the majority of biomes, there was little difference between the two (+ 5 %). The largest differences occurred in spatially patchy regions (detection times calculated from the alternative binning method increased to up to 100 years in some cases), because the trends were smoothed out leaving mostly variability. The pixel level and regional mean detection time is likely to be most robust in the regions of spatially coherent results. Using the IPSL PP as an example (Figure 6), confidence would be high in the equatorial regions and lower in spatially patchy regions, e.g. the Southern Ocean. This is confirmed by calculating the standard deviation on the detection times within each biome (i.e. the variability in pixel level detection time within a biome). So for example, in the equatorial Atlantic, the detection time is 15 + 2 years and in the Southern Ocean (Atlantic sector) the detection time is 43 + 13 years. We've added the standard deviations to Table 1. In response

to the reviewer's comment about why we show the global maps in the first place: The global maps are shown in order to be consistent with the other analyses, e.g. Figure 1. The regional analysis approach is still maintained by presenting the regional mean data in Tables 1 and 2. We chose the IPCC A2 scenario here as it has a steady increase in CO2 emissions and overall the largest increase in emissions over the period to 2100. Thus, we would expect the results to represent a world heavily influenced by global warming. The other scenarios have lower emissions, so the detection time is expected to be even longer. To test this, we applied the detection time analysis to chl from the GFDL model forced by an alternative IPCC scenario, the A1B (the only output immediately available to us). The global mean detection time under this lower emissions scenario was $\sim$10 years longer than for the A2 scenario. However, global CO2 emissions have exceeded the worst case IPCC scenario in 2007 (Raupach et al., 2007), although dropped off recently due to the economic crisis (Le Quere et al., 2009), so perhaps changes in ocean productivity will be detectable even sooner than the models predict. A note to this effect has been added to the discussion.

18. Absolutely right – we've removed the reference to the low latitude Atlantic.

19. 'Just variability' was just a turn of phrase. It wasn't meant to indicate that variability isn't important – obviously it's very important! We've changed this to read 'or natural climate variability',

20. We have changed this sentence to read "The models do well at simulating the contemporary variability in chl, PP and oligotrophic gyre size (Figs. 2, 3 and 4)", which is what Figures 2 and 3 actually show. We've added in the correlation coefficients between satellite-derived and modelled chl and PP on page 10324, and similarly for the gyre size in the caption to Figure 4.

21. The article by Behrenfeld and Siegel (2007) correlates the MEI to chl, but doesn't go so far as to attempt to separate the trend from the MEI-driven variability. We have however added a reference to B&S (2007) here, "For example, if one suspected that the

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El Niño-Southern Oscillation was a dominant source of the decadal variability evident in the SeaWiFS data (as shown in e.g. Behrenfeld et al., 2006; Behrenfeld and Siegel, 2007), one could add an El Niño index term to Eq. (1), assuming a linear response is appropriate."

22. We've added in here the following... "Ocean colour missions are currently underway or planned outside the US, particularly by India and the European Space Agency (ESA). ESA launched the MERIS ocean colour instrument in 2002 and has supported a programme to merge MERIS, MODIS and SeaWiFS data to construct a consistent ocean colour record (GlobColour; www.globcolour.info). ESA also plans to launch an ocean colour sensor on Sentinel-3 in 2013, and India has recently launched OceanSat2 which has ocean colour capabilities. However, restricted routine access to data and poorly characterised imaging capabilities have limited the use of non-US ocean colour data in the past."

23. When analysing the model output, when the trend becomes detectable is not dependent on the year the record starts, because of the way we've defined the trend to be detectable (i.e. outside 1 s.d. of the control run). In fact, this analysis was originally run on model output extending back to 1860 (but of course during the first many, many years a long-term trend could not be distinguished from natural variability). This can be seen in (for example) Figure 7a; if the time series were extended back another 50 years it wouldn't change the result – the global warming and control runs would still be within 1 s.d. of each other until $\sim$ the decade of 2033-2043. We've added a note about this on page 10331.

24. We calculated the linear trend in the full 50 years of the modelled chl and PP records. In some biomes, there is no statistically significant trend. In most biomes, the sign and/or magnitude of the trend do not agree between the 3 different models. The exceptions are the North Pacific and North Atlantic oligotrophic gyres, where the all 3 models have a statistically significant trend, the sign of the trend agrees in all 3 models, and the modelled trend agrees in sign with the SeaWiFS 10-year trend.

Therefore, generally speaking one cannot use the trend in the 10 years of SeaWiFS data to extrapolate to a longer term trend.

25a. Whether or not detecting the effects of anthropogenic climate change was an original driving motivation for the satellite ocean colour programme, it is now being used in that way, and in fact I would argue that this is one of the prime motivators, for scientists and government agencies alike, for continuing an ocean colour satellite programme. I agree whole-heartedly that we can't wait 40 years to take action on climate change – and we are not arguing for that at all! Our point is that we need to continue observing the oceans for decades to be sure of detecting the impact of climate change. On pages 10332-10334, we discuss ways in which the response of ocean biology may manifest itself and be detectable long before 40 years is up. On the question of whether relationships between climate and biology observed over 10 years might be appropriate for longer time scales there are 2 schools of thought. One is that interannual/decadal variability can be used as an analogue of the future response to climate change and the other suggests that future changes will not map onto current modes of variability and that the system could undergo changes which are not analogous with current responses to variability, for example regime shifts, which we also discuss as a possible mechanism in the discussion (a review of papers reflecting both points of view can be found in Stone et al., 2001, J. Clim.). We've altered page 10334 to include discussion of this point.

25b. As an observational oceanographer, I would rather say how well the model captures processes represented in the satellite data, rather than the other way round! I agree that one of the important uses of ocean colour data is improving confidence in model output, but I don't think we should rely on models alone in the future – we need the continued observations too. It is very encouraging that the models seem to do a good job at reproducing the variability in, for example, the extent of the gyres. However, in this analysis, we have not attempted to demonstrate a correspondence between SST or stratification in either the model or the satellite data (contrary to the reviewer's comment that, 'The satellite data also demonstrate the strong correspondence between stratification and SST changes predicted by the models'). All of the models incorporate light and nutrient stress on phytoplankton growth, so they include some of the effects demonstrated in Behrenfeld et al. (2008). It's possible that the patterns in the satellite data and model output match for the wrong reasons, as suggested by the reviewer, but equally it's possible that both the models and satellite chl or PP capture the essentials of how climate variability impacts ocean biology.

Interactive comment on Biogeosciences Discuss., 6, 10311, 2009.