Biogeosciences Discuss., 6, C3811–C3816, 2010
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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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Received and published: 4 January 2010

Review of "Is global warming already changing ocean productivity?" submitted to Biogeosciences by S. A. Henson et al.

In this manuscript the authors use satellite-based model estimates and physical-biogeochemical model estimates to assess whether (and when) the effects of global climate change on ocean chlorophyll and productivity are (will be) detectable. The manuscript is refreshingly well written. The background provided is excellent, and there is an appropriate amount of detail provided on the models used in this analysis. The work is creative, novel, and yields important results. Overall I think this will make an excellent contribution to Biogeosciences. Below I outline three relatively minor general comments and numerous specific comments, which if addressed, I feel will make this

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an even stronger contribution.

General Comments:

1. Contrary to the question posed in the manuscript's title, the authors are not really looking at whether global warming is changing productivity, but rather whether global climate change has a detectable effect on productivity and chlorophyll. As mentioned in the third sentence of the manuscript, future changes in productivity will result not only from warmer temperatures, but also from changes in wind patterns as well as from changes in storm frequency or strength, and other factors as well. I feel the paper would be stronger if 'global warming' was perhaps replaced by 'global climate change' in most, if not all, instances in the manuscript. The title should also be adjusted. Since chlorophyll and productivity are both dealt with in the paper, 'chlorophyll' perhaps should be included in the title.

2. It is not clear to the reader that the 'chlorophyll' being considered here is surface chlorophyll, whereas the productivity estimates presented are actually integrated values. As mentioned below, this is yet another reason why satellite-based (integrated) productivity estimates are not nearly as reliable as satellite-based (surface) chlorophyll concentrations. It is difficult to get any type of subsurface information from satellites. This is a minor point, but an important one. 'Chlorophyll' should be replaced with 'surface chlorophyll' and 'productivity' be replaced with 'depth-integrated productivity' to remind the reader of this key difference.

3. The satellite-based PP estimates cannot be used to evaluate the physical-biogeochemical model estimates. This occurs in multiple places throughout the manuscript. As pointed out below, Friedrichs et al. (2009) show that in some instances physical-biogeochemical model estimates are more skillful than some of the satellite-based PP models used in this analysis.

Specific comments:

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Abstract: Several acronyms are not defined in the abstract. I would remove the acronyms and replace 'SeaWiFS' with: 'chlorophyll and productivity derived from ocean color satellite data'. I would also replace 'longer-term records' with 'longer-term time series'.

p. 10313, 2nd paragraph: I think it might be worth mentioning the long productivity time series from BATS and HOT. Even though these data are not used in this analysis, it would be useful for the reader to know that in situ time series of nearly 20-year duration do exist. Although in situ time-series such as these are not necessarily representative of large ocean biomes, they have the advantage of being able to provide information on subsurface changes.

p. 10314, line 15: Define the A2 scenario here. (It is currently defined/described later in the manuscript.)

p. 10314, line 27: A significant point that needs mentioning here is that 'chl' refers to surface chlorophyll, whereas by 'PP' the authors are referring to depth-integrated productivity. We would expect satellite-derived surface quantities to have lower errors than satellite-derived depth-integrated quantities.

p. 10315, line 8: Friedrichs et al. (2009) showed that roughly half of these uncertainties may be due to errors associated with the input parameters (surface chlorophyll, SST, PAR.)

p. 10315, line 8: It is important to point out to readers that because of the uncertainties associated with estimating integrated PP from satellite-derived surface chlorophyll, one cannot assume a priori that PP estimated from satellite data is more accurate than PP estimated from global physical-biogeochemical models (i.e. compare the 'Total RMSD' and 'Bias' results for Models 2, 9 and 15 with Model 24 in Table 3 of Friedrichs et al., 2009.)

p. 10315, line 23: The phrase 'chlorophyll is converted to PP' may mislead readers.

