Review of Henson et al. Biogeosciences
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The current manuscript employs three biogeochemical models to evaluate detection requirements for long-term climate change effects on ocean biology using satellite remote sensing data. Central findings are interesting and have relevance to the continued maintenance of global satellite assets. I do not have the expertise to critically evaluate the biogeochemical models.

SPECIFIC COMMENTS

1) (Title) The analysis presented in this manuscript does not address the question posed in the title, so I believe it should be changed (perhaps, "Are effects of long-term anthropogenic climate change detectable in current satellite records of surface ocean chlorophyll and primary production?). To answer the current title question, one would have to first determine whether human-induced climate change has already occurred. If not, then the answer is no. If so, one would then have to determine if the climate forcing has changed surface ocean conditions that regulate phytoplankton growth and standing stock. If not, then the answer is again, no. If yes, than the answer to the title's question 'very likely', even if the change is not yet large enough to distinguish from natural variability in current observational data set.

2) (general comment) I do not agree with the general treatment of 'climate effects' throughout this manuscript. Climate varies over a continuum of time scales; from interannual, to decadal, to centennial, to geological. The question that is actually being addressed in this manuscript is, 'Can multidecadal-scale climate trends (assumed here to be only associated with anthropogenic causes - see Fig. 7) be distinguished from interannual and decadal ***climate*** variability in the current 10 year satellite record?' These latter shorter-term climate fluctuations are referred to in the manuscript as 'natural variability', which I feel is an inadequate term. It should be made clear throughout that this 'natural variability' is a form of climate variability. Indeed the magnitude of this shorter-term variability is assessed based on changes in modeled climate forcings. This is not a minor point, as I believe it has strongly influenced many aspects of the manuscript (below).

3) (Abstract) From center of abstract:

> "We find that detection of ***real trends*** in the satellite data is confounded by the relatively short time series and large interannual and decadal variability in productivity."

What makes a trend 'real' or not? The SeaWiFS record is dominated by two very clear trends - the first associated with relaxation from an El Nino and the later a progressive warming period with secondary fluctuations. These trends are very 'real' and are associated with climate variability on the interannual-ENSO-decadal time scales (more below).

4) (general) A general impression given by this manuscript is that earlier satellite-based studies have claimed that anthropogenic climate change impacts are detectable in the satellite record. Accordingly, a central conclusion repeatedly stated in the manuscript is that these longer term trends can not be deciphered from shorter term climate variability in a satellite record of only

~10 yrs. The current manuscript's treatment of the earlier studies is both inaccurate and incomplete. To illustrate, consider the following paragraph (pg. 10314):

> "With over 10 yr of data now available, SeaWiFS products are being used to explore trends in sub-tropical productivity (e.g., Behrenfeld et al., 2006; McClain et al., 2004; Gregg et al., 2005), coastal productivity (Kahru and Mitchell, 2008; Kahru et al., 2009) and extent of the oligotrophic gyres (Polovina et al., 2008; McClain et al., 2004; Irwin and Oliver, 2009). Several of these studies attribute the observed trends to the impact of global climate change. A different conclusion was reached by Yoder et al. (2009), who compared the trends in 8 yr of SeaWiFS chlorophyll to output from a global biogeochemical model. They concluded that trends at 11 selected ocean sites were not unusual in relation to the longer model record."

Now, lets take a look at each reference in order:

(i) Behrenfeld et al. 2006 - This manuscript focused on observed relationships between SeaWiFS chlolorphyll/net primary production (NPP) and indices of climate variability (SST, MEI, stratification) over ~10 years. The conclusion was that most of the variability in biological indices could be directly associated with the climate-driven environmental variables and that this tight correspondence may provide insight on how ocean biology will respond to longer term changes anticipated from global climate models (which likewise impact SST, stratification, etc). For example,

first paragraph: "The observed reductions in ocean productivity during the recent post-1999 warming period provide insight on how future climate change can alter marine food webs."

Pg. 753: "Our results clearly link Chl and NPP changes to SST, stratification and climate variations and provide an opportunity for comparison with simulated ocean responses to a warming climate. Atmosphere–ocean general circulation models coupled to ocean ecosystem models are essential tools for understanding global carbon cycle–climate feedbacks. When forced by rising atmospheric CO2 concentrations, these prognostic models consistently yield increased
SSTs and net decreases in stratified ocean NPP, consistent with observed changes in NPP during the 1999 to 2004 period of rising MEI (Fig. 2a). Also consistent with our results, model-simulated NPP changes are largely due to intensified water-column stratification and are not uniformly distributed across the stratified oceans."

