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

Anonymous Referee #3

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This paper studies the dynamics of total and particulate 14C primary production in oligotrophic and eutrophic pelagic systems, from which rates of release and bacterial use of dissolved organic carbon are inferred. Differences in these rates, and in cell lysis estimates, are interpreted as evidence that productive communities are able to accumulate organic carbon over hourly time scales, but most organic carbon fixed photosynthetically in oligotrophic communities is rapidly (< 15 min) lost to the DOC pool via cell lysis, and respired by bacteria. Discussion of this idea lead the authors to the far-reaching conclusions that conventional assessments of primary production in the oligotrophic ocean are severely underestimated, thus explaining discrepancies between primary production and bacterial carbon demand in the oligotrophic ocean. Although these are important issues, I have some difficulties with the logic and data sustaining the conclusions in the manuscript.

The conceptual model of the cycling of carbon in the microbial food web used (as presented in figure 5) is incomplete, which together with the assumption of steady-state leads to misleading conclusions. Firstly, microzooplankton may graze on both phytoplankton and bacteria and respiration by auto- and heterotrophic eukaryotes are significant loss processes. This means that the assumption that the loss of accumulated TOC produced must derive from respiratory losses mediated by bacteria (p.11669) is not correct. Secondly and most important, local or allochthonous DOC (and not only recently produced DO14C) is an important contribution to the heterotrophic respiration in oligotrophic oceans (e.g., Duarte and Agustí 1998, del Giorgio and Duarte 2002). This implies that bacterial carbon use and respiration cannot be calculated from a steady-state model that only includes instantaneous primary production as organic matter source.

From the large difference between total and particulate 14C primary production after short incubations in oligotrophic habitats, and the rapid loss of only total 14C PP (Fig.2), the paper concludes a very high DOC release and rapid respiration by bacteria. According to the authors, such a high DOC release can only be accounted for by an important cell lysis (L15, p.11670), which does not occur with healthy cells (L10, p.11670). At the same time, the extremely high 14C primary production rates after 15 minutes incubations are interpreted as representative of the high rates of photosynthesis in the oligotrophic ocean, previously undetectable by conventional methods. I find it difficult to reconcile the required prevalence in the phytoplankton of cells that are dead or compromised (L6, p.11670) on the one hand, with such a high photosynthetic activity on the other.

In addition, the conclusion that the extremely high rates of 14C primary production measured after 15 minutes incubations are representative of the oligotrophic ocean, would demand an explanation to a new suitable mechanism supplying the required large amount of inorganic nutrients to the surface of the stratified open ocean. Calculations of nutrient supply mechanisms to the upper oligotrophic ocean, including nitrogen

BGD

8, C5653–C5656, 2012

fixation, diffusive transport and vertical entrainment, are insufficient to support even standard primary production estimations (Johnson et al. 2010). Given the magnitude of the proposed new high flux of carbon in the oligotrophic ocean, a discussion on this issue is necessary.âĂĺ

Such high primary production rates also contravene published independent evidence. Given that the paper sustains that conventional rates of 14C primary production in oligotrophic waters are severely underestimated, we need to compare the proposed GPP rates (after 15 min 14C incubations) with direct GPP measurements from changes in O2 concentration after incubations. In the N Atlantic subtropical Gyre, the range and mean for O2GPP rates in the dataset at www.amt-uk.org/data/respiration.xls, are 10-201 and 69 mmol O2 m-2 d-1, respectively (Gist et al. 2009). Assuming 100 m of photic depth and a PQ of 1, these data become 1.2 - 24.1 mgC m-3 d-1 and a mean of 8.3 mgC m-3 d-1, respectively. The surface 14C PP data presented here (after 15 minutes incubation) are ca. 27 and 10 mgC m-3 15min-1 (Fig.2), that is, assuming 10 hours light, they are 1080 and 400 mgC m-3 d-1. This is 17 to 45 times larger than the highest value in the range of Gist et al (2009), and a discrepancy > two orders of magnitude with the mean published evidence based on a large database. These extremely high data would require extremely solid evidence and a very solid justification.

And this is a critical issue in the manuscript, because the entire discussion rests on these data: time course data in Figure 2, support both the high GPP rates and the inference of high phytoplankton cell lysis and bacterial uptake and respiration. Yet the paper does not provide any argument supporting the possibility of such high GPP data in the upper oligotrophic ocean. And moreover, I have some difficulties not only with the magnitude but also to asses the validity of the data themselves. According to the Methods (p.11665) and Table 3, 20 time course experiments were carried out. However, only 6 out of these 20 time courses are presented in the key Figure 2. Why? Also according to the Methods, 2 dark and 2 light bottles were incubated, which is a very limited number of replicates that may compromise any statistical test of differences. And

BGD

8, C5653–C5656, 2012

yet, data in figure 2 are presented without either their corresponding standard deviations or standard errors. Altogether, this means that the patterns sustaining the entire discussion rest on one (Fig.2.a) or at best 2 (Figs.2.b and 2.c) extremely improbable high data points based on just two replicates and whose variance we ignore, from 6 selected experiments out of 20 performed.

In my opinion, resolving these issues is necessary before we can start a critical debate about the ecological an biogeochemical implications of the observations and conclusions presented in the manuscript.

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