

Interactive comment on “Integrated survey of elemental stoichiometry (C, N, P) from the Western to Eastern Mediterranean Sea” by M. Pujo-Pay et al.

M. Pujo-Pay et al.

pujopay@obs-banyuls.fr

Received and published: 4 February 2011

On behalf of my co-authors, here are the revised version of our manuscript and the answers to the Referee #1's comments. We thank him for his review that helps us to improve the quality of the manuscript. We have taken into account all his remarks and modified deeply our manuscript according to his comments. We hope these modifications will correspond to his attempts.

1. The previous version has been corrected for language by a native speaker (Alison Murray) and again this revised version has been corrected by a native speaker (UK).
2. We have taken into account this recommendation and we have reorganised the pa-

C4967

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



per around scientific questions. We have modified the introduction, the results section and the discussion has been deeply modified. We added subsections in the discussion with subtitles and a conclusion to the manuscript.

3. We don't totally agree with this remark, because the literature was cited in the manuscript. However, to answer to the reviewer's attempts, we have added more previous values found by other authors. Also, we modified the Figure 8 to compare our nutrient data set to previous works in the eastern and western basins.

4. We deleted some "old" references and checked for recent ones. Most of them were already cited in the manuscript but we have added some more among the abundant literature on the subject.

5. We have now enlarged the fonts in the map and the figures to clarity

MAIN REVIEW

1. As explained previously (point 2) we have reorganised the paper around scientific questions, modified the introduction, the results and the discussion sections. So we have rewritten the abstract according to the new version of the paper

2. Introduction. 3. We have taken into account this remark and we have rewritten large parts of the introduction

4. As indicated in the precedent remark 4, we deleted some "old" references to added recent ones

5. We have thoroughly read the paper to precise each statement with relevant and update references, especially the ones pointed out by the reviewer.

6. The hydrography of eastern and western Mediterranean parts are obviously different, as it was indicated on line 14 p7320 of first submitted paper = "Waters that are formed in different areas within the sub-basins and thus have distinct hydrological characteristics". In order to remove all ambiguity we have modified the text. We have

BGD

7, C4967–C4974, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



briefly described the general circulation and cited key papers for more details.

7. We did not aim to cite an exhaustive list of nutrient sources, but to point out some of environmental events which have an important role on the local fertilization. We agree that the atmospheric components act at a larger scale but also at a local one, so we have added atmospheric deposition to the list to answer to the reviewer' attempts.

8. Again, as explained above, we have reorganised the paper around scientific questions and we have re-written the introduction and the objectives of the paper.

9. Materials and Methods 10. As the list of parameters determined during the BOUM cruise is detailed in Moutin et al. (this issue), we have deleted the reference to “whole set of biogeochemical parameters”. The material and methods section is now focused only on the parameters used in our analysis

11. We changed all “accuracy” in “precision” throughout the text

12. As the samples were not filtered, our data are Total Inorganic Carbon (here not very different from dissolved inorganic carbon in such open ultra oligotrophic environment). We agree with the reviewer and we changed DIC for CT. in the text, graphs and tables.

13. Results 14. As asked by the reviewer, we have added a short paragraph to describe the general circulation in the Mediterranean Sea and a recent reference (Millot and taupier-Letage 2005), and we have modified the Figure 1 (1A for the map and 1B) to show the temperature (map) during the BOUM cruise.

15. As the definition of all these parameters were already described in the Figure 3, we have indicated in the text “see legend of Fig. 3 for definition”. The meaning of “limits” was just how the calculations were made to define the top of the thermocline and the basis of the nutriclines.

16. We used the term homogeneous to indicate that on a vertical profile (1D dimension), the deep nutrient concentrations only slightly vary (in a lesser extend than in the upper layer). As the term was maybe not well appropriate because variations are

BGD

7, C4967–C4974, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



encountered across the basins, we have modified the sentence.

17. We have now indicated that this anomaly is characterized by lower concentrations, after the Sicily strait, in the western basin.

