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Interactive Comment

Interactive comment on "Spatio-temporal variability of marine primary and export production in three global coupled climate carbon cycle models" by B. Schneider et al.

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General comments:

This is a very interesting paper from which I learned a great deal. However, I disagree with the main conclusions of the paper, and I think that the main message of the paper is quite different from what the authors have made it out to be. Fortunately, it should be relatively straightforward to modify the paper for publication, as the fundamental analysis of the models is excellent. The problem lies, instead, with the interpretation of the analysis results.

First, let me summarize my disagreements. The third and fourth sentences of the



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abstract on lines 9 to 12, state the following:

"Results from all three coupled models confirm the role of the low-latitude, permanently stratified ocean for global PP anomalies. Two of the models also reproduce the inverse relationship between stratification (SST) and PP, especially in the equatorial Pacific."

In fact, I must agree with the first reviewer that these assertions stretch the truth considerably. The MPI model is so totally dominated by predator-prey oscillations (see Figure 10) that it is difficult to say anything meaningful about the causes of variability in that model. Furthermore, the variability in the NCAR model is extremely small (Figure 10), and the correlation with observed PP variability is much weaker and confined only to a small part of the permanently stratified ocean (see Figure 10, where only the Niño-3 results are shown, and also Figure 12). Only the IPSL model does an excellent job of reproducing the variability (see Figures 10, 11, and 12). Given these problems, I cannot agree with the final sentence of the abstract

"Our finding of a good agreement between independent estimates from coupled models and satellite observations provides increased confidence that such models can be used as a first basis to estimate the impact of future climate change on marine productivity and carbon export."

Indeed, my conclusion is exactly the opposite, definitely for the MPIM and NCAR models, and possibly even for the IPSL model, for reasons that I will go into next.

The key to understanding the results of this paper is to turn the main conclusions of the paper around and ask why all but the IPSL model do so poorly in reproducing the variability, and why does the IPSL model do so much better. The most likely reason for the interannual variability in the Behrenfeld et al. data and its strong correlation with the stability is that that the permanently stratified regions are limited by nutrient supply from below. So how well do the models do in reproducing this fundamental phenomenon? For me, the crux of this paper is in the combined information content of Figures 4 and 5. The only model that gets the 0 to 100 m phosphate in the stratified

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regions of the low latitudes even close to correct is the IPSL model (NOTE: the units in the figure are incorrect; they say umol m-3; I presume mmol m-3 or umol I-1 was intended). The MPIM model has phosphate concentrations that are too high by 0.5 to 1.0 mmol m-3, and the excess in the NCAR model climbs into the 1.0 to 1.5 mmol m-3 range in large areas of the Pacific. By no stretch of the imagination should these be considered reasonable errors, particularly if the aim of the model is to understand variability of PP and its causes in the permanently stratified regions. Examination of Figure 5 suggests where the problem lies: both the MPIM and NCAR models are iron limited. The nutrient concentration is thus allowed to climb well above the observed concentrations. By contrast, the IPSL model is nitrate limited, and thus it draws down the major nutrients to lower levels.

The iron cycle in the IPSL model is not fundamentally different from that in the NCAR and MPIM model, as far as I know. Why, then, is the IPSL model not iron limited in the stratified regions? As far as I am able to tell without a much more detailed investigation than I have time for, it appears that the iron cycle in the IPSL model has one significant feature that is rather different from other models, which is that it never permits the iron concentration to drop below 0.01 nmol I-1 (see top of page 1897; NOTE: in the original Aumont et al., 2003, paper which is given as the principal reference for the PISCES model, the iron was never permitted to drop below 0.02 nmol I-1). Given that the nanophytoplankton in this model have an iron half saturation constant of 0.02 nmol I-1 (per Aumont et al., 2003) this in effect means that the Michaelis-Menten iron limitation of nanophytoplankton is never permitted to drop below (0.1/(0.2 + 0.1) = 0.33) (0.5 in the Aumont et al. version of the model). I strongly suspect that this hard lower bound to iron limitation is why the IPSL model becomes nitrate rather than iron limited in the subtropical gyres. In order to confirm my inference, it would be useful to see a map of surface iron concentration and other analyses of the model results to find out how important the iron supply by this deus ex machina actually is. In Aumont et al. (2003), the contour intervals in their iron map begin at 0.05 nmol l-1, and only the annual mean is shown. The entire stratified region of the ocean is below this contour. Furthermore,

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their iron budget given in Figure 1 is for the world as a whole, not just the stratified region, and does not separately identify the deus ex machina source.

