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)

Comment #1: I agree with the response of the authors.

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. 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.

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).

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.

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).