Biogeosciences Discuss., 9, C8095–C8100, 2013
www.biogeosciences-discuss.net/9/C8095/2013/

Biogeosciences
Discussions

Interactive comment on "Phytoplankton competition during the spring bloom in four Plankton Functional Type Models" *by* T. Hashioka et al.

Anonymous Referee #2

Received and published: 10 February 2013

Interactive comment on Biogeosciences submission bg-2012-570: "Phytoplankton competition during the spring bloom in four Plankton Functional Type Models" by T. Hashioka et al.

General Comments

This manuscript examines the factors influencing the representation of the seasonal phytoplankton bloom in four Plankton Functional Type (PFT) models, each coupled to a different ocean general circulation model. More specifically, the authors attempt to attribute the relative dominance of diatoms over nanophytoplankton during blooms to differential bottom-up (nutrient limitation) or top-down (grazing by zooplankton) effects.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Specific Comments

I commend the authors for undertaking a worthy investigation, and I am satisfied with their description of the motivation for this research (lines 18085: 22-28), that is, understanding the key processes which control bloom dynamics is critical if the community is to interpret how the phytoplanktonic community will respond to long-term global change. Models with reasonable empirical representation of the current phytoplanktonic community should not be interpreted as appropriately representing the underlying dynamics. Whether or not the top-down or bottom-up factors are dominant under the current range of environmental conditions, though, may be irrelevant under future conditions which might differ from conditions observed in the current ocean. Nevertheless, it is valuable to better understand top-down and bottom-up processes in the current ocean. Similar research which investigates the processes which control dynamics should certainly be encouraged.

I have a few key areas of concern about the authors' current approach to investigating these dynamics which I mention briefly below. These overarching concerns will then be followed by a few more minor, specific topics which should also be addressed.

The competitive advantages of different planktonic functional types could be presented more conceptually in the introduction. Although this is largely a model intercomparison project, description of the conventional, ecological hypotheses underlying the modeled responses is warranted. That is, smaller algae tend to be better competitors for light and nutrients (given their larger surface area to volume ratios), but these phytoplankton are more susceptible to microzooplankton grazing. Meso and macrozooplankton are capable of grazing on larger phytoplankton (e.g., diatoms), but these larger mesozooplankton tend to have slower population growth rates relative to the rapidly responding microzooplankton. Meso and macrozooplankton often progress through discrete life stages before reaching adult size, inhibiting their ability to control the biomass accumulation of diatoms during a bloom. There are a number of references that discuss this theory (e.g., Riegman et al. 1993; Calbet and Landry, 2004; Sarthou et al. 2005).

Additionally, although the authors do a fine job of noting the importance of sinking loss in the introduction (pages 18087 and 18088), this process is overlooked in the analyses which follow. Does sinking contribute to phytoplankton loss in these models, and how does the magnitude of this loss compare to that due to grazing?

Another important comment I have concerns the reliability of estimating the relative photosynthesis ratio and inferring the contribution of different limiting factors from the monthly averaged concentrations of tracers. For example, fluxes of nutrients into the phytoplankton could be high while concentrations of nutrients in the euphotic zone may be low. How well do the authors' estimates of photosynthetic rates compare to the models' photosynthetic rates (page 18093)? Presentation of the accuracy of these approximations of model conditions would be valuable.

Describing of the time evolution of the bloom would also permit readers to better understand how the derived estimates of controlling factors structure community composition and bloom dynamics. Highlighting a complete month and attempting to derive a metric of limiting factors strikes me as very difficult given the episodic nature of factors controlling bloom development in temperate oceans (Sverdrup 1953). Is it possible for the authors to demonstrate the evolution of these limiting factors with use of higher-temporal resolution data before relying on the monthly averages? Temporal change in the importance of controlling factors is mentioned for the NEMURO model (lines 18105: 8-11), but this dynamic is not displayed in the current manuscript.

