

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-49

Anonymous Referee #1

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-RC1>, 2022

The authors present data from a detailed oceanographic survey in the Eastern Tropical North Atlantic, along a transect crossing a cyclonic eddy (CE). It is a highly valuable piece of data that contribute to better understand the impact of CE on microbial metabolism. Yet, the spatial resolution for some of the biological variables is limited (primary production, bacterial production and community respiration) and some of the metabolic rates were estimated (bacterial respiration) or measured at lower temperature than in situ temperature (bacterial production and community respiration), which may be distorting the relationship among variables. It is very difficult to understand why bacterial production or respiration could not be incubated at in situ, while Pp was incubated at in situ temperature. This is a major drawback, as the method used for BP estimation actually provides exactly the same BP at 22°C than at 14°C, which seems rather unlikely, at least in the absence of resource limitation. This definitely requires further explanation, or even using a different model for BP estimates. Therefore, the manuscript need a major revision to clarify and, eventually, reanalyze the results. The discussion should also be accordingly revised, and avoid repeating results or speculative statements. Also, the stations should be clearly identified in all the figures, and some figures should be revised. The English usage should be also carefully revised.

Specific comments:

Title: I suggest changing the title, as the authors do not provide growth data and also the term "accelerates" is rather confusing. Moreover, primary production appears to be enhanced in a frontal zone, not in the CE, and this should be clear already in the title.

Abstract:

Line 19: revise "Mauretania" throughout the text and change to "Mauritania".

Line 21: revise the use of the term "cascading", which implies a temporal dimension, that has not been adequately addressed here.

Line 26: revise the use of the term parameter, which is not equivalent to the term variable. As an example, chl-a concentration is not a parameter.

Lines 25-27: please be more specific, and clearly indicate that the maximum concentration of phytoplankton occurred in the frontal zone.

Line 36: indicate to what this percentage is referred to.

Lines 36-37: I do not think that PP/BCD reflects the metabolic state of the microbial community. I suggest either using PP/CR as an estimation of the metabolic state of the microbial plankton community, or use BCD/PP as indication of the fraction of PP production that is processed by bacteria. Please be specific. A $PP/BCD > 1$ does not necessarily imply an autotrophic balance.

Introduction:

Line 90: indicate that you refer to bacterial biomass production.

Line 92: provide more recent references for the effect of DOM on BGE.

Line 100: provide references for BR on eddies.

Lines 105-107: this part is somehow repetitive with information in lines 79-82. Please revise and avoid repetition.

Lines 109-111: this part is also repetitive with that in lines 69-71. Please revise.

Line 116: please specify the spatial resolution of the study.

Materials and methods:

Lines 133-134: please clarify what you mean by "consecutive optimized identification of the eddy".

Lines 141-144: please provide also information about the temporal sequencing of the survey. A supplementary table indicating the sampling rate of each stations would be nice.

Lines 158-162: please re-write for clarity and English usage.

Figure 1: The cruise track is not visible in the figure. Else, the positions of the stations in the CE are not fully visible, I suggest making a different graph for the stations within the CE. Finally, increase the size of the symbols in plots b, c and d.

Line 178: nitrate and nitrite lack the symbol of the charge

Lines 179-181: please provide a reference for this statement.

Lines 187 and 196: please clarify if you measured dAA or dHAA.

Lines 218-221: this is not equivalent to autotrophic plankton biomass, as it is including only pico and nanoplankton. Please use a term that clearly states this to avoid any possible confusion.

Line 227: two duplicate samples and only one killed control is not really sufficient to get accurate estimates of BP.

Line 229: the authors should clearly explain why they did not measure BP at in situ temperature. This is a major limitation of their work, and is not sufficiently justified.

Line 233: the author should consider to use a different model to estimate BP at 22°C from estimates at 14°C.

Line 236: again, the authors should justify the reason why they did not measure CR at in situ temperature. Also, they should explain why they conducted incubations > 24 h, and when. Finally, the number of replicates for CR should be also indicated.

Lines 253-254: again, the number of replicates is too low.

Line 254: indicate where PP incubations were done (controlled chamber?).

Line 264: the author should justify the use of 0.4 instead of 0.2 microns PC filters to separate the dissolved fraction. Some bacteria can pass through 0.4 microns.

Lines 269-270: please re-write for clarity.

Line 302-303: clarify the method of integration. Is the same as the trapezoid rule?

Results:

Figure 2: please clearly indicate in the plots the identification code of each station. Also increase the size of the dots.

Line 373: clarify that this is not autotrophic plankton biomass, it is only pico and nanoplankton using another term to refer to this.

Lines 379-381: as stated above, it is better using a term that clearly define what this variable is, and thus, this sentence can be removed.

