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Biogeosciences
Discussions

Interactive comment on "Marine regime shifts in ocean biogeochemical models: a case study in the Gulf of Alaska" *by* C. Beaulieu et al.

Anonymous Referee #1

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Overview:

This paper explores the response of five different ocean biogeochemical models averaged over the Gulf of Alaska (GoA) to the same physical forcing over the period 1950-2007. Time series of physical (SST and MLD) chemical (DIN, SI, FE) and biological (surface chlorophyll, integrated primary productivity, and surface phytoplankton and zooplankton biomass) from the models are analyzed using a change point detection scheme. The method consists of a set of regression models that can classify the time series as a constant mean, mean shift, trend, shift in the intercept of the trend and shift in both the intercept and trend. The method is able to detect one change throughout the time series. A downward trend in GoA SST is identified prior to 1976 followed by a weak upward trend afterwards with a slight upward trend in MLD over the

period. Most of the simulated biogeochemistry time series indicate a change around 1976 but show a mix of behavior with the simpler models exhibiting more regime-like behavior than the more complex models.

A comparison of how different ocean biogeochemical models with the same physical state simulate time series of key quantities in different parts of the globe, including the Gulf of Alaska, is a useful endeavor. The same can be said for a fairly rigorous evaluation of change points in these time series. Thus I accept this paper for publication in Biogeosciences, but I think the manuscript can be improved in several ways and thus I recommend a major revision.

Comments:

1) While regime analysis has become very popular especially in climate and marine ecosystem analysis, it may lead to a misinterpretation of the underlying dynamics of a system, especially for relatively short time series. For example, regimes are often linked to phases of the Pacific Decadal Oscillation (PDO). The transition in the North Pacific around 1976-77 has been linked to a change in the PDO (which extends into the tropics), while the one around 1998 is more associated with the second EOF sometimes termed the North Pacific Gyre Oscillation (NPGO, also relevant for the discussion in the Introduction, page 5, lines 22-33). Rather than regimes these just might be periods where one pattern is more prevalent than another, where both of these patterns impact the GoA. In addition, evaluating the time series as single AR1 process (for the model of the mean and no change) may not be the best null hypothesis. Anomalies in the state of the GoA are strongly influenced by ENSO and other factors. If these processes fluctuate, they could cause rapid changes in the remote time series even if these processes are linear and add to each other.

The authors should discuss these complicating factors (or perhaps even try and incorporate them as one of their models. See the following papers:

Bond, N. A., J. E. Overland, M. Spillane, and P. Stabeno (2003), Recent

shifts in the state of the North Pacific, Geophys. Res. Lett., 30(23), 2183, doi:10.1029/2003GL018597. B

Di Lorenzo, E. et al. North Pacific Gyre Oscillation links ocean climate and ecosystem change. Geophys. Res. Lett. 35, L08607 (2008).

Matthew Newman, 2007: Interannual to Decadal Predictability of Tropical and North Pacific Sea Surface Temperatures. J. Climate, 20, 2333–2356. doi: http://dx.doi.org/10.1175/JCLI4165.1

Newman, M., G. P. Compo, M. A. Alexander, 2003: ENSO-forced variability of the Pacific Decadal Oscillation. J. Climate, 16, 3853-3857.

Schneider, N., and B. D. Cornuelle (2005), The forcing of the Pacific Decadal Oscillation, J. Clim., 18, 4355 – 4373.

2) How would the method used here classify a pure sine wave with (one) zero value some where in the time series? Would it classify the zero crossing as a change point or regime shift? (Same goes for the changes in the “trends” when the amplitude of the waves switches sign.) Clearly the dynamics behind an oscillatory signal would likely be quite different than the dynamics for a regime shift or trend.

3) The authors should probably reference other modeling studies of decadal physical and biogeochemical changes in the northeast Pacific:

Alexander, M., A. Capotondi, A. Miller, F. Chai, R. Brodeur and C. Deser, 2008: Decadal variability in the Northeast Pacific in a physical-ecosystem model: The role of mixed layer depth and trophic interactions. Journal of Geophysical Research - Oceans, 113, C02017, doi:10.1029/2007JC004359.

Capotondi, A., M. A. Alexander, C. Deser, and A. J. Miller (2005), Low frequency pycnocline variability in the northeast Pacific, J. Phys. Oceanogr., 35, 1403 – 1420

Haigh, S. P., K. L. Denman, and W. W. Hsieh (2001), Simulation of the planktonic

ecosystem response to pre- and post-1976 forcing in an isopycnic model of the North Pacific, Can. J. Fish. Aquat. Sci., 58, 703 – 722.

4) The reference for Wunsch 1999 on page 4 line 20 is missing from the reference list.

5) The authors should probably include a discussion of the physical model simulation here even if it is described in other papers and/or on line.