On a similar point, estimation of plankton phenology in the models was not well described, nor was the atmospheric forcing. The authors should provide more information regarding model initial conditions, model spin-up, and atmospheric forcing (lines 18090: 16-18). I am assuming that the NCEP/NCAR "data" used was time variable (1996-2007) rather than averaged into a standard climatology. If so, there is likely variability in bloom timing. Was the time of blooms (and the controlling factors examined) constant through time, or did this shift slightly from year to year?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Limiting spatial coverage to high-latitude systems may unnecessarily complicate interpretation of bottom-up and top-down control in these PFTs. By exploring a wider range of environmental conditions (and so including the subtropical ocean ecosystems) may allow more complete comparison between top-down and bottom-up processes in the ocean. The justification for limiting the analysis to the high-latitude regions was unclear to me.

I found it difficult to judge differences among the biological models when variability in the nutrient dynamics are not noted. Is it possible to attribute changes in model output to differences in PFT structure without also examining changes in the larger biogeochemical properties? Testing the response of the PFT models to a standard range of environment conditions may be more enlightening than the current approach in which the relive importance of PFT, biogeochemical, and circulation models is challenging.

Another concern I have is the reliance on model-model comparison when discussing the percentage of diatoms in a bloom. The disagreement between the two “observational” datasets of Hirata et al. (2011) and Alvain et al. (2008) appear to be greater than that among models. I would recommend attempting to use real observations from focused time series (perhaps similar to the approach of Saba et al. 2011) rather than relying on statistical models.

Finally, I was somewhat disappointed by the lack of critical discussion in the manuscript. Although I recognize that the current manuscript has been an immense amount of work, it ends rather abruptly after identifying differences among the models. What is the way forward? What is necessary to resolve the differences among the models? What types of data should observationalists be focusing on? Does sinking play an important role that needs to be addressed? As noted above, phenology and life-history development of mesozooplankton is thought to play a key role in the competitive advantage of diatoms over nanophytoplankton. The authors describe that the NEMURO model is the only PFT model they examined which includes some representation of zooplankton life history. Did the authors feel that characteristic was necessary?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Technical Comments:

Figure 5 is not discussed to an extant warranting inclusion in the manuscript.

Figure 6 is suggested to indicate the “strength of grazing pressure on phytoplankton.” This interpretation was unclear to me. If zooplankton-to-chlorophyll concentrations are relatively high, does that mean that there is a large amount of grazing pressure because of the abundance of zooplankton, or does that mean that grazing pressure is low because of the lack of clear bottom-up limitation to zooplankton biomass? Differences were attributed to “different ecosystem structure,” but the interpretation of the figure was unclear.

The comment on page 18095, lines 15-20 should perhaps be attributed to the circulation or atmospheric models used. Here, without discussion, I feel as if reader will assume that the lack of spatial features is a fault of the PFT models. That is unlikely to be the case given the resolution of the physical models.

Overall, the manuscript is well-written. There are about a dozen grammatical errors dispersed throughout the manuscript, but I will not waste time identifying each now. Check for subject-verb agreement and remember that zooplankton and phytoplankton are already plural. The first error I found on this specific rule was on page 18089, line 20: “mesozooplankton mainly represents a type of copepods.” This should read “mesozooplankton mainly represent a type of copepod.”

Calbet, A., and M. R. Landry (2004), Phytoplankton growth, microzooplankton grazing, and carbon cycling in marine systems, Limnol. Oceanogr., 49(1), 51-57.

Riegman, R., B. R. Kuipers, A. A. M. Noordeloos, and H. J. Witte (1993), Size-differential control of phytoplankton and the structure of plankton communities., Neth. J. Sea Res., 31(3), 255-265.

Saba, V.S., M.A.M. Friedrichs, and the PPARR team, 2011. An evaluation of ocean color model estimates of marine primary productivity in coastal and pelagic regions

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

across the globe. Biogeosciences, 8, 489-503, doi: 10.5194/bg-8-489-2011.

Sarthou, G., K. R. Timmermans, S. Blain, and P. Treguer (2005), Growth physiology and fate of diatoms in the ocean: a review, J. Sea Res., 53(1-2), 25-42.

Sverdrup, H. U. (1953), On conditions for the vernal blooming of phytoplankton, Journal du Conseil International pour l'Exploration de la Mer 18: 287–295. doi:10.1093/icesjms/18.3.287

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C8095/2013/bgd-9-C8095-2013-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., 9, 18083, 2012.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper