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Interactive comment on “Biogeochemical controls on the bacterial population in the eastern Atlantic Ocean” by S. B. Neogi et al.

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We thank the reviewers for their valuable comments to make our manuscript better understandable. We are pleased that both of the reviewers has acknowledged our large scale analyses of biogeochemical and bacteriological parameters and our novel finding about the important role of dissolved organic matter on *Vibrio* abundance and survival in the nutrient-poor, oligotrophic ocean. We have modified the manuscript according to the thoughtful suggestions of the reviewers. Major changes in the revised manuscript are as follows: i) the focus on *Vibrio* has been emphasized, ii) the significance of the dissolved (organic) nutrients specifically for *Vibrio* has been emphasized – especially by providing an additional statistical verification (multi-dimensional scaling), iii) the cause of large scale variability in the observed parameters has been further ad-

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dressed in the discussion with inclusion of the role of upwelling filaments, and iv) some description and the discussion of the results on biogeochemical parameters have been shortened to keep the story more focused.

Individual responses of the reviewers' comments are mentioned below.

Reviewer 1

General comment 1: "a few statements are unclear and certain measurements presented in the methods and results sections lack sufficient justification of their inclusion in the introduction and discussion sections".

Response: We have deleted such statements, e.g., methods and results on Enterobacteriaceae have been deleted.

General comment 2: "In addition, the authors fail to acknowledge a likely cause of the large scale variability observed in their study, that is the offshore export of organic material by the numerous upwelling filaments affecting the study region".

Response: We had only mentioned about the role of offshore transport as one of the probable causes of DOM increase as a phrase in lines 22-25, page 7809. However, in the revised manuscript the role of this important process has been highlighted according to the reviewer's suggestion.

Specific comment 1: "Further justification is needed for why the cultivable population and specifically the *Vibrio* spp. were targeted for this study encompassing the open ocean".

Response: Changes in biogeochemical parameters are likely to affect more to the cultivable bacterial population than the non-cultivable fraction because the cultivable fraction has high metabolic activities while in the non-cultivable part metabolism is attenuated to a great extent. *Vibrio* spp. was targeted because of its link with diseases in human as well as aquatic organisms. Changes in *Vibrio* population in ocean can ultimately affect its coastal population and thus human health. These justifications are

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explained in the third, fourth and fifth paragraphs of Introduction part of the revised manuscript.

Specific comment 2: “It remains unclear if the finding that *Vibrio* positively correlates with DOM holds for other more quantitatively important Bacteria members found in open ocean environments such as the alphaproteobacteria. Does the total bacterial population (represented by DAPI counts) positively correlate with DOM? This can be addressed with this dataset and should be included with a plot in Figure 4”.

Response: The overall bacterial counts represented by DAPI and cultivable bacterial count (CBC) also correlated positively with DOC and DON but the correlations were less strong in comparison to cultivable *Vibrio*. According to the suggestion of the reviewer, this has been addressed in the third paragraph of section 3.3 of Result part and relevant plots have been included in Figure 5 (previous Figure 4) of the revised manuscript. Besides, we have also discussed this issue in the second paragraph of the 4.4 section of Discussion part in the revised manuscript.

Specific comment 3: “Why were measurements of presumptive enterobacteriaceae included in the methods and results sections? No discussion of enterobacteriaceae was provided in the Introduction and a justification of its inclusion in this study is lacking”.

Response: In parallel to *Vibrio* we also measured presumptive enterobacteriaceae to understand whether the variation of biogeochemical parameters can affect the two different bacterial groups in the same manner. We admit that the Introduction and Discussion parts lacked details about enterobacteriaceae. Therefore, following the comment of reviewer1 and to make the manuscript story more focused, we have deleted the methods and results on enterobacteriaceae in the revised manuscript.

Specific comment 4: “The far eastern sector of the North Atlantic is affected by cross-shelf export of recently fixed organic matter to the offshore ocean environment via the numerous upwelling filaments that extend from the west coasts of the Iberian peninsula and African continent along $\sim 15\text{--}45^\circ\text{N}$. These filaments can extend up to 1000 km off-

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shore and carry elevated concentrations of DOM and POM in the surface waters. The transit times within the filaments to the offshore system is rapid and it is likely that the elevated concentrations of DOM and bacterial abundance including *Vibrio* that the authors find in the offshore stations could be explained by entrainment of filament waters into those sampled in this study. It would be possible to assess the degree of impact of the filament waters on the sampled stations using the available measurements of temperature, salinity, chlorophyll, and/or satellite observations of ocean color at the time of sample collection. The overall significance that the entrainment of upwelling filaments waters has on the observed biogeochemical and bacterial variability needs to be assessed. For a description of the physical nature of these upwelling filaments I refer the authors to Barton et al., 1998, 2004; Knoll et al., 2002 and for description of biogeochemical dynamics within the filaments to Gabric et al., 1993; Arístegui et al., 2004; García-Muñoz et al., 2004; Álvarez-Salgado et al., 2007; and Alonso-González et al., 2009 among others”.

