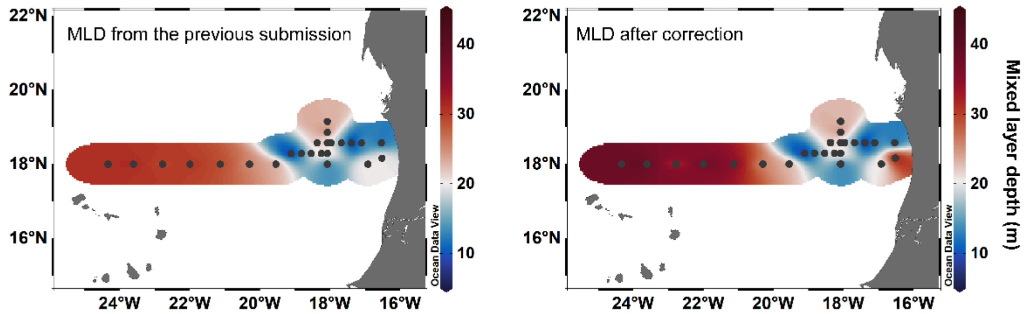
Review of manuscript “Eddy enhanced primary production sustains heterotrophic microbial activities in the Eastern Tropical North Atlantic”

The authors have made a great effort revising the original manuscript and have adequately addressed all my concerns. I only have a few minor revisions/suggestions.

We thank the reviewer for further supporting our manuscript and the helpful comments. We have made the changes according to the reviewer’s suggestions as outlined below.

During the revision of our manuscript, we noticed that the algorithm used to calculate the mixed layer depth based on the density criterion after Levitus, 1982 (the depth at which a change from the surface density of 0.125 kg m^{-3} has occurred) resulted in a slight underestimation of the mixed layer depth in a few profiles from the open ocean (E1, S1, E2, S3) and coastal (E5) stations. We have changed the depth of the mixed layer in these profiles (five out of 24) by visually checking them and making any necessary corrections manually. We have changed the text, tables (SI Table n°1), and figures (Figures 3 and 6) accordingly. Those changes do not affect the interpretation of the results and all trends remained the same. Please see below for a comparison of the mixed layer depth before and after manual correction.



Line 335 an elsewhere. The term nutricline is not correctly used here a several other parts of the manuscript. I suggest changing it to “nutrient isolines”.

We have made the correction according to the referee's suggestion.

Line 384. Revise this maximum value for integrated Chl-a (and also in Table 1) as the value is not coherent with figure 3, where the maximum values in the map is 160 mg m-2

We have revised the values and changed figure 3 accordingly.

Line 524. Revise this fragment “coupled but differently” for clarity. I guess the authors mean that both variables were more coupled than in the stations outside the eddy (correlation coefficient is higher)

We have changed the text to:

In contrast to the stations outside the eddy, HB was not correlated to PP_{TOT}, PP_{DOC} and SL-DOC ($p>0.05$), but was strongly correlated to Chl-a and autotrophic pico-and nanoplankton biomass ($r=0.57$ and 0.76, respectively, $p<0.001$). Lines 523-525.

Lines 603-606. The authors could extend a bit more this discussion adding relevant references dealing with variation of PER along productivity gradients and/or in relation to phytoplankton size.

We have made the correction according to the referee's suggestion.

Line 651. Please consider revising the expression “makes sense” as it seems to colloquial.

We have changed this sentence to: “SL-DOC concentrations showed a strong positive correlation with BR, indicating that high molecular weight DOC compounds (>1 kDa) are an available carbon source for heterotrophic microbes (Amon and Benner, 1994, 1996; Benner and Amon, 2015).” Lines 650-653.

Line 652. I suggest changing “favourable” to “utilizable” or “available”.

We have made the correction according to the referee's suggestion.

Lines 655-656. Very low BGEs could be related to nutrient limitation. If bacteria have C available but not inorganic nutrients, the building of biomass (i.e BP) may be limited.

We have added the following sentence to add this possibility: “One explanation might be that variability of nutrient availability in the surface waters limited the building of bacterial biomass (Thingstad et al., 1997; Janson et al., 2006; Berggren et al., 2010) but this requires further studies.”. Lines 656-659.