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Biogeosciences Discussions

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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.

B. Schneider et al.

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General comments

First, we would like to thank both reviewers for their valuable comments that helped to clarify and improve the manuscript. We agree with both that the conclusion that all three models represent the observed climate-productivity interactions well over-stretches the results. To better distinguish between the models' capability to reproduce the observed climate and PP variability a more extensive discussion of the underlying iron cycling will be given, as this explains most of the model differences and caveats.

Response to the comments made by reviewer #1

EGU

Specific comments

1. ...the paper lacks focus.

   As the paper investigates next to the spatio-temporal variability of PP and EP the climate impact on both as represented by the models, the title was changed into :'Climate-induced variability of marine primary and export production in three global coupled climate carbon cycle models'.

2. The assertion that all three models represent the observed climate/pp variability very well stretches the truth considerably.

   This argument definitely needs clarification in the text. However, the first result is still true, PP anomalies from the global and the stratified ocean are indeed strongly correlated in all three models, however, in MPIM the amplitude and frequency is strongly overestimated, while in NCAR slightly underestimated. This has been added to the text (p. 19, l. 22-24).

3. What is the impact of temperature on PP in the models?

   The discussion in the revised text now explains that the major difference determining the models' capability to reproduce the observed climate/PP variability is getting the iron cycle right. Therefore, we added a lot of information on the modeled iron cycling in the text, so that a more in-depth discussion of temperature effects is not needed (p. 7, l. 1-4; p. 9, l. 19, p.20, l. 3-7, p. 21, l. 16-20; p. 21, l. 21 – p. 22, l. 24; p. 23, l. 3-10).

   The conclusion from this discussion is that two models (MPIM, NCAR) are strongly iron limited. On the one hand, this allows macronutrient concentrations to climb above observed values and, on the other hand, as iron is mainly supplied by dust deposition in those models, the response of these models to climate variability must be strongly suppressed.

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For IPSL we explain that there is an 'artificial' iron source by restoring the iron concentration to a minimum value of 0.01 nM l^{-1}. For example, we added (p. 21, l. 25 – p. 22, l. 10): 'This baseline concentration represents a non-accounted source of iron, which could arise from processes that are not explicitly taken into account in the model, like temperature or light effects on iron availability, iron released from ligands and dissolved or particulate matter, variable iron content in deposited dust, different rations of bio-available versus dissolved iron from recycling (e.g. micro- versus macrozooplankton), changes in phytoplankton size and/or physiology like half-saturation constants or iron demand. The iron restoring formulation allows to correctly represent the width of the equatorial tongue in chlorophyll and the location of the iron-to-nitrate limitation transition, thus yielding a better representation of nutrient co-limitations (Fig. 5). By doing so, the natural variability of iron is partly suppressed, dampening the signal that otherwise would be transferred into PP variability. Nevertheless, IPSL shows the best representation of interannual climate/PP variability both temporally (Fig. 10) and spatially (Fig. 12). This is due to the fact that next to the location of the iron-to-nitrate limitation transition the impact of ENSO variability on the supply of NO_3 is well reproduced.'

In the conclusions on page 25 we state: ' ... (this study) also highlights the importance of the modeled iron cycle on the impact of climate variability on marine productivity. Only one model (IPSL) is able to reproduce the observed relationship between climate (stratification, SST) and PP, and this result may to some extent be attributed to an artificial iron source.'

4. The recurrent mentioning of the uncertainties of satellite-based methods has been reduced and more details on the model uncertainties are given, instead (see iron discussion).

5. We agree with reviewer #1 that comparing future climate change with permanent El Nino conditions is overly simplifying, however, to describe climate-driven

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changes in surface ocean stratification, particularly in the low latitudes, it is an illustrative example.

