Biogeosciences Discuss., 9, C1318–C1321, 2012
www.biogeosciences-discuss.net/9/C1318/2012/

Biogeosciences
Discussions

Interactive comment on "Enhanced carbon overconsumption in response to increasing temperatures during a mesocosm experiment" *by* J. Taucher et al.

D. Hutchins (Referee)

dahutch@usc.edu

Received and published: 20 May 2012

This paper focuses on a mesocosm experiment examining the biogeochemical responses of a Baltic Sea phytoplankton community incubated at three temperatures bracketing the in situ values. The questions addressed are important ones, and the findings are significant in terms of the possible effects of future warming on estuarine carbon and nutrient cycling. The authors have put together a well-written manuscript and are careful not to over-interpret their data, making this a valuable contribution to the literature on global change effects on phytoplankton-driven biogeochemistry. The paper does have a few outstanding issues that should be addressed in a revision, but it

should ultimately make a strong contribution to Biogeosciences. The biggest problem I have with the paper is the way that gas exchange was calculated in the mesocosms is far from clear. They apparently set wind speed in their calculations to 6 m s-1, even though actual wind speed in their indoor experiments was near zero, to account for the artificial mixing of the water column with paddles in the mesocosms. This may be a reasonable approach, but these calculations should be shown in detail to help justify this unconventional "fudge factor". The authors also need to be cautious about using circular reasoning here, since this manipulation of the equation is justified on the basis that it allows balancing of the carbon budget, while later in the paper the carbon budget is used to discuss the implications for gas fluxes. This is not a fatal flaw, but the authors need to be as up front as possible about these gas exchange calculations and the inherent assumptions and limitations. The best way to do this would be to show the calculations themselves. Another question I have is about the fate of the missing N, since they acknowledge that they were unable to balance the N budget, with about 8.4 umol L-1 unaccounted for. They don't seem to believe it ended up on the bottom of the tanks, as they were well stirred, although I note there will always be a stagnant boundary layer of some magnitude at the bottom of such an enclosure, and it doesn't seem that it would be too difficult to sample this with well-designed miniature sediment traps or even by vacuuming the bottom with a peristaltic pump. I suggest though that another possible loss term for N might have been degassing of regenerated NH4+ from these well mixed systems, since it is quite volatile. Although measured ammonium concentrations were reasonably low (0.2-0.6 uM), this standing stock measurement certainly doesn't preclude a rapidly turning over flux to the atmosphere. The batch design of these experiments lends itself to obtaining good mass balances, but tends to bias their results towards the stationary phase part of the growth curve. This was especially evident in this experiment, since nutrients were exhausted within 5 days but sampling was carried out to 30 days. It seems therefore that much of the carbon overconsumption they observed was a function of severely nutrient limited growth. High C:N ratios in stationary phase phytoplankton have been recognized for a long time, but the effect of high temperature on further increasing these ratios is new. We have also examined temperature effects on C:N ratios in a North Atlantic Bloom diatom/coccolithophore community (Feng et al. 2009 MEPS 388) and a Bering Sea diatom community (Hare et al. 2007 MEPS 352) maintained in steady-state exponential growth using our shipboard continuous culture systems, and in a cultured coccolithophore (Feng et al. 2008, TEJP 43) and several cyanobacteria (Hutchins et al. 2007 L&O 52, Fu et al. 2007 J. Phycol. 43) growing in exponential steady-state cultures. In none of these cases with exponentially growing cells did we see any effect of temperature on C:N ratios (although the Feng et al. 2009 paper shows fairly large warming-mediated increases in POC: Chl a ratios), and the authors also note in the discussion that their results are different than those of several other studies that looked for a temperature effect on elemental ratios. They attribute their results possibly to the specific diatom species that dominated in their mesocosms, but does this mean that the implications of their results are limited only to the declining and senescent phase of blooms? Some discussion of this issue is warranted. Because of this emphasis on the end of the bloom and elemental ratios in senescent and dying cells, bacterial influences on elemental ratios would have become increasingly important in the last half of their experiment. It is therefore unfortunate that no measurements of bacterial production or abundance were undertaken, as these may have been quite informative. The other obviously missing parameters are any measurements of particulate P or Si (as this was a diatom bloom, after all), preventing an examination of temperature effects on full Redfield ratios and on mass balances of these two other important and potentially limiting nutrients. A minor issue is that the role and fate of the added copepods (10 Acartia per L) is given rather short shrift in the text, which states that they were observed for the first half of the experiment, but their fate is not discussed further. I agree with their assertion that copepod grazing and carbon mass would not have been enough to significantly affect the final mass balances of elements, although the addition of 8.3 uM copepod carbon (assuming 5 ug/animal) to the initial 24-30 uM POC is certainly a significant perturbation of the initial carbon budget. Another subject that is not addressed at all

is the implications of the low temperature treatment results for past cooler times, such as glacial periods. If their results imply higher C consumption relative to N used in the future warmer ocean, does this also mean that during the LGM C:N consumption would have been significantly lower than today? Although they did a great deal of work to include this low temperature treatment, they don't discuss or use its results much except as a comparator for the two higher temperatures. All in all, these issues I have raised do not outweigh the considerable value of the paper to the global change field, and it should definitely be published after suitable revisions.

Interactive comment on Biogeosciences Discuss., 9, 3479, 2012.