All else in the paper flows from the above. Since the MPIM and NCAR models are iron limited in the low latitudes, and since dust supply of iron is by far the dominant source in the stratified regions of the ocean (e.g., see Figure 9a of Aumont et al., 2003) and since dust supply is not linked to either the stratification or temperature in the ocean, it is no surprise that the response of biology to changes in stratification (i.e., to vertical nutrient supply) is so strongly suppressed in these two models. The MPIM and NCAR models have plenty of excess major nutrients already (Figure 4). It is only iron that they are lacking, and since dust input is the primary source for iron in the stratified regions, the changes in stratification and the impact of this on vertical exchange have only minimal impact on PP. (NOTE: the problem of predator-prey oscillations in the MPIM model shown in Figure 10 is unfortunate, and fixable). The IPSL model, being the only one that is truly macronutrient limited in the stratified regions, is the only one that responds strongly to changes in stratification. However, as noted above, it appears likely that the reason the IPSL model is not iron limited is because of the aforementioned deus ex machina. If so, we must then conclude that the IPSL model may be getting the "right" answer for the wrong reason.

My overall conclusion from this analysis is that getting the iron cycle right is crucial to predicting the interannual variability in the permanently stratified region, and the evidence reviewed above suggests that none of the models does this correctly. The IPSL model predicts the interannual variability in PP extremely well, but the fact that they found it necessary to insert an artificial generator of iron suggests that they may not have the correct mechanisms for doing this. The NCAR and MPIM models have iron limitation in the permanently stratified region, which allows the major nutrients to increase way beyond the observations, and greatly reduces the sensitivity of these models to changes in stratification and vertical exchange.

I have a second major point to make. The regressions in Figure 11 and Tables 1 and

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2, are all very interesting and nicely thought out. Somewhere around page 1899, one begins to get excited that maybe there is information in the interannual variability that can help us to predict the response to global warming, only to have our hopes dashed at the top of page 1900, where we find out that the response of the IPSL model to global warming over the rest of this century shows a completely different behavior than one would expect by extrapolating the interannual variability (wasn't the global warming response already revealed in Bopp et al., 2001, or is this a different model?). This is an extremely important conclusion. There are those who argue that the global warming response is strongly related to the interannual variability. If the authors have evidence to the contrary for the biological response, I would certainly like to see it published as soon as possible, and more ought to be said about it in this paper.

Specific suggestions:

(1) The abstract should be completely rewritten to emphasize the points made above. In its present form, I feel that it is rather inaccurate. The introduction will probably have to be refocused accordingly.

(2) p. 1880, lines 2 to 4, Matsumoto et al. (2004) showed most of the OCMIP models do not fit radiocarbon and CFC observations. The range of export production in acceptable models is much narrower.

(3) p. 1884, line 7, a minor point, but 20% for CaCO3 export is now thought to be too high.

(4) p. 1884, section 2.2.3, a list of tracers solved by NCAR would be useful. I was at first puzzled as to why the NCAR model is phosphate limited, but never nitrate limited in Figure 5. A note on the fact that nitrate is not included should be added to the caption to Figure 5.

(5) P. 1889, line 12, it looks to me like R is closer to 0.80.

(6) P. 1890, lines 18 to 22, I don't agree with this optimistic statement. Greater caution

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is warranted, and I think this study demonstrates a very strong reason why!

(7) P. 1891, lines 2 to 6, doesn't the Carr et al. paper have some measure of which of the PP models are better (I don't remember)? I think we have a better idea of what the PP is than the full range of 35 to 70 of all the models.

(8) P. 1891, lines 16-18, it is asserted that the modeled PP in coastal regions is the reason the models are so much lower than observed. I would like to see a demonstration of this. I suspect the open ocean accounts for most of the error.

(9) Pp. 1892-3, discussion of EP in models: something should be said about DOC export and its implications for the comparison of the models with "observations". Also, the references given here and in the introduction could perhaps usefully also include Dunne et al., Empirical and mechanistic models for the particle export ratio. Global Biogeochemical Cycles 19, GB4026, doi:4010.1029/2004GB002390 (2005).

(10) P. 1894, lines 24 to 27, this sentence is easily misinterpreted. The MPIM model is too contaminated by predator-prey oscillations to say anything meaningful. The other two models do seem to show the low latitude variability dominating, but given that the NCAR model has a greatly diminished range relative to the observations, I am not sure that one should be so firm in the assertion that the models have :exactly the same behaviour which was described by Behrenfeld et al. (2006)." Much more caution is warranted.

(11) P. 1895, near the bottom, why do you think the models have the opposite temperature dependence from what one expect based on Eppley (1972)? Clearly, temperature dependence of physiology is not the driving factor. I wonder if temperature is not just giving us the same information as stratification.

(12) P. 1898, lines 15-16, it is reasonable to expect that PP and EP should be positively correlated. Why are there some areas of negative correlation?

(13) P. 1899, lines 2-3, I disagree, as I hope my general comments make clear. Also,

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line 7, I don't see the results as being robust across all the models.

(14) Figure 9 is very interesting. Couldn't more be said about the disagreements between the two observational estimates and the models?

Jorge Sarmiento

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