

RESPONSE to REVIEWER #3 (130315)  
BGD  
11, C8710–C8713, 2015 Interactive Comment

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Biogeosciences Discussions

Interactive comment on “Peru upwelling plankton respiration: calculations of carbon flux, nutrient retention efficiency and heterotrophic energy production” by T. T. Packard et al.

Anonymous Referee #3 Received and published: 13 February 2015

*This is a very interesting, well written and thought provoking manuscript that pursues the estimation of important and difficult to measure biogeochemical variables such as organic carbon flux and nutrient retention efficiency from estimates of plankton respiration derived from measurements of the ETS activity. Both the concept, calculations and results are novel, clearly presented and relevant for Biogeosciences. However I have some questions that would like to see discussed.*

*Comment #1: The model to estimate  $F_c$  from  $R$  (Eq.(1), Page 16184) is one-dimensional, which has several implications.*

*1.1. Lateral inputs of organic matter are assumed negligible for  $R$  compared to vertical fluxes (Page 16183, line 17). This goes against abundant observations of relevant lateral POC and DOC transport in upwelling systems, which should be important here given the dynamics of the upwelling during the sampling, described in section 3. Even in less dynamic and less heterogeneous systems, the horizontal scale of the region supplying organic carbon to a water column should depend on the depth of the water column, which varies here from 63 to 4755 m. It is not clear which is the top area of the seawater cube where  $F_c$  is estimated (Page 16183, line 15), nor if it is different for shallow and deep stations. I think that it is important that the horizontal domain of the model is stated. The consequences of the assumption of irrelevance of lateral inputs should be discussed, and if possible quantified.*

Authors' response: A priori, we thought as does the reviewer, but it seems that even in the dynamic waters of an upwelling system the vertical dynamics dominate horizontal ones. Using particulate protein (PP) as a proxy for particulate organic carbon (POC) and drawing on the analyses in Fig. 4 of Garfield et al. (1979) we find that the strongest horizontal gradient is in the offshore direction in the upper 130 m of the water column. This is  $5.51 \times 10^{-5}$  microgram PP per meter in the offshore direction. In the vertical direction we draw on Fig. 3 and Table 1 from Garfield et al. (1978) to calculate a vertical gradient of 326 microgram/l over 500m or 0.652 microgram/l per meter. Accordingly, the ratio of the vertical to horizontal gradient is  $0.652 / 5.51 \times 10^{-5} = 0.119 \times 10^5 = 1.2 \times 10^4$ , ten thousand fold greater! This means that in the strongest case for a horizontal gradient, in the upper 130 m of the water column the vertical protein gradient is ten thousand times greater than the horizontal gradient. In the deeper parts of the water column the horizontal gradients become even smaller, and the long-shore gradients are even smaller than the offshore

gradients (Garfield et al., 1978). Thus we feel justified in using a vertical model to calculate carbon flux.

As for the top of the seawater cube where carbon flux is estimated, it is the bottom of the euphotic zone ( $E_z$ ).

Proposed change in the manuscript: In our response to reviewer one's comment #6 we proposed that on page 16183, line 16-17 we would modify the text. Here we propose the same modification, but with the Garfield et al (1978) reference added. "The planktonic  $RCO_2$  in a seawater cube is considered as equivalent to the difference between the total  $F_{C1}$  through the top of the cube and total  $F_{C2}$  through the bottom of the cube, where total carbon flux refers to the sum of the DOC and the POC carbon flux, one can write an expression,  $RCO_2 = F_{C1} - F_{C2}$ . We assume on the basis of Craig (1971), Garfield et al, (1978), Carlson et al. (2010), Hansell et al. (2012) that DOC based R and lateral POC flux, compared to  $F_C$ , are less than 30% of the total. In other words, in the vertical, one-dimensional case, the changes in the  $F_C$  between depths in a water column are equal to the  $RCO_2$  between those depths. "

*Comment #2: The assumption that DOC-based R is negligible (Page 16183, line 17) also goes against abundant empirical evidence, and hence requires an analysis of the error introduced in  $F_C$  estimations. This analysis should include potential biases in terms of both magnitude and variation because surely the relative contribution of DOC and POC to R will vary in space and time.*

In reviewing the DOC literature by Reiner Schlitzer, Dennis Hansell, Craig Carlson and others we feel that our first-approximation-model for carbon-flux-from-respiration is valid. It is true that the calculations by this model will overestimate vertical carbon flux by the amount of respiration based on DOC oxidation, but Hansell et al. (2012) state that "...POC export fluxes will far exceed water column integrated rates of DOC removal in equatorial and coastal upwelling areas." This means that to a first-approximation focusing on POC is valid. Furthermore, Santinelli et al. (2013) point out that, "DOC concentrations are low in the upwelling regions, where water with high nutrients and low DOC are mixed with surface waters..". Earlier Schlitzer (2002) in a modeling study of global DOC distributions comments that, "...globally the relative size of downward DOC fluxes compared to particle fluxes decreases rapidly with depth, from about 30% at the base of the euphotic zone to 5% at 1078 m depth...". These percentages could be used as error estimates (on the high side) associated with our decision to focus on the POC flux.