Technical corrections

1. the word 'different' has been removed wherever unnecessary.
2. In the text it is stated that satellite algorithms and 3-D models are largely, but not completely independent. As mentioned by reviewer #1 satellite algorithms are diagnostic, while 3-D models are prognostic, so even if similar underlying assumptions for P-I curve or growth rates are used these methods are somehow independent.
3. typo
4. now published by Najjar et al., 2007.
5. we added (p.9, l. 23-25): '..., though the regenerated contribution is probably lower than in the real ocean as only the turnover of semi-labile dissolved organic matter (DOM) is considered.
6. typo
7. The interpretation of surface nutrient concentrations has been extended by adding information about the iron cycle, see also point 3 of specific comments (above). In terms of MLD_{max}, also the use of different definitions for MLD_{max} has shown that this variable is never very well reproduced by any model. This results from the fact that errors in temperature and salinity distributions probably add up, here, which is a common feature in coupled climate models (Schneider et al., 2007). Nevertheless, deviations between modeled and observed MLD_{max}

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are relatively low in the low latitude (equatorial) ocean, the area that is of major interest in our study.

8. see above, point 3 specific comments

9. The result of 75 GtC/yr PP from the PISCES model as given in Carr et al. (2006) is unrealistically high. Most probably, this value was taken from a preliminary model version that was never published. In Amount et al. (2003), which is cited in Carr et al. (2006), PP amounts to 43 GtC/yr. All published versions of the PISCES model yield annual mean PP values between 30 and 45 GtC/yr. We're sorry for this confusion.

10. This sentence now says (p. 19, l. 10-15): 'What is more, such anomalies are highly correlated with shifts in the climate system in a way that stronger stratification and the resulting surface ocean warming, which correspond to more El Nino-like conditions, result in negative PP and chlorophyll anomalies over much of the tropics and subtropics, because stronger stratification results in less nutrient supply from deep waters, which in turn limits phytoplankton growth.

11. The climate/PP variability relationship found for the low-latitude ocean has certainly also been tested on results from the high latitudes, separated by hemispheres. There is no such behaviour, most probably as here anomalies in PP and climate variability are out of phase, cancelling out each other when averaged over larger regions. This has been added to the text (p. 19, l. 27 – p. 20, l. 1): 'The latter can not be found for the high northern or southern latitudes, which highlights the dominant role of the low-latitude ocean in setting the global signal of PP variability.'

12. p. 13, l. 11-13: 'The fully coupled carbon-cycle climate models used in the current study generate their own internal climate variability including coupled ocean-atmosphere modes such as El Nino Southern Oscillation (ENSO).'

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p.20, l. 3-8: 'Note that in interpreting Fig. 10, one should focus on the magnitude and frequency of the PP variability, not the phasing of specific PP events. Since the models are fully coupled and thus have no real time information other than from CO_2 emissions, they each generate their own unique internal climate variability that can only statistically be compared with other models and observations.'

13. Statement has been removed.

14. This part is important to explain the iron cycle in the IPSL model.

15. As the paper is about climate impact on PP variability, which is now also indicated by the title, this topic is one of the central points in the study.

16. This statement has been changed: please see above point 3 specific comments.

17. The results cited here refer to global changes, mainly based on temperature effects. In our study nutrient limitation due to stratification (as a response to temperature change) is also taken into account and only the low-latitude permanently stratified ocean is regarded. Certainly, when looking at the global impact one has to consider the high latitudes, which will probably show an increase in productivity due to sea-ice retreat and extension of the growing season, which has been shown by Bopp et al. (2001).

18. Please see above: point 3 specific comments.

19. Changed into (p. 25, l. 2-4): 'The current study has illustrated a strong link between marine productivity and climate variability in coupled climate carbon cycle models, which has already been observed from satellite records (Behrenfeld et al., 2006).'

20. Has been included in the caption of Figure 2.

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21. We’re sorry for the small size of many figures, which most probably will be due to the specific web-layout.
22. Replaced.
23. Figure caption changed.
24. The different areas have been taken into account when computing budgets, regressions, etc., for the map projections this is of minor importance.

Interactive comment on Biogeosciences Discuss., 4, 1877, 2007.