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Biogeosciences Discussions

Interactive comment on "Climate driven decadal variations of biological production and plankton biomass in the equatorial Pacific Ocean: is this a regime shift?" *by* X. J. Wang et al.

X. J. Wang et al.

wwang@essic.umd.edu

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Re: Referee #1

In this paper the authors use a physical-biogeochemical model to explore the shifts in nutrient, biomass, zooplankton for two periods (1988-1996 and 1999-2007) to explore the regime shift. They use a model that has been well documented, and consider the model shifts in these two time periods extensively. The bottom line is that there does seem to be a shift induced by changes in the physic forcing, particularly an increase in primary and secondary production. Though competent illustrations are given and regional variations in these changes are explored, I felt the results were lacking. Yes,

the model shows a regime shift, but it is just a model. The authors need to go further and indicate how well this compares to observations. I am aware that there are few such observation, but there needs to be some attempt to show these results are not just a model artefact. Can any inferences made from the observations they do reference for fisheries and carbon cycle that suggest the regime shifts that the model captures. Importantly, the authors also need to explore further why their model behaves in this manner; it is, for instance, not immediately clear why there was such a big shift in secondary production.

Response: We understand the reviewer's concern on "it is just a model", and appreciate the suggestion of comparing model outputs with observations. Over the past several years, we have utilized many satellite and in situ datasets for model calibration and validation. For instance, we used in situ data to derive and parameterize the phytoplankton dynamic model which predicts a variable carbon to chlorophyll ratio as a function of irradiance, nutrient concentration and temperature (Wang et al., Biogeosciences, 6, 391-404, 2009). We used the Sea-viewing Wide Field-of-view Sensor (SeaWiFS) chlorophyll for model validations for the tropical Pacific. Here, we cite a reviewer of a slightly different version of this manuscript previously submitted to PNAS: "The model has been used successfully before to study changes in tropical Pacific biogeochemistry and plankton population dynamics associated with the 97-98 El Nino-La Nina cycle. The model is sufficiently well validated to provide a credible analysis of the longer time scale changes targeted in this paper". However, we agree with the reviewer's comment to explore further (see details below).

Examples of further things to explore: - Are there any observations of a shift to large phytoplankton? Could this be an artifact of the model parameterization?

Response: Yes, observation does show a shift to large-cell phytoplankton species in the equatorial Pacific Ocean during the transition from a warm phase to a cold phase (Strutton and Chavez, 2000). We have added the following sentences in the Introduction: "Moreover, shipboard measurements indicate that during the cold phase, many

biogeochemical parameters (e.g., nitrate, chlorophyll and primary production) were comparable to, or in excess of their respective climatological means in the equatorial Pacific Ocean (Strutton and Chavez, 2000). In particular, large-cell phytoplankton increased at least threefold, from 10% to 30% of the total chlorophyll".

- Has there been any documentation of the increased secondary production, and again could this be a artefact of the grazing parameterization.

Response: There are limited observations of secondary production. However, recent field measurements suggest that conditions have changed to support higher standing stocks of zooplankton (Decima et al., DSR II, in press). We have added relevant information in the text.

- Could these shifts be a result of a longer term drift in the model? The model has only a 30 year spinup, which is not long enough to remove all drift in the biogeochemical fields. Has there been a control run without changes in physical forcing done to establish this drift?

Response: The model is a basin-scale model for the Pacific domain between 30°S-30°N, with initial conditions taken from Levitus climatology. We have done numerous spinups, and found that 30-year spinup is long enough. Here, we provide a figure showing model SST and surface nitrate for the last four years (as supplement).

- Why is there a shift to more large phytoplankton? Does increased nutrients alone explain this?

Response: Yes, increased nutrients result in a shift to more large-cell phytoplankton. In general, large-cell phytoplankton requires relatively high nutrient concentration, thus is sensitive to nutrient conditions. We have revised the manuscript accordingly.

- I assume biomass remains constant even though primary production increases because secondary production increases too - but this should maybe be stated.

Response: This is a good suggestion. We have revised the text accordingly.

- Why, in the model, does this increase in secondary production occur. It is much larger than the primary production increase - why?

Response: The equatorial Pacific is characterized as a high-nutrient-low-chlorophyll (HNLC) region due to a few factors, including strong grazing by micro-zooplankton on phytoplankton (Landry et al., 1997). Improved nutrient conditions result in higher growth rate, leading to higher primary production. Because of strong grazing pressure, some of the increased nutrient uptake is transferred from phytoplankton pools to zooplankton pools, resulting in an increase in secondary production. We have explored this issue during the revision. Note: our model produces a larger increase in primary production than in secondary production (see Figure 1).