Pg. 754: "The index used here (MEI) to relate climate variability to NPP trends does not distinguish natural from anthropogenic contributions"

(ii) McClain et al. 2004 - This manuscript provided the first description of changes in the size of oligotrophic gyres. Their objective (which I've confirmed with the lead author) was simply to describe observed changes. Their conclusions:

Pg. 294: "Although the available ocean-color record is still very short to unequivocally correlate climate variability indices to observed trends in biomass, some consistent patterns are emerging."

Pg. 299: "Although the available ocean-color record is still too short to unambiguously relate these trends in biomass to climate variability, some consistent dynamic patterns related to known climate cycles (PDO, NAO, ACW) are beginning to emerge. A longer ocean record (~10 years) will be required to establish a more concrete correlation between the long-term variability of the subtropical gyres and climate trends. The exact nature of the couplings will require modeling studies in addition to the collection and analysis of longer satellite and in situ data records.... The analyses presented herein are intended to simply highlight the possible connections between biological and physical variability in the subtropical gyres on seasonal and interannual time scales"

<u>(iii)Gregg et al. 2005 </u>- This manuscript described relationships between changes in chlorophyll and SST for SeaWiFS. The authors stated that the global relationships between chl and SST reflect a dependence of ocean biology on climate forcings that can occur over a broad range of time scales (specifically naming important basin oscillations).

pg L03606: "The 6-year time series of global ocean chlorophyll from SeaWiFS is insufficient to unambiguously characterize long-term trends. It is also difficult to relate the trends to climate decadal oscillatory behavior, such as the North Atlantic Oscillation and Pacific Decadal Oscillation, among others..."

They came (unfortunately) close to suggesting that the observed trends in coastal zones may be indicative of long-term climate change, but fortunately never went that far.

<u>(iv) Kahru and Mitchell, 2008 </u>/ <u>Kahru et al., 2009</u> - These two manuscripts describe changes in eastern boundary current bloom magnitudes observed by OCTS and SeaWiFS (global and California, respectively). In both cases, the authors discuss (and dismiss) possible (natural) climate variability as the cause and leave the question open for future research, e.g.:

(***Abstract, Kahru et al***) "The reasons for this increase are not clear but are associated with various environmental conditions."

<u>(v) Polovina et al., 2008 </u>- This is the ***one and only*** manuscript I found specifically stating that the expansion of gyre size is indicative of long term climate change:

Pg L03618, Abstract: "The expansion of the low chlorophyll waters is consistent with global warming scenarios based on increased vertical stratification in the mid-latitudes, but the rates of expansion we observe already greatly exceed recent model predictions."

(vi) <u>Irwin and Oliver, 2009 </u>- This is the third manuscript that has been published regarding the expansion of the open ocean gyres. Their conclusion regarding this changes is as follows:

Pg L18609: "The rate of change of the combined area of the three oligotrophic regions appears to be linked to the PDO and suggests the existence of a second mechanism driving the aggregate areas.... Our analysis of the areas of oligotrophic provinces in the global ocean suggests that the signal of temperature change and the PDO are jointly imprinted on the provinces leading to extreme oligotrophic regions growing at an accelerating rate within the envelope circumscribed by the PDO. The PDO appears to be stable over many centuries which suggests the existence of a stable envelope for the oligotrophic gyres."

(vii) Yoder et al. 2009 - This paper is not available for comment.

In Conclusion, the current manuscript is inaccurately portrays most of the earlier studies. My recommendations are (1) remove the above paragraph and replace it with a couple paragraphs summarizing the finding of each early study, (2) remove the statement of the Yoder paper being different from the earlier studies, and (3) remove the repeated emphasis that long-term trends can not be deciphered from a 10 year satellite record and make this statement only once in the discussion, possibly pointing out that the Polovina et al paper went too far in this regard. Additional relevant publications that should be considered here are:

a) Martinez et al., 2009 Science 326, 1253 - this is an excellent and very relevant paper that demonstrates the link between decadal scale climate variability and surface chlorophyll over the full CZCS-SeaWiFS period - demonstrating consistent climate-biology relationships between earlier interannual trends and decadal trends

b) Behrenfeld & Siegel 2007 *IGBP News Letter* 68, 4-7 - extension of SeaWiFS finding over a 50 year period using contemporary relationships between chlorophyll and a climate index

c) Behrenfeld et al. 2009. *Bull. Amer. Meteor. Soc*. August 2009. 90(8): S68-S73.
d) Behrenfeld et al 2008 *Bull. Amer. Meteor. Soc*. July 2008. pp. S56-S61.

These two papers extend the SeaWiFS analysis to *include high latitude* regions and over a longer time frame

e) Behrenfeld et al. 2008. *Phil. Trans. Royal Soc. B* 363, doi:10.1098/rstb.2008.0019 - this paper revisits the SeaWiFS record and distinguishes chlorophyll changes due to changing biomass, growth rate, and photoacclimation, pointing out a very important issue that needs to be considered when interpreting satellite data and when comparing satellite data with output of biogeochemical models (section 9 of the paper).

