Response to comments from reviewer #2

General comments:

This study mainly investigated how cyclonic eddy (CE) affects heterotrophic bacterial activities in the surface waters of the eastern tropical North Atlantic by using measurements of various parameters related to the microbial activities. The measurements are valuable for understanding the effect of CE on the microbial activities. The study is interesting and suitable for the scope of this journal. However, there are several points which should be made clearer before publication. Please find below specific comments.

Major comments:

In this study, bacterial biomass production (BP) and community respiration (CR) rates are the most important parameters. Those rates depend on in situ temperature. However, BP and CR were estimated not at in situ temperature but at 14 â□□. The reason why the authors used 14 â□□ as incubation temperature should be mentioned.

We thank the reviewer for the thorough review of our manuscript. We agree with the reviewer that incubating at in situ temperatures would have been favorable. As explained as response to a comment from reviewer #1 regarding the same criticism, we had to choose a different incubation temperature than in situ temperature for CR for technical reasons. Only a fridge with a temperature range between 4 °C to 16 °C was available to us that could be used with the optode setup. To obtain comparable results for BP and CR, we have used the same temperature for both rate measurements, i.e., 14 °C. The well-documented dependence of CR and BP rates on temperature (e.g., López-Urrutia and Morán, 2007; Yvon-Durocher et al., 2012) allowed us to correct for the difference between incubation and in situ temperature. The temperature correction is explained in detail in the Methods section and the SI.

There are several points that are not based on the clear evidences:

1)bacterial respiration rates are related to semi-labile (SL) dissolved organic carbon (DOC) concentration (lines 651-652), 2) microbes in the CE preferentially use SL-DOC (lines 696-697), 3) microbes do not grow in tandem with the increase in dissolved primary production (PP_{DOC}) but are related to the different requirement between BR and BP (lines 715-717), and 4) bacterial growth efficiency (BGE) varies depending on both BP via phytoplankton taxonomical composition and BR via the quantity and quality of the SL-DOC (lines 720-722). The statements 1), 2) and 3) are probably based on the results of correlations between relevant parameters (Fig. 7), while the statement of 4) is probably based on Table 2, Figs. 6a,b and 7. The results that each statement is based on

are not clear at present. Please make the statements clearer by referring to proper results.

The reviewer is correct. The statements 1-3 are based on Fig. 7 (Fig.6 in the original manuscript), while statement 4 is based on Fig. 7 (Fig.6 in the original manuscript) and Table 2. Some of the statements would be changed in a revised version due to comments from reviewer #1, but where appropriate, we will refer to the Figure or Table for each of the statements.

Specific comments:

Line 271: How long scintillation vials are left open after addition of HCl should be described and proper reference should also be added here. I wonder if all dissolved inorganic carbon can be removed by the method or not.

The scintillation vials were left open for 14 hours after addition of HCL according to the method described in Steemann Nielsen (1952). We will add this information and the reference. The blanks showed no evidence of remaining inorganic carbon.

Figure 3: Adding the depth profiles of BGE and PP_{DOC} is helpful for readers.

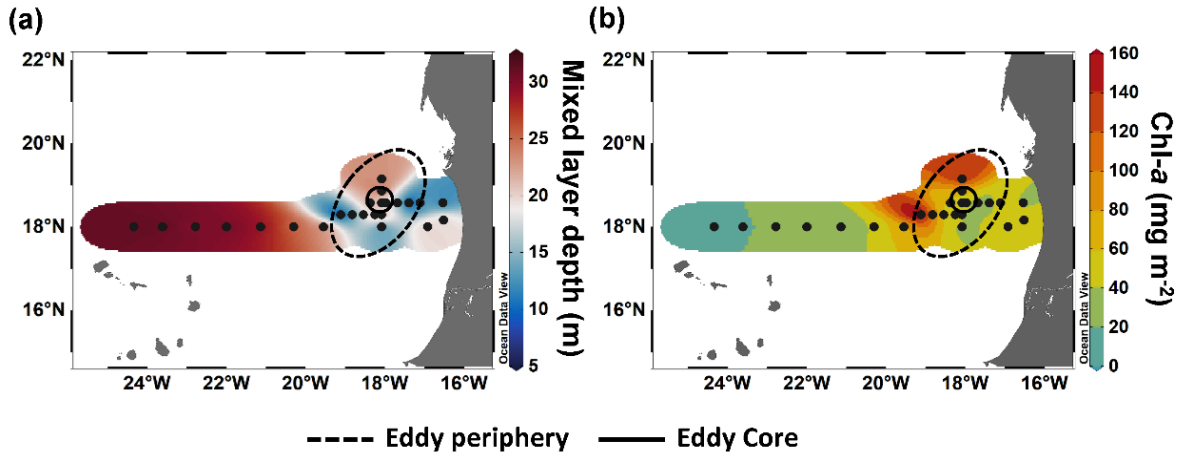
We will add the depth profiles to the Figures 3 and 4, respectively.

Lines 569 and 593: The authors mentioned high vertical mixing due to strong surface winds. Showing the strong surface wind data would be helpful for readers.

