Biogeosciences Discuss., 8, C4640–C4643, 2011
www.biogeosciences-discuss.net/8/C4640/2011/

Biogeosciences Discussions

Interactive comment on "Temperature thresholds for Arctic plankton community metabolism: an experimental assessment" *by* J. M. Holding et al.

Anonymous Referee #1

Received and published: 1 December 2011

The paper "Temperature thresholds for Arctic plankton community metabolism: an experimental assessment" by J. M. Holding and colleagues describes a set of mesocosm experiments where the community metabolism of two different Arctic populations was assessed at a variety of temperatures up to 10 degrees C. During these 15-day experiments, water samples were periodically removed for assessment of chlorophyll concentration, gross primary production (GPP), community respiration (CR), and net community production (NCP). GPP, CR, and NPP were assessed from changes in oxygen concentration in light and dark bottles.

General Comments

I think that it is important to understand how the plankton communities in high latitude waters are likely to respond to increases in water temperature and mesocosm

experiments are a good way assess these responses. I thought that the use of multiple plankton populations was a good approach, particularly since the two populations had different thermal histories. However, I am very concerned that the authors didn't measure a sufficient number of parameters associated with the plankton community to unambiguously evaluate the community response to temperature. For example, the authors don't provide any information about nutrient concentrations. There is no mention of nutrient additions so I assume that the populations had to exist on whatever nutrients were available at the time of collection. Given a 15-day incubation period, it is very likely that nutrients in some of the treatments may have been exhausted. Is it possible that this might explain the reduction in chlorophyll in the 9 and 10 degree treatments? We don't know because nutrient data were not presented. In addition, phytoplankton biomass was only characterized using measurements of chlorophyll and heterotrophic biomass wasn't characterized at all. Because chlorophyll/cell can change under different light regimes (independent of temperature), it is important to measure phytoplankton cell number and the light levels that the community experienced at the start of the experiment and the light levels produced by the fluorescent lights used in the incubations. This is the only way to know whether changes in chlorophyll are due to photoacclimation by the phytoplankton community during a shift to a new light regime or to changes in phytoplankton abundance.

Because these critical measurements were either not made or were not presented, it is difficult to interpret the chlorophyll-normalized values for community metabolism – and these are at the heart of the paper.

Specific Comments

1) In the Introduction, the authors ignore the impact of cooling of northward-advecting waters and how this would impact air-sea CO2 exchange. The CO2 sink is driven in part by biological drawdown of CO2, but also by the fact that cooling waters have a lowered pCO2 and facilitate greater air-sea exchange. This should be mentioned in the paper.

2) None of the oxygen data used to calculate community metabolism are presented. How often during a 24-hour period was O2 measured? And the authors never describe how GPP, CR, and NPP are actually calculated. A diagram showing the experimental set-up would also be helpful. It was hard to envision from the text alone.

3) Because the paper is based on the balance between heterotrophic and autotrophic processes, it would be highly desirable to characterize the heterotrophic and autotrophic communities. Only chlorophyll was measured, but chlorophyll is an insufficient quantity to use as a normalization parameter for characterizing community responses. Most of the heterotrophic processes will be from non-phytoplankton and it is not clear how relevant a community respiration term normalized by chlorophyll is. Particulate organic carbon would have been a much better choice for normalization parameter.

4) The authors stated that, "When measured initially, the replicates of the Barents Sea plankton community samples were different, with one replicate acting strongly heterotrophic and the other acting autotrophic". However, somehow after that initial period, both replicates behaved the same over time. How was that possible? Something must have shifted during the initial phase of the experiment and it is important to know what it was.

5) On page 11293, the authors state that GPP is independent of temperature. However, Figure 4b appears to show that GPP is low at the lowest temperature but then increases dramatically to its peak at about 3 degrees and then declines steadily with temperature thereafter. There certainly appears to be a possible relationship to me. How was the p value of 0.50 for the relationship between GPP and T determined? Using a linear model? Clearly a non-linear model would fit these data much better.

6) For Figure 7, the authors state that there is no significant relationship between chlorophyll and temperature, yet there is a curved line drawn through the data giving the impression that chlorophyll peaks at some intermediate temperature. This line should be removed if no significant trend exists.

7) I don't think that these experiments provide sufficient evidence to conclude that the Arctic will become net heterotrophic when water temperatures reach 5 degrees. It will depend on whether the Arctic Ocean becomes more productive in the future.

Technical Comments

Page 11286 Line 25. This sentence makes it sound like CO2 is capable of sinking. I think the meant to say the high capability of the Arctic Ocean to act as a CO2 sink.

Page 11292 Line 13. Change forth to fourth.

Page 11293 Line 5. Temperature was left off the slope unit – it is a change in chlorophyll per degree change in temperature. Line 8. Change adapted to acclimated. Line 16. Change patters to patterns

Page 11301. The first four values for GPP/CR in Table 1 are wrong (4.87/8.50 does not equal 11.58).

Page 11302. Either the volumetric or the specific NCP for the t0 has a wrong sign. One can't be positive while the other is negative.

Interactive comment on Biogeosciences Discuss., 8, 11285, 2011.