5) (page 10313, top of page) Text reads:

> "At high latitudes, where phytoplankton growth is light limited during winter, decreased mixing may result in earlier re-stratification and a lengthened growing season, resulting in increased PP (Bopp et al., 2001; Doney, 2006)."

While this might be consistent with the GFDL model, it is not consistent with the IPSL or NCAR model (see figure 5), the satellite record (see references c & d above), or expectations of Evans and Parslow (1985).

6) (page 10313, line 25) The statement “SeaWiFS has been measuring surface chlorophyll...” is not strictly correct, as SeaWiFS measures radiances, from which water leaving radiances are derived, from which chlorophyll is derived.

7) (page 10313, line 28) ‘...data are available’ not ‘data is available’

8) (page 10315) Regarding the statement, “However, chl can change without corresponding changes in phytoplankton biomass or PP, due to the ability of cells to alter their chlorophyll to carbon ratio in response to light or nutrient stress (e.g., Laws and Bannister, 1980; Geider, 1987)”, reference e above demonstrates how important this physiological variability is to the SeaWiFS record.

9) (page 10315) I’m very concerned about the linear regression approach applied to the SeaWiFS data. The SeaWiFS record is biphasic (as discussed above) and simply running a single linear regression line through the data seems like a *very* poor way to evaluate observed changes (it will strongly minimize the scale of changes observed during the two phases and may even give the wrong *sign* for most of the record in some regions. I think this needs to be more carefully re-evaluated - with revision to the SeaWiFS data points in current figures 2 and 3

10) (page 10316) It was nice to see that the CbPM was also investigated (although the Westberry et al 2008 algorithm would have been the better choice). Earlier, on the same page, it is stated that “chl can change...[due to]...response to light or nutrient stress”. The CbPM attempts to account for this variability, while your 3 other algorithms do nothing to account for this variability. The fact that you found that “each MLD product resulted in substantially different magnitude and spatial distribution of statistically significant trends” seems to say something very significant about the confidence that can be placed on the SeaWiFS trends you evaluated (?), particularly for the other 3 algorithms that simply ignore this source of variability.

11) (page 10316, line 20) the wording ‘incoming flux of wind stress’ seems odd to me. Is this correct?

12) (page 10325-26) I found the section beginning with “In the Arabian Sea,....” to “when viewed in a long-term context” unsatisfying. Specifically, the fundamental assumption of this investigation is that the models can adequately characterize natural climate variability on the interannual to decadal time scale such that one can evaluate whether current trends in the SeaWiFS record are ‘outside’ this range. Chlorophyll changes in the Arabian Sea and North Atlantic were outside the expected range, so the authors argue that in these two cases the models are wrong. This violates the initial assumption. If the models are to be questioned, then we must question all regions. Perhaps the models overestimate ‘natural variability’ in many regions, meaning that the assessment that SeaWiFS trends are ‘within natural climate variability’ is incorrect. I think that one must simply state that the Arabian Sea and North Atlantic results are outside expected variability and leave it at that, otherwise the overall approach is compromised.

13) (page 10327) In the statement, “The negative trends in SeaWiFS chl in the oligotrophic gyres (Fig. 1) have been attributed to global warming-related increases in SST and stratification (Polovina et al., 2008; Behrenfeld et al., 2006).” The second reference is incorrect.

14) (page 10327) From the statement, “The North Atlantic has an increasing trend in oligotrophic area with large decadal variability superimposed in the GFDL and NCAR models. No trend in
the North or South Atlantic gyre size is evident in the IPSL model. This may be due to the implementation of a minimum iron concentration in the IPSL model, which has the effect of dampening the variability of iron and corresponding variability in PP.”, is it also not possible that the IPSL result is correct and the satellite detected trends are a reflection of anthropogenic forcings? I personally doubt it, but again it compromises the underlying assumption of this study when a model error is blamed each time findings emerge where satellite observed changes exceed expectations based on interannual to decadal variability.

15) (page 10328) As noted above (comment 5), the statement here that “The models generally show a decreasing trend in chl in the oligotrophic gyres and ***high latitudes***, and increasing trends in the Southern Ocean” contradicts the statement on page 10313.

16) (page 10330) Given the successful demonstration by Martinez et al. 2009 (reference a above) that the Antoine et al re-analysis of CZCS data gives similar climate dependencies between chlorophyll and SST as observed for SeaWiFS alone (and the lack of a similar demonstration of Gregg et al. 2003 data set), I think it is no longer adequate to simply ignore the longer term satellite record by citing the inconsistency between the Antoine analysis and Gregg analysis. Thus, I think this section needs to be revised and updated to accommodate the new Martinez findings. (Note that given this, the final sentence on page 10329 is no longer correct).

17) I’m not very comfortable (yet) with the analysis of ‘required satellite years to detect trend’. What I don’t understand is how spatial integration influences the results. Specifically, when I look at figure 6, I interpret it as indicating the number of years of continuous satellite data needed to detect a long term climate trend at the pixel scale. Since ‘natural variability’ at the pixel is so much greater than variability within regionally averaged bins, it would seem to me that the number of years required will be strongly dependent on degree of binning. Am I missing something here? Also, why show the global maps in the first place, rather than just sticking to the regional analysis approach of the first part of the manuscript? Similarly, how does area of integration influence the confidence of the model results? Certainly we have much less confidence in model descriptions at the pixel scale than at the regional scale. How much confidence can we then have on pixel level ‘required satellite years’? Finally, for these projections, it would seem to me that the required years of satellite data is very much dependent on the strength of the anthropogenic climate change. In this analysis, you adopt a single scenario for future change. How would ‘required years’ change if you used other climate change scenarios?

18) (page 10331) Regarding the statement, “The IPSL model suggests that the global warming signal in PP may be detectable within the satellite era in the equatorial and low latitude Atlantic”, I can see this in Table 2 for the Equatorial Atlantic, but not the low latitude Atlantic.

19) (page 10332) In the statement, “Ten-plus years of ocean colour data have provided unprecedented coverage of changes in ocean productivity – but are the observed changes reflecting global warming or ***just variability***?”, what does this mean ‘just variability’??? Clearly,

the analysis presented here does not regard random noise, but evaluates short term climate variability simulated in models and reflected in satellite data. This 'just variability' may be critical for us to understand climate-biology links that are expressed over short time scales but are mechanistically equivalent to dependencies acting over much longer time scales.

20) (page 10332) I don't agree that figures 2 and 3 provide evidence supporting the statement, "The models clearly do well at simulating current conditions". Can you be more quantitative?

21) (page 10334) Regarding the statement, "For example, if one suspected that the El Nino-Southern Oscillation was a dominant source of the decadal variability evident in the SeaWiFS data, one could add an El Nino index term to Eq. (1), assuming a linear response is appropriate. This could assist in separating the decadal variability from the trend and might permit even a trend of small magnitude, relative to the variability, to be examined.", see reference b above.

22) (page 10334) The paragraph beginning with "All these considerations..." completely ignores all of the non-US ocean color sensors planned for the future. I think this paragraph needs to be rewritten to reflect these other data sources and what will be required from the global science community to ensure that the combined data set fulfills scientific needs.

23) (table 2) the year when long term trends will be detectable is dependent upon the year that the record starts, so this needs to be added to the legend. Also, do we start with SeaWiFS, OCTS, or CZCS (see comment 16).

24) (general) One of the major things that seems to be missing in this manuscript is a set of 12 plots (as in figures 2 & 3) showing the 1960 to 2009 modeled chlorophyll records for the 3 models. In other words, figures 2 and 3 show the model results for 10-year running values. What does the record look like for simple chlorophyll concentrations over the 50 year period? Is there an appreciable trend? Can a long term trend be deciphered in the model data for the past 50 years from the shorter term climate variability? This is not quite the same question as asking how long into the future a satellite record is needed to detect changes, since the past rate of change is likely different from the future rate.

25) As a final, sort of broader set of comments/questions .....

Is directly observing the effect of anthropogenic climate change a driving motivation for satellite ocean color measurements? This seems to be an assumption of this analysis, but I'm not sure it is correct. Is it not far more important to use the satellite data to evaluate climate-biology interactions functioning now as a means of evaluating confidence in model predictions over much longer time scales? The climate change problem is upon us today and we can't simply afford to wait for 40 years to do something. This raises the question of how important is the time-scale of the climate forcing on the resultant physical-biological relationship? Can relationships observed over 10 years provide insights and validation for longer term trends? Can this question be answered with the models? I would hate to think that we need 40 years of continuous satellite data before a primary mission is accomplished. I'm guessing that over this time period changes will be so large that we won't need to scrutinize a satellite data set to demonstrate them.

While this manuscript has emphasized how a 10 year satellite record is inadequate to detect long term trends (seems like kind of a ‘duh’ to me), what instead struck me as particularly interesting is how well the satellite data captures processes represented in the model. For example, the changing extent of the central gyres is very consistent with the model predictions. The satellite data also demonstrate the strong correspondence between stratification and SST changes predicted by the models. Such findings give me much greater confidence in the models’ ability to predict longer term change, and here is where I think the value of the satellite data really lies, not in the direct observation of long term trends over half a century. My biggest concern is that we may not be interpreting the satellite data correctly, such that the mechanisms underlying current satellite observed changes are not the same as those functioning in the models (see reference e), so the patterns match but for the wrong reasons.