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Biogeosciences Discussions

Interactive comment on "Rapid carbon cycling in the oligotrophic ocean" *by* C. M. Duarte and S. Agustí

Anonymous Referee #1

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This study examined the release over time of dissolved 14C during standard 14CO2 primary production measurements in several oceanic regions visited over the years. The authors relate this release to a measure of phytoplankton lysis and then analyzed their results using a simple model of carbon flow between phytoplankton, DOC, and bacteria. The writing style of the paper is very compelling and engaging, and the topic the authors address is important.

But the paper has lots of problems. It is very difficult to see what is actually new about the study, other than the interesting observation about a positive correlation between lysis and extracellular release. As the authors acknowledge, extracellular release has been examined extensively (albeit not recently), starting with studies published back in the 1970s. Likewise, several studies have examined the dependence of 14C primary

production estimates on incubation times. The authors mention briefly some of these previous studies, but they don't mention John Marra's work on the topic. A couple of his papers are cited below. (Overall, the reference list of this paper is very short, especially after the authors' papers are subtracted out.) The authors have to identify which specific problem has not been adequately examined before and how they will shed new light on it.

Another overall problem with the paper is that too often it is hard to understand what is going on. Specific examples are discussed below. One big one is that the authors talk about DOC, TOC, and particulate carbon, but in fact the authors have measured none of these things. They have data "only" about 14C. So, their terms and language need to reflect that, as discussed below in more detail.

Specific comments

P11662, bottom: In this short review in the Introduction, the authors don't say exactly what they mean by "high" bacterial carbon demand; they do not give any percentages or ratios relative to primary production. Models may or may not need to have a large fraction of primary production released into the DOC pool, depending on what "high" is. Models by Nagata (2000) and Anderson and Ducklow (2001) (none all models in Anderson and Ducklow) assume that only 10% of primary production is excreted, yet in these models bacteria processing a "high" fraction of primary production, supported by DOC release by other components of the food web.

The importance of this paper does not rest on the reader believing that bacterial carbon demand is really high, perhaps unreasonably high. Understanding extracellular release would be important regardless of whether the emperor has clothes or not.

Somewhere here, the authors should cite Fouilland and Mostajir (2010), one of the most recent data synthesis studies of bacterial carbon demand and primary production. Also, Williams (1990) is still one of the most complete reviews on phytoplankton excretion.

P11665, line 24: What was the level of 14C-DOC in the original 14CO2 stocks?

Since the authors criticize a bit the "traditional 14C method", they need to assure readers that they minimized all possible artifacts which have been identified over the years. Another one that comes to mind is trace metal contamination. It can both inhibit and stimulate 14CO2 uptake.

P11666, line 10: What kind of "membrane" filters? Polycarbonate, nitrocellulose, mixed esters of cellulose or what? Actually, the text already gives probably the most important detail: that the pore size of these filters is 0.22 um. This is a significant difference from the vast majority of 14CO2 primary production studies which use GF/F filters. This is worth pointing out and discussed briefly.

P11666, line 22: The description of the methods for assessing phytoplankton lysis, which is now buried in the middle of this paragraph, should be put into its own paragraph.

P11667, line 10: The left side of the equation is incorrect; the volume units (uL) should not be in it. Better would be to either say "specific lysis rate" (or something like that) or give a symbol for this parameter in the equation.

More importantly, this equation would be more useful if it explicitly included the actual measured parameter, the dissolved esterase activity.

P11667, line 20: "POC production" is not accurate and misleading here. A more precise, informative, and commonly used phrase is "particulate primary production" or even better "particulate 14C-primary production".

P11667, line 23: The authors report a negative correlation here between phytoplankton lysis rates and phytoplankton biomass. First, they give an r^2, but r should be reported because this is a correlation problem, not a regression problem; note that r^2 will always be positive even for negative correlations.

More importantly, the analysis is suspect because chlorophyll is used for calculating

both lysis rates and phytoplankton biomass, meaning that the relationship between the two may be an artifact of the analysis; one cannot compare X and Y when B is used to calculate both X and Y. The analysis would be much more convincing and statistically valid if the authors analyzed the original dissolved esterase activity versus chlorophyll.

p11668, line 4 and elsewhere: The terms "TOC" and "POC" are inaccurate and very misleading. The authors did not measure TOC or POC or changes in these two pools. They measured the movement of 14C, so their terms should reflect that.

p11671: This section is very difficult to wade through and understand. It may help to put the results of the modeling efforts in the Results section and to separate description of the model results from their interpretation and discussion. A table or two could be used to summarize what combinations of parameters were tested. There are discrepancies among the text, Table 2 and Figure 6. Table 2 does not summarize all values that were examined and tested.

P11673, line 10: The authors here say that "cycling of carbon in the microbial food web occurs at a characteristic time scale of 10–15 min". First, it is not clear where "10-15 min" comes from. But more important, if the authors mean all carbon, this is very hard to believe and is inconsistent with virtually everything known about microbes and carbon cycling in the oceans. Perhaps some free amino acids cycle on this time scale, but certainly not the entire pool of carbon. That implies incredibly high growth rates or extremely low growth efficiencies or some combination of both.

p11674, bottom: The authors have to mention viruses as one possible mechanism accounting for the observed lysis. It doesn't change their argument. But more troublesome, they really can't discount grazing on phytoplankton, or even that some of the esterase activity may come from bacteria or other organisms.

Table 2: The information in this table should be combined somehow with Figure 5 or 6. It is likely that Table 2 and the figures will not be on the same page (or computer screen), making it difficult for readers to go back and forth between them.

Some of the values here are very extreme and are not the same as those actually discussed in the main text. The extreme ones include the assumption that percent extracellular release is 90% for all panels where in fact the caption for Figure 6 says 40%. Table 2 gives a bacterial growth efficiency (BGE) of only 2%, but it seems others were tested. Although BGE can be as low as 2%, the grand average for the oceans is closer to 10%. What happens when that value is assumed?

Table 3: The time frame for these slopes is not clear. Are they the slopes of the initial part of the time course experiments? If the time courses were divided up, then the actual time frame should be given.

The number of time points for each experiment should be given. If the same for all, the number can be put into the table caption. The column with "Experiment" does not give any information (readers can simply count the lines of the data to deduce this) and should be deleted. Rather than the slope for "TOC" (again, the wrong term), the slope for DOC should be given. Errors on all slopes should be given. The location of the column with p-values implies that they apply to only the TOC data. Why is that? If errors are given, then the p-value column isn't as necessary. And now the values (only 0 or 1??) look truncated.

Figure 1: The orientation of this figure is weird, with lysis rate constants on the y-axis (and the label orientated the wrong way). The orientation gives the impression of a depth profile, which isn't what the data are about. The lysis rate constants should be on the x-axis, as in Figure 4. The difference between Figures 1 and 4 (the same type of data are plotted) is not clear.

Figure 2: The authors should give the dissolved 14C data, in place of the total 14C (dissolved and particulate data).

Also, the time zero values are not given, and the methods don't say anything about time zero. Were time zeros measured? Or they assumed to be background?

Figure 3: This figure is hard to understand, mostly because the y-axis label is inconsistent with the figure caption.

Also, the micron sign is missing from the axes labels.

References

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Nagata, T. 2000. Production mechanisms of dissolved organic matter. Pages 121-152 in D. L. Kirchman, editor. Microbial Ecology of the Oceans. Wiley-Liss, New York.

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