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## ***Interactive comment on “Seasonal and latitudinal patterns of pelagic community metabolism in surface waters of the Atlantic Ocean” by S. Agusti***

**Anonymous Referee #3**

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The objective of this study is to analyze the variability on the metabolic balance of the pelagic communities of the Atlantic Ocean. To this aim the plankton metabolism (GPP, NCP and R) and chlorophyll a concentration were measured at the surface of the Atlantic Ocean during four latitudinal cruises in spring and autumn. Seasonality was only found for CR and NCP, not for GPP and chl<sub>a</sub>, leading to the conclusion that R explains the net heterotrophic balances and variability of NCP, which are attributed to the influence of temperature on the activity of heterotrophic zooplankton and bacteria. I think that the discussion on mesozooplankton should be excluded as the contribution of these organisms to the R rates measured by the author (in 125 mL samples from Niskin bottles) should be negligible compared to those of pro- and eukaryotic microbes. Regarding temperature, although it is certainly an important factor, for sustaining the observed net heterotrophy the critical point is the supply of allochthonous organic mat-

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ter, which should be discussed in the manuscript. However I will focus my comments here on my main concern, which is not discussion but the data set and its analysis. Reviewing interpretations in Discussion would require resolving these issues first.

I am disinclined to accept that we can “analyze the spatial and seasonal variability on the metabolic balance of the pelagic communities of the Atlantic Ocean” by measuring GPP and R at one single depth in the water column. As the author states, “the net metabolism of a system is an important descriptor of the role of the biological processes in the carbon flow”; which hence critically requires integrating those processes over the spatial and temporal scales of the system. Commonly accepted scales for integration of trophic processes in planktonic communities in the open ocean are the photic zone, the mixed layer, the compensation or the critical depth, but not the surface of the water column. Two good references here are Smith and Hollibaugh (1993) and Williams (1998), who wrote “I have come to the conclusion that with the data sets of net primary production and respiration currently available to us, generalizations on the regional distribution of carbon balance in the oceans cannot be derived from simple regression analysis of volumetric observations”. Particularly when they come from a single depth, disregarding the variability of subsurface processes. Recent evidence and discussion on the issue (e.g. Gist et al. 2009) are based on photic zone integrated data, and hence cannot be compared with the data set presented here. Even within the data set presented here I find it difficult to compare surface P:R balances between systems with such a different vertical distribution, variability and control of physical, chemical and biological properties as the ocean gyres, the Equatorial upwelling, and the NW African upwelling.

Inferences on the seasonal variability are based on the combination of all the data from each cruise into a single mean, which is assumed to be the representative value of the corresponding rate for that season in the Atlantic (Tables 1 and 2). However, it is commonly accepted that the study of primary production in the ocean requires a biogeographic partition to accommodate regional differences in control and seasonal-

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ity, and the same necessarily applies to the balances between primary production and respiration. The presented mean of some surface data of P and R from the NAST, CNRY, WTRA, SATL and BRZ (Longhurst 1998) is to me unintelligible. And deriving the seasonality of the metabolic balance in the Atlantic Ocean from the difference between two such means in different seasons, is not possible, especially when different provinces were sampled in different cruises (e.g. Latitude 2 included the NW African upwelling) (see fig.1).

Data are presented without their corresponding s.e. or deviation, which makes figures 3 to 5 ineffective, and their interpretation not acceptable. This is an important issue that needs to be revised because the observation of higher across systems variability of R compared to GPP is quite an unusual one, especially when data come from the highly productive NW African upwelling, the equatorial upwelling and the oligotrophic gyres. Interestingly, the highest R data come from Latitude 4 and 1, that did not cross through the NW African upwelling. Similarly unusual is the correlation of NCP with R but not with GPP (e.g. Aristegui and Harrison 2002; del Giorgio and Duarte 2002). The author states that the predominantly “low productivity was expected since the area included the sampling in the ultra oligotrophic waters of both North and South Atlantic subtropical gyres” (L5. Discussion). However in a extensive analysis including 6 AMT cruises and the global database at [www.amt-uk.org/data/respiration.xls](http://www.amt-uk.org/data/respiration.xls), Gist et al (2009) found maxima R rates in photic zone of the N and S Atlantic gyres of ca. 2 and 1.5 mmol O<sub>2</sub> m<sup>-3</sup> d<sup>-1</sup>, respectively. In the R data set presented here aprox half of the surface data are > 2 mmol O<sub>2</sub> m<sup>-3</sup> d<sup>-1</sup>, with maxima rates of >7 mmol O<sub>2</sub> m<sup>-3</sup> d<sup>-1</sup>. The data presented should be accompanied by an estimate of their precision, and the author should explain the high R rates.

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