Proposed change in the manuscript: We propose to use our response to comment #1 here.

*Comment #3 The one-dimensional model assumes that the relation between  $F_C$  and R is time independent, or that the system is in steady state. However the temporal scales of  $F_C$  and R are not the same, and connection does not need to be instantaneous. The derived time dependence may be particularly important here, not only because of the dynamic upwelling, but because R is derived from ETS measurements where accumulated biomass (whose response to organic carbon inputs is slower than that of metabolic rates) may play a higher role than in situ activity. In a highly dynamic upwelling, high heterotrophic biomass may be related to previous organic carbon inputs, while a large  $F_C$  may occur together with low biomass. Given*

*that the  $F_c$  should vary in time (and space) with productivity pulses, and that biomass evolution is not instantaneous, the potential limitations in estimating  $F_c$  from instantaneous vertical profiles of  $R$  should be discussed.*

Authors response: As with all ocean properties temporal and spatial scales are critical to understanding the meaning of any measurement, calculation, or model. Carbon flux calculations here are based on the particulate matter trapped inside a Niskin Bottle the moment it is tripped. It took about an hour to get that sample back on deck, filtered, and frozen in liquid nitrogen and 20 minutes to obtain the ETS activity reading. An ETS activity measurement is not as instantaneous as a CTD or transmissometer reading, but it is much closer to being instantaneous than a Winkler-based respiration, a C-14 based- or N-15 based- productivity measurement. The time and space scale for an invitro-based chlorophyll measurement on a filtered phytoplankton sample is a rough equivalent. Compared to a sediment trap measurement an ETS activity measurement is instantaneous. It should be comparable to the thorium-POC method for calculating carbon fluxes. We should state here that this use of respiration to calculate carbon flux goes back a long way (Riley, 1951), is fundamental in the development of Eppley and Peterson's (1979) Nature paper and is a logical extension of the thinking in Suess (1980) and Martin et al, (1987). With this conceptual relationship between carbon flux and respiration, we calculated a deep-sea carbon sequestration rate of 22 Gt C per year below 200m in Packard et al.(1988). In Packard and Christensen (2004) we developed the idea in detail. In Packard and Codispoti (2008) we applied it to calculating carbon flux in the Nansen Basin, in Packard and Gómez, 2013 we applied it to zooplankton carbon flux, and in Osma et al (2014) we applied it to calculating carbon flux in the Namibian upwelling. Proposed changes by the authors: In a revision we would add, on page 16184, line 9, the following sentence. "Note that the  $F_c$  here represents the flux at the time the CTD-Niskin cast was made. It is a fine scale calculation of C-Flux."

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Comment #4: *The model rests conceptually in the progressive consumption of organic carbon associated to its vertical flux. However, this progressive consumption carries with it changes in composition and lability of organic matter. The composition of organic carbon should also vary along the horizontal scale. How these changes in composition and lability reflect on the relation between  $F_c$  and  $R$  along the vertical and horizontal axes?*

Authors' response: ETS activity, in the ocean will not sense variability in the composition and lability variations in the POC or DOC. The posited variations mentioned by the reviewer could be reflected in physiological measures of the  $CO_2$  production rate, the electron flux connecting organic carbon oxidation and the consumption of the terminal electron acceptor, the reduction rate of the terminal electron acceptor, and the ATP production rate. However, as far as the authors know, these physiological rates in the deep-sea can not be measured by current technology. That is why they used ETS activity measurements as proxys for these rates.