Table 1: please clarify how do you integrate down to 100 m in stations lacking samples below 50-75 m. Revise the use of the term "parameter". Why do the authors specify depths and sampling date for only some of the stations?

Table1: the differences between integrated chl-a- between EDZ1 and E3 are weird and not expected from what is presented in figure 3 (although in figure 3 the stations are not clearly indicated). Overall the results section is very difficult to follow due to the lack of station labels in figures.

Figure 3: please add station labels and increase the size of dots. Rename the variable AutPI for clarity (it is only pico and nano plankton biomass).

Line 433: clarify if you refer to integrated or volumetric BP rates

Line 430: $PP/BCD < 1$, does not indicate heterotrophic balance or conditions, it just indicates that concurrent PP is not fulfilling BCD. Revise and be more specific. I suggest either using PP/CR as an estimation of the metabolic state of the microbial plankton community, or use BCD/PP as indication of the fraction of PP production that is processed by bacteria.

Line 453: I suggest including this calculation (PP_{DOC}/BCD) in table 2.

Figure 4: please add station labels and increase the size of dots. Clearly state in the figure legend that BP and BR are estimates and indicate the method used for that estimation.

Table 2: I suggest using PP/CR and BCD/PP as more insightful ratios than BCD/PP .

Line 484: revise the usage of the term "indices" here, as it does not reflect the content of the section.

Line 488: the correlation between cell-specific BR and BGE is spurious as both contain the variable BR. Please remove from the analysis.

Figure 5: I suggest representing BCD/PP , I find it more intuitive than the inverse.

Lines 491-493: I suggest removing also the correlation between chl-a and the biomass of pico and nanophytoplankton, as it is not necessary. It is enough indicating that the discrepancies are due to the fact that chl-a is total, and the biomass is only from small phytoplankton.

Figure 6: I suggest removing plot (a) (because it is spurious) and (b) (as it is not necessary). Maybe the authors could add plots relating Chl-a vs. BP and/or BCD vs. PP_{DOC}.

Lines 513-524 and figure 7: please revise to eliminate the spurious correlations (e.g. BCD vs BR or BR; PP_{tot} vs PP_{doc}). The authors could try to calculate correlation using data not affected and affected by the CE.

Discussion:

Lines 545-546: this is speculative as the authors do not have data about the fraction of large phytoplankton. The relation between chl-a and biomass are also affected by factors such as photoacclimation. The authors can only guess that in more productive stations large phytoplankton is likely more relevant, but they do not have data to support that statement.

Line 553-555: this is again speculative, the authors do not have data on the contribution of small planktons, they only have total chl-a and the biomass of the small fraction, but the relation between chl-a and biomass is not straightforward. I suggest eliminating this statement

Lines 565: I suggest using an alternative to "compression", such as e.g. "uplifting".

Lines 567-568: revise for English usage.

Lines 573-575: revise the sentence, it is hard to follow the reasoning.

Lines 576-580: delete as this is mostly results.

Lines 594-598: again very speculative. The authors do not have data about the presence of diatoms or dinoflagellates in this study. Delete or rewrite.

Line 603: the use of the term "diversity" is not appropriate here, as the authors only provide data of a couple of functional phytoplankton groups.

Lines 603-606: revise English usage as it is very difficult to understand the sentence.

Lines 607-608: delete this sentence, the authors do not have data on phytoplankton taxonomy, only flow cytometry counts of different groups based on scatter and fluorescence.

Lines 619-621: this sentence is just repeating results. Please, delete.

Line 621-625: please revise English usage.

Lines 630-631: please indicate where this correlation is found in the results, as the correlation matrix in figure 7 was calculated including all data.

Lines 631-633: revise English usage. Else, it is hard to see such continuous trends in HB or PP in the figures.

Line 635: delete this first sentence.

Line 636: explain the acronym CanUS.

Lines 639-641: re-write for clarity. Again, avoid statements about phytoplankton compositions, as the authors are not reporting such data (they only have cytometric groups).

Line 656: please tone down, change "state" to "suggest".

Lines 661-662: certainly BGEs are very low, which may be partially related to a severe underestimation of BP (see general comments and comments to the materials and methods section).

Lines 686-688: please delete references to the presence of diatoms and/or dinoflagellates as these data are not provided. Also tone down the statement.

Lines 689-706: all this discussion must be revised once BP estimates are clarified. Also english usage should be revised.

Lines 707-714: all this paragraph is about an spurious relationship. In addition, the authors do not have data on bacterial community composition. I suggest deleting it.

Line 715: revise the usage of the term "growth" as this variable was not included in this study.

Lines 720-722: revise as it is very difficult to follow the reasoning, as phytoplankton taxonomic composition is not provided in this study.

Lines 732-736: revise English usage. In addition, revise statements about temporal dynamics, which does not seem to be adequately resolved in this survey.