Response: We specially thank reviewer 1 for the interesting contribution. According to the suggestion of the reviewer, we have discussed about the role of the offshore export of organic matter via the numerous upwelling filaments as the probable cause of increase of chlorophyll as well as dissolved organic matter concentrations in some mid-Atlantic locations in the second paragraph of the 4.4 section of Discussion part in the revised manuscript. We have also included most of the references suggested by the reviewer.

Specific comment 5: “Pg. 7804, lines 5-22. The cruise track crossed a large gradient in biogeochemical gradients from temperate to oligotrophic to eutrophic systems. Taking an average of nutrient concentrations found in surface waters across this productivity gradient in order to describe the average nutrient regime across the whole study area is misleading. It would serve the reader better to provide a range of nutrient concentrations found in the surface waters of each biogeochemical regime instead of averaged data”.

Response: We agree with this comment of reviewer 1. However, reviewer 2 suggested to concise descriptions on biogeochemical variations. Considering both views, we have avoided description of average values in most cases and pointed out the relative depth-wise fluctuations (section 3.4 of Result part) in the revised manuscript. We have also modified Table 1 with inclusion of biogeochemical provinces in one column so that the readers can understand the range of nutrient concentrations in the surface waters of each biogeochemical regime.

Specific comment 6: “The data presented in Figure 5 are ill-served by a box and whiskers plot. I suggest removal of the boxes and replace with a line graph, leaving the standard deviation bars. Also, depth should be moved to the y-axis, oriented so that the surface is at the top of the plot with increasing depth moving towards the bottom of the plot. The current presentation is confusing to the reader”.

Response: We have modified Figure 5 (now Figure 6 in revised manuscript) as suggested by the reviewer.

Technical comment 1: “Pg. 7794, line 27. Statement beginning with “In a recent study: :.”, this statement is unclear as to meaning; verb tense incorrect”.

Response: The grammatical mistake has been corrected in the revised manuscript and written as "In a recent study it has been revealed. . .".

Technical comment 2: “Pg. 7800, line 14. “The N/P ratio of DIN was mostly below: :.”, this statement is missing a mention of dissolved inorganic phosphorus (DIP) or soluble reactive phosphorus (SRP) so that it reads, “The N/P ratio of DIN to DIP was mostly: :.”.

Response: We have modified the phrase according to the reviewer’s suggestion in section 3.1 of result part of the revised manuscript.

Reviewer 2:

General comment 1: “The horizontal and vertical distribution of inorganic nutrients,

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dissolved organic matter and bacterial abundance is well documented in the literature. Far less is known on the spatial distribution of cultivable *Vibrio* and Enterobacteriaceae, and of the potential environmental factors that control their abundances. This aspect deserves more attention and should be the focus of a re-submitted manuscript that has undergone major revisions”.

Response: We have taken care about this view of reviewer 2 in the revised manuscript, particularly we have given more attention on the regulation of *Vibrio* abundance by various biogeochemical parameters. However, according to the suggestion of reviewer 1, we have omitted results of Enterobacteriaceae and focused more on *Vibrio* spp. Many studies have previously analyzed oceanic bacterial population; nevertheless, most of those analyses were based on culture independent techniques. The effect of the changes in nutrient and other biogeochemical parameters is likely to affect more to the cultivable fraction of bacterial population than non-cultivable fraction because cultivable fraction are metabolically more reactive while the non-cultivable part has very limited metabolic activities. Little is known about the influences of biogeochemical parameters on cultivable bacterial population, particularly *Vibrio* spp. in oceanic habitats.

Specific comment 1: “I suggest the authors shorten substantially the description and the discussion of the results on biogeochemical parameters and total bacterial abundances”.

Response: According to the suggestion of the reviewer, the description and discussion on biogeochemical parameters have been substantially reduced and details on total bacterial abundances have been shortened, e.g., deletions of sentences or phrases in both paragraphs of section 3.1, second and third paragraphs of section 3.3, third paragraph of section 3.4 of Result part, and first paragraph of section 4.1, section 4.2 and both paragraphs of section 4.3 of Discussion part. Besides, some details on biogeochemical part have been deleted in the Introduction.

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Specific comment 2: “Fig. 4: I suggest the authors apply a multivariate analysis to present the correlations among all parameters in a single graph”.

Response: We have included a figure (Fig. 4 in revised manuscript) showing the result of multivariate analysis (multi-dimensional scaling). However, according to the suggestion of reviewer 1, we have modified the previous Fig. 4 (Fig. 5 of revised manuscript) so that individual correlations are also better explained.

Specific comment 3: “Several of the cited references could be replaced by more recent publications”.

Response: In the revised manuscript we have omitted some references published earlier, e.g., Cole et al. (1987) and Sarmiento et al. (1998). We have included some references suggested by reviewer 1 and additionally several new citations, e.g., Longhurst (2007), Constantin de Magny et al. (2008), and Lipp and Westrich (2011).

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