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Comment #5: A single equation is used to convert ETS measurements to  $RO_2$  though the water column, however I would expect this relationship to change with the composition and activity of the plankton community along the water column (e.g. with the relative abundance of auto/heterotrophic organisms, eu/prokaryotes, etc.), especially between the phytoplankton rich euphotic zone and the bathypelagic waters. This equation derives from the ETS: $RO_2$  regression in Packard and Christensen (2004), which is based on 14 paired measurements from 4 stations in the Gulf of

Maine (Packard and Williams 1981), where subsurface chlorophyll a maxima of 1.1 to 3.2 g Chla/L were found between 25 and 40 m depth, and surface values ranged 0.17 to 0.77 g Chla/L. Both the magnitude and distribution of Chla in the euphotic zone here (Figure 1b) are very different to those in the Gulf of Maine. Moreover, in Packard and Williams (1981) RO<sub>2</sub> measurements were only made in the upper 10, 10, 9 and 25 m of each station, thus always excluding the DCM, and at depths where Chla concentration ranged from 0.09 to 0.94 ug/L. I feel that the use of this regression to convert ETS measurements to RO<sub>2</sub> throughout the euphotic zone, mesopelagic and bathypelagic waters of the Peru upwelling needs to be justified. Are there other regressions available that may cover the spectrum of ecosystems, or at least the ranges of biomass included in this study? 3) The ranges of ETS activity in the regression of Packard and Christensen (2004) is 293 to 919 g O<sub>2</sub> L<sup>-1</sup> d<sup>-1</sup>, however the range of ETS measurements here is 0.06 to 532g O<sub>2</sub> L<sup>-1</sup> d<sup>-1</sup> (calculated from Table 3). 88 out of 102 measurements (Table 3) are out of the range in the regression, and all the data deeper than 100 m (29 measurements) are at least two to four orders of magnitude below the minimum value in the regression. The validity of the regression outside its range needs to be defended.

RESPONSE- In the Nansen Basin we compared profiles of oxygen utilization rates down to 500 m as calculated from the Apparent Oxygen Utilization (AOU) and <sup>3</sup>H-<sup>3</sup>He ages according to Zheng et al. (1997), and from ETS-based oxygen utilization rates (OURs) using the factor 0.26 from Packard and Christensen (2004) and Packard and Williams (1980). The rates compare well (Fig. 3 from Packard and Codispoti (2007) attached below). We argue that because the ETS-based OURs compared well with the OURs calculated from the AOU/(<sup>3</sup>H-<sup>3</sup>He age) -based OUR, this is the best available way to calculate water respiration from our ETS measurements. We would like to point out that regardless of the way the respiration is calculated, the major contributions of this paper are: the use of water column respiration to calculate ocean sections of carbon flux, nutrient retention deficiency, and heterotrophic energy production as well as the recognition of the role in the exponent, b, in determining carbon-flux transfer efficiency and the NRE.