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Other input parameters like SST and PAR go into these satellite-based models as well. Surface chlorophyll is just one parameter that goes into the integrated PP models. This really is more than a ‘conversion’.

p. 10316, lines 3-15: Can be removed

p. 10316, line 17: ‘define’ should be ‘estimate’

p. 10321, line 13: ‘SeaWiFS PP data’ should be ‘SeaWiFS-derived PP’, i.e. the word ‘data’ should be deleted.

p. 10322, line 17: I believe ‘n* is increased by a factor of 1.59’ should be: ‘n**= 1.59 * n*’ or ‘n** is a factor of 1.59 larger than n*’.

p. 10323, line 24: Similarities and differences between the results shown in Fig. 1 and those of Behrenfeld et al. 2006 should be mentioned here as well.

p. 10324, lines 22: No, the biogeochemical models cannot really be ‘evaluated’ by comparing to the satellite-derived PP. As stated above, errors in the satellite-derived PP can be greater than those associated with the modeled PP. It is still an interesting comparison – the sentence just needs to be reworded.

p. 10324, lines 23, 25: Careful here and line three of captions for Fig. 2 and 3: ‘SeaWiFS data’ should be ‘SeaWiFS-derived PP and chl’

p. 10326, line 16: Again, the authors should be careful about saying that the biogeochemical models are wrong because they don’t match the satellite-derived PP. (See Friedrichs et al., 2009 Table 3: Results from Model 24, an NCAR variant, are more highly correlated to the PP data than are the Carr et al. satellite-based PP model results.)

p. 10329, line 19: This is a very important result of this paper. Although many regions will require a 50-60 year time series (a depressing result), this analysis points out where we should start looking for trends: not necessarily in the polar regions, but rather in the

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equatorial areas. This result is important enough to warrant inclusion in the abstract. (However: why do the authors say in the text that the North Pacific has short detection times – Table 1 shows 41 years, which is much higher than the time required for the equatorial regions.)

p. 10329, line 23: Again, why is the North Pacific mentioned here? Table 1 indicates that the North Pacific detection time is 35-41 years – higher than average.

Fig 6: Table 1 seems to illustrate the results much more clearly than does Figure 6. Is Figure 6 necessary? Could it be smoothed? An alternative would be to plot the values in Table 1 on a map, so each biome would be a different color. It's also fine as is.

Sections 3.4 and 3.5 are closely related. (Aren't the questions posed actually the same?) The authors could consider combining these sections.

The relationship between Table 1 and Table 2 is a bit confusing. Table 1 indicates that we would need a 41-year time series to detect a chlorophyll trend in the high latitude North Pacific, and a 35-year time series to detect a trend in the equatorial Pacific. Then why do we expect to see a trend in 2067 in the N. Pacific and two years later (2069) in the equatorial Pacific? This needs a bit more explanation.

p. 10334, line 5: This also points to the problem with the empirical satellite-based productivity estimates. These empirically tuned models are not likely to be reliable if we reach such a 'tipping point'. As an example, note that the satellite-based PP models in the tropical Pacific worked well in the 1990s, but not nearly as well in the 1980s (Friedrichs et al., 2009.)

p. 10335, line 1: "unlikely to be an effective strategy". In my opinion, this analysis does not indicate that the oceanographic community should not set up in situ time series sites. This could be stated a little less strongly. We absolutely need subsurface information, which is not available from satellite data.

Reference:

Friedrichs, M.A.M., M.-E. Carr, R. Barber, M. Scardi, D. Antoine, R.A. Armstrong, I. Asanuma, M.J. Behrenfeld, E.T. Buitenhuis, F. Chai, J.R. Christian, A.M. Ciotti, S.C. Doney, M. Dowell, J. Dunne, B. Gentili, W. Gregg, N. Hoepffner, J. Ishizaka, T. Kameda, I. Lima, J. Marra, F. Mélin, J.K. Moore, A. Morel, R.T. O'Malley, J. O'Reilly, V.S. Saba, M. Schmeltz, T.J. Smyth, J. Tjiputra, K. Waters, T.K. Westberry, A. Winguth, 2009. Assessing the uncertainties of model estimates of primary productivity in the tropical Pacific Ocean. Journal of Marine Systems, 76, doi:10.1016/j.marsys.2008.05.010.

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