

Interactive comment on “Evidence of high N₂ fixation rates in productive waters of the temperate Northeast Atlantic” by Debany Fonseca-Batista et al.

Debany Fonseca-Batista et al.

dbatista8@hotmail.com

Received and published: 24 September 2018

Biogeosciences Editor's comments

This study was previously submitted to *Frontiers in Microbiology (FiTM)* and was rejected after critical reviews by two referees. Both reviewers are positive about the publication of this study in *FiTM*, although the comments are pretty critical, particularly reviewer#2. So the reason of rejection appears to be that the authors are not able to adequately address the concerns raised by the reviewers. Both reviewers are nice, despite of being somehow critical. I have carefully read the comments and replies to the comments, and reviewed the ms myself. The manuscript could be published only

C1

after major revision suggested by both reviewers.

We would like to thank the Editor for taking the time to review this manuscript himself. We have carefully considered the comments and have revised the manuscript accordingly. We addressed all the comments in a point-by-point manner.

The key concerns are from reviewer#2, and I fully side with him/her. “Overall the text is too speculative- the discussion, although well written, is much too long with 10 pages, and especially the last 5 pages are full of speculation and partly wander too far from discussing actually measured data. It is impressive from how many different angles the authors try to shed light on their data, but when large parts of text end in hypothetical conclusions, assumptions, possibilities, it somehow makes the story implode back onto its core data, and it makes reading the long discussion a bit frustrating. I feel that the few interesting points could be summarized and put into necessary context in a much shorter way. I wonder whether the manuscript could be partly rewritten in order to focus more on the rates and less on the weak nifH data”.

We understand the Editor's concerns related to the comments made by the Reviewer #2 during processing by *Frontiers in Marine Science (FiMS)*. However, we would like to point out that significant modifications had been made in the manuscript prior to the submission to *Biogeosciences*, and further adjustments were now implemented to focus the text on our data and less on hypothetical environmental drivers: (1) the discussion section is now 5 pages (compared to the initial 10 pages mentioned above). The most criticized section related to the environmental drivers of N₂ fixation now only has 2 pages (instead of 5). (2) the main hypotheses put forward to explain the distribution and magnitude of N₂ fixation rates observed in this study were better summarized, made more concise and backed up with additional data from the GEOVIDE cruise that were not included in the last version submitted to *FiMS*. These supporting data include particulate organic carbon concentration in the surface waters and export efficiency (reflecting the potential for shallow recycling) (Lemaitre et al., under review in this same BG special issue for the GEOVIDE project), dissolved iron concentrations (Tonnard

C2

et al., under review in the GEOVIDE special issue), and nifH data on the diazotroph community in the Iberian Basin and West European Basin.

Please note that the nifH sequences recovered from the GEOVIDE sampling have recently been submitted to the GenBank database, and we will soon have the accession numbers for them. These will be added to the manuscript (methods section 2.3) and the phylogenetic tree in Supplementary Figure S1, before publication, if the manuscript is accepted.

The major revisions should be made including: (1) Stay focused on what you want to say. The authors need to focus on N₂ fixation and productive water in this study, and get all relevant placed in the text first. (2) The figures and tables need to be re-structured. In summary, the major re-structuring is required as following. (1) Table 1 might be useful, but table 2 can be placed in the supplementary text.

A major restructuring of the figures and tables was implemented as suggested by the Editor: - both pair of Figures (Fig. 4-5 and Fig. 7-8) have now been merged. - Fig. 6 was removed from the article, the phytoplankton pigments data is now only cited as part of another manuscript in preparation, as follows: Tonnard et al., in preparation. - Table 2 was transferred to supplementary material.

Based on the BG Editor's latest comments, texts were adapted, particularly in the abstract, introduction, methods and results (to add DNA analysis and results from GEOVIDE cruise) and finally in the discussion to focus more on N₂ fixation rates, their significance at the regional and basin scales, and their contribution to primary production.

The key points includes: (1) N₂ fixation rate should be the figure 2, and primary production figure 3. Other figures are then arranged in an order to explain the N₂ fixation and primary production (2) In addition, Fig.7 and Fig.8 can be merged. Figure 4 and Figure 5 might be merged as well. (3) Figure 6 may not be appropriate in this study because it will be published somewhere else.

C3

Figures and Tables were re-arranged as suggested (see above). Concerning the order of figure presentation, we preferred discussing the sections on θ/S diagrams and nutrients and chlorophyll prior to the rates measurements. We believe that introducing the physico-chemical features of the studied region not only allows one to easily grasp the environmental context of the study, but also to highlight specific regions with characteristic traits of interest which will not need to be detailed when discussing N₂ fixation rates and Primary Production.

Other key concerns for example are the following (4) In the abstract, the table 1 was not mentioned which does not echo with the title "productive water"

We agree, the abstract was adapted to focus more on N₂ fixation rates, their contribution to primary production, but also to reduce the attention brought to specific hypothetical environmental drivers.

