Biogeosciences Discuss., 7, C1497–C1500, 2010
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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.

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Received and published: 22 June 2010

Re: Referee #2

This is a very descriptive paper that presents ocean model results of changes in biology/ ecosystems in the equatorial Pacific about the 1997-1998 shift. Although the model results are potentially interesting, the paper suffers from several shortcomings. First, the results are not organized around a scientific question. Second, there is very little attempt to anchor the results in observations, much less to use the model to interpret observations. Third, the model used does not appear to include a sediment source of iron. Taken together, these shortcomings mean that the paper is not acceptable for publication in its present form. However, if these questions are appropriately addressed through the review process, the paper should be considered for publication in this journal.

As stated above, the principal shortcoming is that the paper here is not organized around a science hypothesis, but is instead largely descriptive. Previous papers have described decadal variations in equatorial Pacific biology/ecosystems, but the authors do not appear to be offering a new mechanism or interpretation that builds on previous work. The authors should rethink the main points of emphasis, as the paper would be strengthened if it were to address a science question, either by shedding light on an existing question or posing a new question. Emphasizing or highlighting a new mechanism would help.

Response: We have added some relevant references and revised the paper to address the above comments. The following are some key responses: (1) Introduction: "These observations imply the possibility of a biogeochemical regime shift in the equatorial Pacific in response to the late 1990s physical changes. However, a global model (Rodgers et al., 2008) could not find a similar signal for the 1997/1998 transition"; (2) Introduction: "The objective of this study is to test the hypothesis that there is a biogeochemical regime shift in the late 1990s in the equatorial Pacific".

The second important limitation here is that the authors do not make a convincing case that they have exhausted the available data in evaluating the model simulations. If the authors believe otherwise, they must be very clear in arguing why. Modeling papers are of limited utility if they provide neither a hypothesis/mechanism nor an account of the observations.

Response: We have utilized most available data (e.g., 1997-2007 satellite and 1994-2007 in situ chlorophyll data) for model calibration and validation (see responses to reviewer 1's comments). In addition, the model has been validated in many other biogeochemical fields, e.g., nitrate, ammonium, dissolved and particulate organic nitrogen

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(Wang et al., 2008; Wang et al., 2009).

A related point on the data side is with respect to the NCEP/NCAR data used to force the model. Is this the NCEP-1 or NCEP-2 product? What are the limitations of this data, and to what extent might this introduce biases into the study here? What evaluations of "skill" of the surface windstresses with respect to the "regime shift" have been published, and does the NCEP product here tend to over-represent or under-represent the "regime shift" in the physical state of the atmosphere?

Response: This is a good question. We have clarified this in the paper (i.e., NCEP-I). Our team has done many model sensitivity studies, evaluating effects of wind forcing on physical, biological and chemical fields. We are aware of that NCEP-I winds tend to under-estimate interannual variability. Thus we believe that NCEP-I may under-represent the regime shift. We have addressed this issue in the revised manuscript.

Regarding the sedimentary source of iron, previously published studies have argued that sedimentary sources of iron can have a strong impact in modulating the decadal response of equatorial ecosystems. The authors will need to adequately reference previous published work on this subject, and furthermore explain the potential biases associated with the lack of sediment iron sources in their model. Would they expect an iron source to amplify or damp the reported signal? Again, this discussion would be most constructive if it were to occur within the context of discussion of observations.

Response: We have referenced some previous work on this subject. However, we are not aware of any observations showing that sedimentary source of iron plays a large role in the equatorial Pacific biogeochemistry. There are limited in situ dissolved iron data (some work reported total iron) in the literature, using different techniques (e.g., Gordon, Mackey and Measures) thus measuring different forms of iron. Recent measurements by Murray would provide insight. However, we are not aware of any publications of iron data. Nevertheless, we believe that an iron source would amplify the reported signal.

Further detailed issues/questions: - line 5 on page 2175: the isotherm depth does not only represent the intensity of upwelling, it also reflects non-local influences (Busalacchi and O'Brien studies with equatorial Kelvin waves, etc.)

Response: We agree so we have reworded as: the Z20 "largely represents the intensity of upwelling".

- how exactly is FeEA calculated?

Response: FeEA is the anomaly (relative to climatology) of entrainment rate of dissolved iron into the mixed layer. In the model, iron entrainment (FeE) is calculated as: $FeE=\Delta h1/\Delta t(Fe2-Fe1)$ ($\Delta h1>0$) , where h1 is MLD, and Fe1and Fe2 the iron concentrations in the mixed layer and below, respectively.

- What exactly is being argued about the relative roles of MLD and Z20 anomalies in driving biological production and plankton biomass? This point needs to be made more clearly

Response: This is a good point. We have revised accordingly.

The manuscript also suffers from a number of grammatical mistakes that the authors should correct before resubmission.

Response: We have read and checked the revised manuscript carefully.

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