- Why is there larger temporal variability in the later period?

Response: This is a good question. We believe that it is caused by physical processes, as shown in mixed layer depth anomaly. Further analyses are needed to explore decadal changes in seasonal patterns of physical fields, which is beyond this paper's scope. We plan to address this issue in our future manuscript that has a focus on physical processes.

- What are the combined effect of primary and secondary shifts on, for instance, export of organic matter? The authors have a last paragraph reflecting on carbon cycle, but don't attempt to use their model to speculate on what might happen.

Response: This is a good question. The increases of primary production and secondary production should have large effects on carbon uptake and storage as organic materials. We have revised the manuscript to speculate on possible implications for the carbon cycle.

Details: 2172, line 5: "these" is ambiguous, do you mean "these studies deal specifically with"?

Response: We have re-worded as "these changes are associated with the 1997-1998

ENSO events".

2173, lines 15-20: Are there important signals over the 30 year period from outside the model domain that would impact the shifts that they see? Can these be assumed to be negligible?

Response: We have considered these issues. We believe that any impact from outside the model domain would be very small, if not negligible, for two reasons: (1) our analyses focus on the equatorial Pacific region (i.e., 5°N-5°S) where upwelling is the dominant process, and (2) we only use 20 years (1988-2007) of model outputs for the analyses.

2174, line 20-30: Though the authors give these demonstrations of their "model fidelity" they only give observations from 1990-1996, could they not give PP values from the latter period - satellite derived possibly? Such a comparison of the observations from both periods would go a long way to "validate" the long term model shifts.

Response: We have considered using remote sensed PP for a comparison. However, satellite derived PPs are not direct measurements, and also need validations. Nevertheless, we have used the most calibrated and validated satellite product (i.e., chlorophyll) together with several in situ datasets of water-column chlorophyll for model validation, which spans from 1994 to 2007 (see Wang et al., Biogeosciences, 6, 391-404, 2009). We have reworded accordingly.

2176, lines 7-9: Are there any evidence of this in the real ocean?

Response: We did not find evidence in the literature. Fortunately, recent field measurements conducted in 2004-2005 do show a large increase in zooplankton (80-90%) (Decima et al., DSR II, in press), but little change (or just a small increase) in phytoplankton (Taylor et al., DSR II, in press), comparing with the data collected in early 1990s in the equatorial Pacific. We have added these references, and reworded accordingly.

2176, line 17: "placed" might be better as "located"

Response: done.

2177, line 2-3: I'm not sure what "pooled two-sample" means - please elaborate.

Response: We provide a detailed method in Supplement.

2177, line 14: remove "the"

Response: done.

2177, line 17: What mean is used to find these anomalies? Do both periods have same mean, or is the mean specific for both periods? Please state.

Response: We have added one sentence for the figure caption: "Note: anomaly is calculated using the mean from each period".

2179, line 2: should "states" be singular

Response: We have reworded the sentence.

2179, line 15: remove "the" from "the sign"

Response: Done.

2179, paragraph starting line 25: This information seems a little out of place here, at least mention that these physical changes are included in the model. And does your model respond in the appropriate way - you do have stronger upwelling, but how about "circulation", stronger SEC and deeper thermocline?

Response: We have revised this section.

2180, section 4.3: Ending the paper with this much conjecture is problematic. If your model does not resolve the carbon cycle, then the least you could do is calculate shifts in export of organic matter.

Response: Our model does have a carbon chemistry component. We added this sec-

tion as a PNAS reviewer suggested: “there are obvious implications of these changes for the carbon cycle since the upwelling that affects nutrient supply in the cold tongue also affects the outgassing of CO2. Some discussion of this topic would be of interest to general readers in view of recent observational studies published on the topic by Feely and collaborators”. Currently, we are preparing one manuscript addressing the decadal changes in the carbon cycle, and combined influences of physical and biological changes.

Fig 3: labels are messed up in my version. Also state what the white and black lines represent.

Response: We have added “Superimposed lines on (b), (d) and (f) denote MLD (solid) and Fe110 (dashed) for 1988-1996 (black) and 1999-2007 (white)”.

Interactive comment on Biogeosciences Discuss., 7, 2169, 2010.