(5) Line 24 delete the phrase of (38.8–46.5° N; 8.0–19.7° W) in May 2014

The latitudinal and longitudinal range of the studied region was deleted, we just kept "May 2014" to indicate the time of sampling which is relevant based on the importance of seasonality in this work, a condition now better highlighted in the abstract (line 23)

(6) L26-28. This statement is too descriptive

The sentence was modified, to bring the reader's attention to the facts that (1) such high N₂ fixation rates have not yet been observed earlier along the whole eastern Atlantic boundary, (2) elevated rates like these have only been reported at the western Atlantic boundary in very specific nutrient-rich environments such as: coastal, shelf and mesohaline waters. The sentence now reads as follows (lines 29-31):

"In the Atlantic Ocean, N₂ fixation rates exceeding 1000 $\mu\text{mol N m}^{-2} \text{d}^{-1}$ have previously only been reported in the temperate and tropical western North Atlantic waters having coastal, shelf or mesohaline characteristics, as opposed to the mostly open ocean conditions studied here."

C4

(7) L28. Delete the phrase In agreement with previous studies,

The sentence was deleted and the text was adapted as follows (lines 31-35 and 37-39):

“At the two sites where N₂ fixation activity was the highest; nifH sequences assigned to the prymnesiophyte-symbiont *Candidatus Atelocyanobacterium thalassa* (UCYN-A) dominated the nifH sequence pool recovered from DNA samples, while the remaining sequences, as for all the ones recovered from the other sites, belonged exclusively to non-cyanobacterial phylotypes.” . . . “Earlier studies in the Iberian region were conducted largely outside the bloom period, unlike the present work which was carried out in spring, yet in all cases the assessment of nifH gene diversity, suggests a predominance of UCYN-A and non-cyanobacterial diazotrophs.”

(8) L35-38. Please provide solid evidence, instead of proposing something.

The hypotheses are now introduced along with supporting information presented in the corresponding discussion section, regarding the abundance of particulate organic carbon in surface waters (based on Lemaitre et al., under review in the same GEOVIDE BG Special Issue), and on the in situ excess phosphorus data. The text was adapted as follows (lines 35-43):

“Previous studies in the Iberian Basin have systematically reported lower N₂ fixation rates (from < 0.1 to 140 $\mu\text{mol N m}^{-2} \text{d}^{-1}$), as compared to those found in the present study, and this regardless of whether the bubble-addition method or the dissolution method were applied. Earlier studies in the Iberian region were conducted largely outside the bloom period, unlike the present work which was carried out in spring, yet in all cases the assessment of nifH gene diversity, suggests a predominance of UCYN-A and non-cyanobacterial diazotrophs. We support that the unexpectedly high N₂ fixation activities recorded at the time of our study were promoted by the availability of phytoplankton-derived organic matter produced during the spring bloom, as evidenced by the significant surface particulate organic carbon concentrations, and by the presence of excess phosphorus signature in surface waters, particularly at the sites with

C5

extreme activities.”

(9) L39-40. These data appear to be placed in the supplementary materials. Therefore, it is no appropriate to be discussed in the abstract.

We agree, that sentence was deleted.ĀĀĀ

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-220/bg-2018-220-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-220>, 2018.

C6

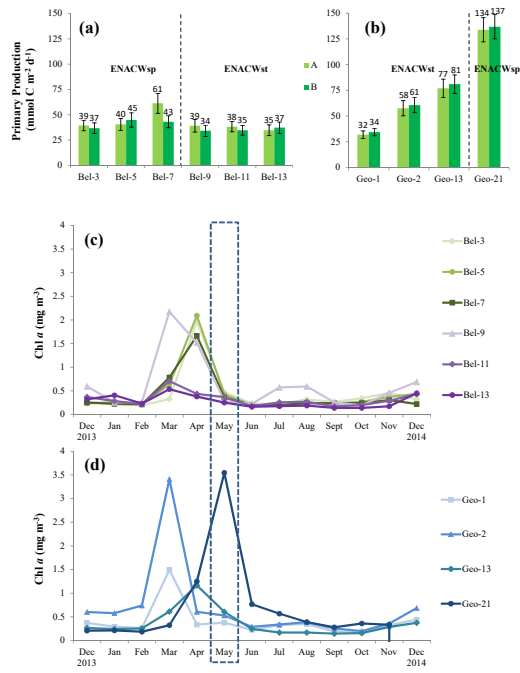


Figure 4

Fig. 1. Figure 4: Spatial distribution of depth-integrated primary production

C7

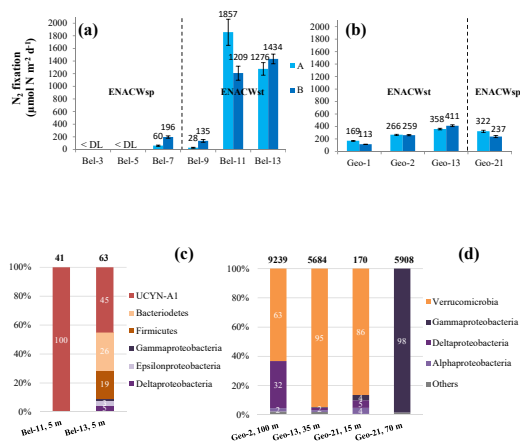


Figure 5

Fig. 2. Figure 5: Spatial distribution of depth-integrated N2 fixation

C8