Biogeosciences Discuss., 10, C6186–C6188, 2013
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Biogeosciences
Discussions
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10, C6186–C6188, 2013

Interactive comment on "Temperature and phytoplankton cell size regulate carbon uptake and carbon overconsumption in the ocean" *by* S. E. Craig et al.

Anonymous Referee #3

Received and published: 29 October 2013

I regret that I do not think that this paper merits publication. There are two major problems.

Firstly, the title refers to "carbon overconsumption" and a major conclusion of the paper is that this overconsumption is due to small phytoplankton cells. However, it is not clear how these conclusions are reached. It it seems to flow from assumptions that relate to low nitrate concentrations in the surface mixed layer (p13, lines 1-17). Use of a different approach would lead to different conclusions. The method used in this paper is to calculate carbon content of all phytoplankton size classes using literature values for cellular carbon content. If the same literature is used to calculate the NITROGEN content of phytoplankton size classes, then any calculated carbon:nitrogen ratios would be identical to the literature values used to establish the C and N cell content. A different conclusion is then reached because of the methodology adopted. If the conclusions drawn by the authors are to be supported, then their alternative methodology must be fully explained and justified. But it seems to me that, if it is acceptable to use literature values for carbon content, then it must be acceptable to use literature values for nitrogen content. Use of such N-content and C-content estimates then would not demonstrate any carbon overconsumption. Therefore, the conclusions drawn in this paper must be false. The discussion is also very superficial and does not draw on the large body of information that exists on new production and f-ratio, regenerated N and rates of recycling.

Secondly, the methodology used to determine net community production (NCP) has resulted in totally implausible values. An estimate of 90.47 mol C m-2 y-1 for annual depth-integrated production (p9, line 16) is equivalent to 1085 gC m-2 y-1 – about 10 times the value that would be expected for a temperate coastal ocean at this latitude! I was not convinced that the method is justifiable. The assumptions used must be explicitly described and clarified. It may be possible to estimate NCP by subtracting a biomass estimate for one month from the subsequent month, but only if the same water mass is followed in a Lagrangian experiment and if there is a robust estimate of dispersion. This is not the case in this study, which samples the same station (defined by latitude and longitude, not phytoplankton population) and does not account for different phytoplankton assemblages in different water masses.

It appears that NCP was calculated only using positive values (P9, lines 6, 9, and 16); that is, when there was an increase in biomass from one month to the next, not a decrease. So there has been selection of a sub-set of the data. And what about the production that occurs even when standing stock is declining? But the greatest problems appear when depth-integrated production is estimated. When primary production is estimated by the 14C method, or by oxygen titration, then depth-integrated production calculations take into account light attenuation through the water column.

This is not done here and NCP appears to be extrapolated from a value for the surface m3, to the whole water column (per m2). Superficially, it might seem that a change in biomass per unit time should provide a good estimate of production, but only if all of the uncertainties associated with the estimates are quantified. In this study they are not, and those uncertainties are likely to be large. Cell counts by microscopy are notoriously imprecise (I assume that is how diatom and dinoflagellate numbers were determined, but it is not explained): data from Bedford Basin are used as a proxy for the station HL2 (so 2 different populations were sampled at different frequencies): the method assumes that the same phytoplankton assemblage has been sampled – it has not: there is a high reliance on climatological mean values to reveal features that are not apparent in the data for individual years (p11, line 2).

So I cannot support publication of this paper because the data do not justify the conclusions. Carbon overconsumption has not been demonstrated. The estimates of depth-integrated NCP are dubious, so the conclusions about the assemblage being "uncoupled from the Chl a standing stock" cannot be supported. And the speculation about a future ocean, and the consequences of higher temperature are not supported by data.

Interactive comment on Biogeosciences Discuss., 10, 11255, 2013.