The wind data was shown in Supplementary Figure 6 of the original submission. We will refer to this Figure.

Lines 608-609: Mixed layer depths should be added to Figures 2, 3, and 4 for easy readability.

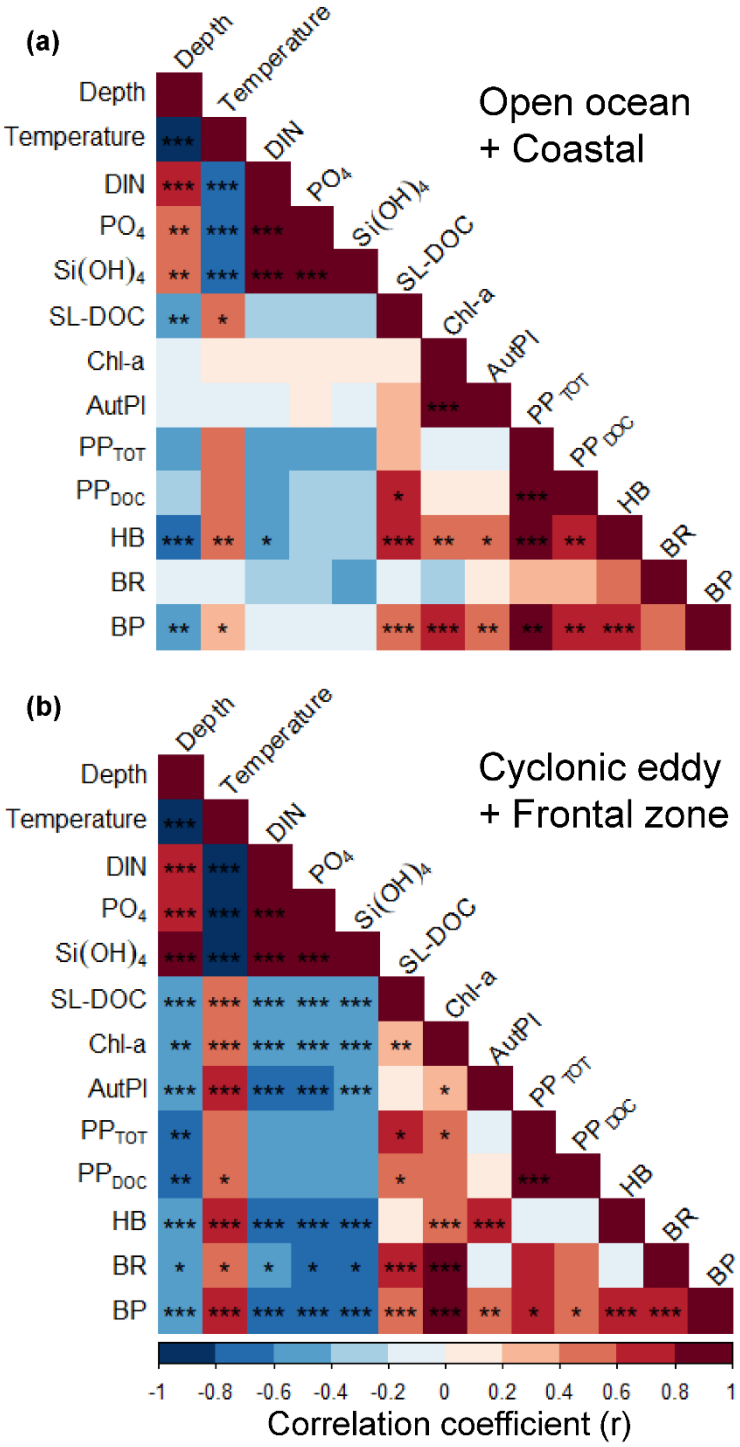
We will consider the reviewer's suggestion. However, after including the mixed layer depth in the plots, we feel that the figures might be overloaded. Instead, we suggest to show the mixed layer depth in a new Figure (3a, see below) and in Table S1.



Proposed new Figure 3: Spatial distribution of the mixed layer depth (a) and chlorophyll *a* integrated over the upper 100 m depth (b) during M156.

Lines 630-631: Please clarify whether all the data of HB abundance and particulate primary production or a part of those data were used

The statement was based on the correlation of the parameters in the open ocean and coastal stations only (stations not affected by the eddy). According to a comment from reviewer #1, we will include a correlation analysis for the eddy-influenced stations and the stations not influenced by the eddy in two panels A and B (see proposed new figure below) and remove the original correlation analysis. We will refer to the new Figure and change the text to make it clearer that the statement is based on the correlations for the stations not affected by the eddy.



Proposed new Figure 7: Pearson correlation matrix of biochemical parameters, metabolic activities, and bacterial abundance in the upper 100 m in samples not influenced by the cyclonic eddy (i.e., coastal and open ocean stations) (a) and samples influenced by the cyclonic eddy (b). Statistical significance: '***'< 0.001, '**'< 0.01, '*'< 0.05.