Are the BGC models driven offline where the ocean model (NEMO) is run first and then the values are fed into the BGC models? Note this does not allow for feedback of the biology on the physics (e.g. changes in solar absorption by phytoplankton).

If the surface sensible and latent heat flux are computed using the observed air temperature and model SST, the results will be to strongly relax the model SST towards the observed SST (as the observed air temperature & SST are highly correlated. If this is the case then the model will likely obtain the correct SST regardless of if it is a good simulation or not, e.g. see:

Seager, R., Y. Kushnir, and M. A. Cane (1995), On heat flux boundary conditions for ocean models, J. Phys. Oceanogr., 25, 3219 – 3230.

5) During late winter in the subarctic North Pacific the mixed layer extends to the upper portion of the halocline, located between depths of approximately 70 and 120 m (Roden, 1964; Freeland et al., 1997; de Boyer Montegut et al., 2004) and the MLD is mainly controlled by salinity not by temperature (this would have impacted the Polovina et al. [1995] finding). Low-frequency changes in the Ekman pumping in the Gulf of Alaska, which vertically displaces the halocline, may impact the wintertime MLD by moving a layer with strong density gradients toward or away from the surface. After the mid-1970s the pycnocline was shallower in the central part of the Gulf of Alaska and deeper in a broad band along the coast, primarily due to the local response to Ekman pumping (Cummins and Lagerloef, 2002; Capotondi et al., 2005). This impact should be included using a density definition for MLD although a change in MLD about

1976-77 seen in other studies is not found here.

de Boyer Montegut, C., G. Madec, A. S. Fischer, A. Lazar, and D. Iudicone (2004), Mixed layer depth over the global ocean: An examination of profile data and a profile-based climatology, J. Geophys. Res., 109, C12003, doi:10.1029/2004JC002378. D

Capotondi, A., M. A. Alexander, C. Deser, and A. J. Miller (2005), Low frequency pycnocline variability in the northeast Pacific, J. Phys. Oceanogr., 35, 1403 – 1420.

Cummins, P. F., and G. S. Lagerloef (2002), Low frequency pycnocline depth variability at station P in the northeast Pacific, J. Phys. Oceanogr., 32, 3207 – 3215.

Freeland, H., K. Denman, C. S. Wong, F. Whitney, and R. Jacques (1997), Evidence of change in the winter mixed layer in the northeast Pacific Ocean, Deep Sea Res., Part I, 44, 2117 – 2129.

Roden, G. I. (1964), Shallow temperature inversions in the Pacific Ocean, J. Geophys. Res., 69, 2899 – 2914.

7) Page 15. Paragraph 12-20. The authors indicate that several of the models depict a regime shift in the Gulf of Alaska in late 1980s (instead of the mid 1970s) and that this shift seems to be mainly forced by changes in MLD. However, the change detection method and Figs. 2-4 do not appear to show much of a change in MLD around 1989 either in the full time series or in the PCs.

8) Give the correlation values for the curves in Fig. 7 & 8. Are the correlation values during the different epochs significantly different from each other (rather than significant – i.e. different than zero). The lines in Figs. 7 do not seem significantly different from each other, especially given the large spread (see 7 above).

9) Bottom of page 16 top of 17 (also in the abstract). The authors indicate that all models simulate a decrease in nutrients and biological productivity after 1976. Perhaps, this statement is based on Fig. 3; however, an examination of Figs. A1-A5 indicates more complex behavior. For example, the change point analysis suggests a downward

trend for PHY & ZOO for the DiatHadOCC and PlankTOM10 models and an upward trend in FE in the ERSEM model over the entire record.

10) Bottom of page 17 top of 18. A point of clarification ... "20th century" simulations from the CMIP5 archive (the simulations that are referenced the most from the archive) do not produce a climate shift in the mid 1970s. These models are initialized in the mid 19th century and due to chaotic interactions values during a given time in the model do not directly correspond to those in nature (although the idea is that the models have the correct sensitivity to climate change and have the basic statistics of climate variability correct.) The Meehl and Teng studies (including the one referenced here) are based on initialized hindcast model runs just within a few years (up to 10) prior to the period examined.

11) The authors note that the simpler models tend to produce more regime-like behavior. Are there references from other fields, e.g. systems theory, which can support this from a more general perspective?

12) While the authors note that the models produce different change points, they don't comment much on the difference between models. Indeed one is struck by how different the simulations are especially given that the physical forcing is identical. What does this say about the state of ocean BGC modeling? Are there observations say at OWS P that could support one model over another? Are the BGC models highly nonlinear in that one should perform multiple ensembles (based on different initial BGC conditions) as is done for climate system forecasts – i.e. one would get different results from individual ensemble members. If the model results are so different, does that suggest caution in using change point analyses?

Interactive comment on Biogeosciences Discuss., 12, 14003, 2015.