

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-49

Anonymous Referee #2

Referee comment on "Eddy-enhanced primary production sustains heterotrophic microbial activities in the Eastern Tropical North Atlantic" by Quentin Devresse et al.,
Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-49-RC2>, 2022

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 °C. The reason why the authors used 14 °C as incubation temperature should be mentioned.
- 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.

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.
- Figure 3: Adding the depth profiles of BGE and PP_{DOC} is helpful for readers.
- 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.
- Lines 608-609: Mixed layer depths should be added to Figures 2, 3, and 4 for easy readability.
- Lines 630-631: Please clarify whether all the data of HB abundance and particulate primary production or a part of those data were used.