RESPONSE to REVIEWER #3 (130315) BGD 11, C8710–C8713, 2015 Interactive Comment
(T. T. Packard, N. Osma, I. Fernández-Urruzola, L. A. Codispoti, J. P. Christensen, and M. Gómez.
Peru upwelling plankton respiration: calculations of carbon flux, nutrient retention efficiency and heterotrophic energy production. Biogeosciences Discuss., 11, 16177-16206, 2014)
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Comment #1: I agree with the response of the authors.

Authors' response: Thanks!

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Comment #2: I may be misunderstanding the authors. To me the expression DOC-based respiration stands for the portion of R supported by DOC, rather than the amount of DOC drawdown by R. (We agree.) However, it seems that the focus in the response of the authors is on substantiating that the contribution of DOC to the vertical flux of TOC may be minor, which I agree. If I am correct, I would suggest to change the expression DOC-based R to DOC flux. I would also suggest to explicitly state in the manuscript that FC stands for particle C flux. However, DOC based R (I mean the portion of R supported by DOC) is included in the ETS measurement, hence I would say that rather than assuming that DOC flux is negligible, the calculated FC would be total rather than POC only.

Authors' response: Please note that we calculate carbon-flux below the euphotic zone where the DOC has already been drawn down significantly. We are aware from our own experiments in the Gulf of St Lawrence, that plankton community rapidly assimilates labile DOC and that the degradation of this DOC supports the ETS-based respiration (Packard et al., 2000).

Reference: Packard, T., Chen, W., Blasco, D., Savenkoff, C., Vezina, A. F., Tian, R., ... & Ingram, R. G. (2000). Dissolved organic carbon in the Gulf of St. Lawrence. *Deep Sea Research Part II: Topical Studies in Oceanography*, *47*(3), 435-459.

Note that in the carbon-flux methods section we now say," Conceptually, planktonic RCO2 in a seawater cube is considered as equivalent to the difference between the total FC1 through the top of the cube and total FC2 through the bottom of the cube, where total carbon flux refers to the sum of the DOC and the POC carbon flux. We deduce, on the basis of (Craig, 1971; Carlson et al., 2010; Hansell et al., 2012), that R based on DOC and lateral POC flux, compared to the R based on the vertical flux of labile POC, is less than 30% of the total R. Note that if organic matter, in any form, is resistant to oxidation (Arrieta et al., 2015) its flux through the water column will not be detected by respiration measurements. The flux will be transparent to our ETS measurements. However, the dissolved organic matter in the ocean, at least, appears to be oxidizable (Arrieta et al., 2015). In all cases, to a first approximation, one can express our conceptual model by the expression,RCO2 =FC1 -FC2.In other words,in the vertical, one-dimensional case, the changes in the FC between depths in a water column are equal to the RCO2 between those depths."

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Comment #3: My concern is not for the scale of the measurement of R, but for the assumption of steady-state implicit in the calculation of FC from "instantaneous" R in a water column of up to 2000 m depth. In my opinion this assumption should be at least explicit in the paper. (Please see also Comment # 5 below).

Authors' response: Just before equation #2 in the carbon-flux method section, we have added the line: "Note that these carbon-flux calculations represent the flux at the time the CTD-Niskin cast was made. They are fine scale calculations of C-Flux."

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Comment #4: I understand the usefulness of the ETS method for measuring R in the deep ocean. However, sustained differences in lability and nutritional value of organic matter between the euphotic zone and the deep ocean should also reflect in differences in the biomass of heterotrophs and respiratory enzymes per unit organic carbon available (and hence in ETS measurements, and not only in the physiological rates). Moreover, while deep R is ultimately constrained by the FC (there cannot be deep R without FC), the FC is not completely constrained by R (there can be a vertical flux of recalcitrant C with zero or near zero R). The question hence was not why the authors used the ETS method, but how changes in C usability may bias the estimation of FC from R. However, I understand that these changes may be difficult to parameterise here and these potential flaws in the accuracy of calculations do not undermine the value of ideas and novel approaches presented in this paper.

Authors' response: This is true," ....the FC is not completely constrained by R (there can be a vertical flux of recalcitrant C with zero or near zero R."  This means that our way of calculating Fc will underestimate carbon flux by the amount of recalcitrant POC and DOC that is passing through an ocean layer.  We have assumed that this fraction is minimal, but we will have stated this assumption (above) and have cited Arrieta et al 2015 and this reviewer for pointing this out.

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Comment #5: Although I still feel uneasy by the large disagreement in ranges of measured and predicted rates, and by differences in modelled and predicted ecosystems, I totally agree that the major contributions of this paper are conceptual rather than quantitative, and hence the way the respiration is calculated is of relatively minor importance. The comparison of OUR rates derived from AOU and 3H–3He ages with those calculated from ETS measurements using the 0.26 factor in Packard and Codispoti (2007) supports the qualitative validity of the calculation for this paper.  Moreover, given the differences in timescale of these measurements (which are actually highlighted in the legend of Fig.3 in Packard and Codispoti (2007)), such general agreement seems to also support the validity of the steady state assumption (my comment #3).

Authors' response.  Thanks!  We would like to compare our work with the Jenkins method in other deep ocean waters, but at the moment are not in a position to do this.  For the moment, we will stick with our 0.26 factor until we have better data.  Now, in the paper, this section reads:

"Potential respiration and respiration were calculated from the combined ETS data set according to Packard and Christensen (2004) and Packard and Codispoti (2007). Tables 2 and 3 explain the calculations in detail. Table 3 presents the calculations as RO2 in units of μmol O2 m−3 h−1 for oxic waters. Using ETS activity as a proxy for RO2 requires selection of a ratio of potential respiration (Φ) to RO2. Since direct measurements of RO2 cannot be made below the euphoric zone, a true calibration cannot be made. The Φ to RO2 ratio should range around 0.5 if Φ represents Vmax of the ETS and standard physiological rates, governed by enzyme activities, operate close to 1/2 their potential capacity (Cleland, 1967). In our hands (sense Schatteman et al. (1988); Sigman et al. (1997)), with our methodology (Packard and Williams, 1981), and by our analysis (Packard and Christensen, 2004) we calculated a Φ-to-RO2 ratio, 0.26 (Table2), that successfully predicted RO2 in the epipelagic and the mesopelagic waters of the Nansen Basin of the Arctic Ocean (Packard and Codispoti, 2007). In that study, RO2 was a long-term average RO2 calculated by the AOU-He-tritium method of Jenkins (1982, 1984) as used by W. Roether in Zheng et al. (1997). We have chosen to use the same Φ to RO2 ratio of 0.26 here (Table 2 and 3). RCO2 (Fig. 2a) was then calculated from RO2 using a Redfield ratio (C/O2) of 0.71 from Takahashi et al. (1985). This is the best available way to calculate water respiration from our water column ETS measurements."