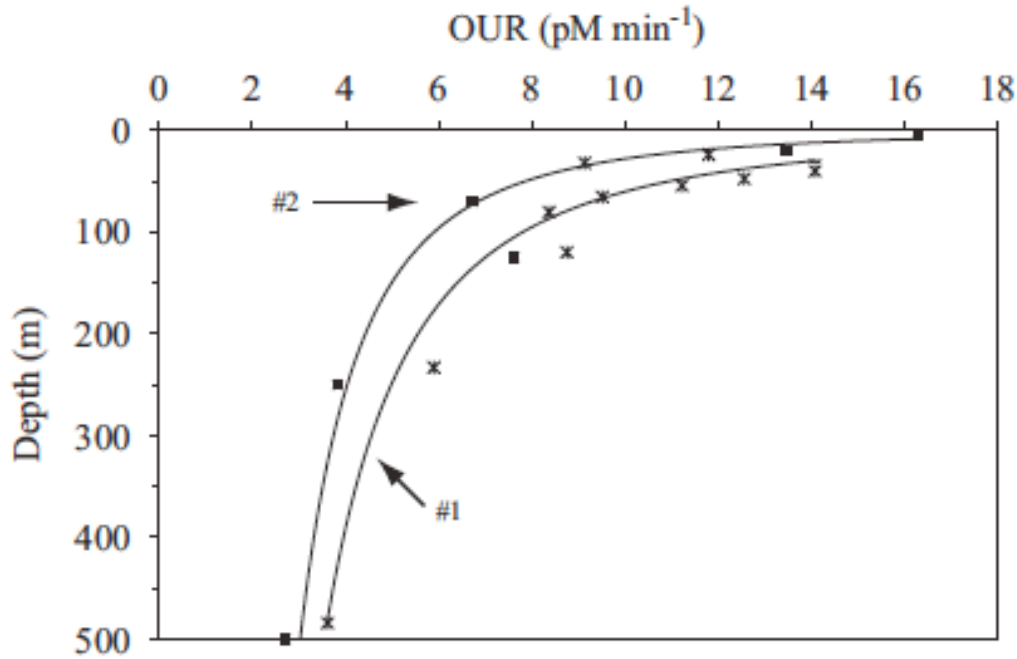


Fig. 3. Comparison between oxygen utilization rates in the Nansen Basin as calculated by two different methods. (1) OUR from AOU and  $^3\text{H}$ - $^3\text{He}$  ages according to Zheng et al. (1997,  $\text{OUR} = 52.155Z^{-(0.4058)}$ ,  $r^2 = 0.8274$  (stars)). (2) ETS-based  $R_{\text{O}_2}$ s from absorbances in Fig. 2 according to Table 1 [ $R_{\text{O}_2} = 67.477Z^{-(0.5534)}$ ,  $r^2 = 0.6924$  (squares)]. The latter is a composite of measurements taken under the Arctic ice cap north of the Yermak Plateau between 13 and 15 April 1981 as the ice station drifted 6.3 km/day from  $83^\circ03.85'\text{N}$ ,  $7^\circ14.40'\text{E}$  to  $82^\circ57.93'\text{N}$ ,  $6^\circ58.97'\text{E}$  (Hunkins, 1986). Sonic depth ranged from 4024 to 3982 m. Note that changes over months are required for the measurements of profile #1, whereas changes over minutes are sufficient for measurements of profile #2.

Proposed changes in the manuscript: On line 19-20, page 16182 we would change the sentence that now reads, “The calculations .... (2007)” to “The calculations for potential respiration from the combined ETS data set were made according to Packard and Christensen (2004). The ETS activity, in units of micro electron equivalents  $\text{min}^{-1} \text{m}^{-3}$ , was divided by 4 because 4 electrons are required to reduce 1 molecule of  $\text{O}_2$  in respiration ( $4\text{e}^- + 4\text{H}^+ + \text{O}_2 \rightarrow 2\text{H}_2\text{O}$ ). The result is potential respiration in units of micro mol  $\text{O}_2 \text{min}^{-1} \text{m}^{-3}$ . Multiply by 60 to achieve hourly units (Table 2). The calculations for respiratory oxygen consumption from potential respiration were made according to Packard and Codispoti (2007). The potential respiration in units of micro mol  $\text{O}_2 \text{h}^{-1} \text{m}^{-3}$  were multiplied in Table 2 by the unitless factor, 0.26, from Packard and Christensen (2004). This factor was chosen because of its success in calculating the water column oxygen utilization rate (OUR) in the Arctic ocean where the OUR

had previously been calculated from the Apparent Oxygen Utilization (AOU) and  $^3\text{H}$ - $^3\text{He}$  ages by Zheng et al. (1997). This is the best available way to calculate water respiration from our water column ETS measurements.”

*Comment #6: Minor comments: Page 16179, line 2, rather than net primary production, I think it should be net community production.*

*Authors' response: We agree, especially since we are citing the Ducklow and Doney (2013) reference where this is particularly the case..*

*Proposed change: On page 16179, line 2, we will change the text to read: Plankton community respiration is a key variable in calculating net community productivity (Ducklow and Doney, 2013)*

*Comment #7: If ETS measurements and  $\text{RO}_2$  calculations were made throughout the water column, why in Fig.2.a only R in the EZ is based directly on the ETS measurements while the R in the aphotic zone below is based on the R models in Table 4 (Page 16183, line 8)? Are these modelled R estimates or the actual data what are used to estimate Fc from integrated R?*

*Author' response: We could have used the respiration models given in Table 4 throughout the water column. However, since we had abundant direct ETS-based respiration measurements in the euphotic zone (Table 3) we decided to use them in Fig. 2. We feel that this is the best way to prepare the section on respiration in Fig. 2a.*

*Proposed change in the text: On page 16183, lines 7-10, after Charland (2002) would replace the existing text with the following. “These R models at each station (Table 4) along with the euphotic zone respiration data (Table 3), were used to create Fig. 2a. Note that R in the Ez of these sections is based on the respiration calculations in Table 3 while the R in the aphotic zone below is based on the R models in Table 4.”*

*Comment #8: This manuscript makes a solid statement of the central position of plankton R in the connection of ecological and biogeochemical fluxes of energy, carbon and nutrients in the ocean, and from there explores new modes of estimating difficult to measure biogeochemical rates. This is important and timely, and a difficult undertaking because of the complexity of those connections across time and space. Although I ask the authors to explore and discuss these difficulties, and limitations derived from the necessary simplification in their model, their original approach is insightful and potentially important.*

*Interactive comment on Biogeosciences Discuss., 11, 16177, 2014.*

*Authors' response: We have tried to conscientiously to answer each of the reviewer's comments in a substantive way. Should our responses be found wanting, we will reconsider our answers and proposed changes.*

## REFERENCES (not given in the paper)

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