18. We have modified the sentence to clarify it.

19. We have clearly stated in the revised manuscript that DOM is used for DOC, DON and DOP and that POM is used for POC, PN and PP. According to the station, the DOP concentration was undetectable below 150 to 500 m in the western basin and below 200 to 750 in the eastern basin. It is clearly indicated in the text.

20. The average deep concentrations were calculated for depths higher than 1000 m. According to the reviewer's remark, it seems more appropriate to use the limit of the deep layer defined in the following paragraph. That is the reason why in the revised manuscript we finally modified the order of graphs (Fig. 7 becomes Fig. 2) and tables (Table 2 becomes Table 1), in order to define, first the successive layers, and then to calculate the different mean concentrations in each layer.

21. DIC (named CT in the reviewed manuscript) concentrations were given in the Table 2 (now Table 1). We added the CT concentrations in the text, concomitantly to DIN and DIP, and proposed the map of CT along the transect in Fig. 2B.

22. We have modified this sentence and we gave the exact percentage for N and P and deleted the reference to Table 1 (now Table 2).

23. The concentrations are now given.

24. According to this remark, we have only considered the integrated quantities in the BL. Then, we have transformed each integrated quantity in percentages encountered along the whole transect to separate different groups of parameters which have different patterns along the longitudinal gradient,

25. We moved the elemental stoichiometry part in the discussion. We have also mod-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



ified the explanation for the division of the water column to shorten and clarify this distinction. In fact, the choice made on oxygen criterion rather than on water mass type was to avoid introducing a pure physical criterion which is obviously relevant for circulation and export, but not for biological functioning of the ecosystems. For example in figure 1 below, the Levantine Intermediate Water moves in the east from surface (high oxygen concentration, low density) to \sim 500-1000 m depth in the western basin (minimum in oxygen). As we aimed to discuss the differences and similarities in biological functioning between the Eastern and the Western basins, we chose the oxygen criterion which is directly link to biological activity, but also coupled to the general circulation. This choice certainly includes water masses with different origin and/or ages, but we have tested that the range of density was narrow for ML and DL (figure 1 below). The ML is quasi isopycnal, at least in the eastern basin, and roughly corresponds to the LIW in the western basin. We have now justified this choice in the revised version of our manuscript (see fig. 1 added)

26. According to the reviewer's remark, We have modified the Fig. 7 (now Fig. 2).

27. We have now explained this point in the legend of Table 2 (now Table 1)

28. This sentence has been deleted in the revised manuscript

29. We don't totally agree with that remark. Here, the combined graph clearly illustrates the continuity of the relationship from east to west and enlarge the range of nutrient concentrations. It is not uncommon to superimpose N:P plot from different oceans to show the homogeneity of nutrient distribution. It is the same rational than to superimpose the N:P data for all the world oceans (Atlantic, Pacific and Indian; see fig. 2 hereafter). Numerous authors did that. For example, Downing (1997) combined data from open photic zone, estuaries, harbours, bays, deep and open ocean and from organic surface films in his Fig. 1 p 240 (Downing J.A., 1997. Marine nitrogen: Phosphorus stoichiometry and the global N:P cycle, Biogeochemistry 37: 237–252), or Sarmiento and Gruber (2006) used data from WOCE and GEOSECS cruises in their

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Fig. 4.2.1. p120 or in the famous Fig. 5.3.4. p191 (Sarmiento J.L. and Gruber N., 2006. Ocean Biogeochemical Dynamics. Princeton, Woodstock: Princeton University Press. 503 pp. doi:10.1017/S0016756807003755)

30. According to this remark, we have added a paragraph to discuss the seasonal variation of the intercept.

31. The rational is to introduce the figure 9 where all the C:N:P ratios in the DIP, DOM and POM compartment are presented. Even if CT is obviously in excess, these ratios show the difference between west and east basins.

32. Discussion and Conclusions. 33. As explained above, the discussion has been deeply modified. Also we added subsections in the discussion with subtitles and we added a separated conclusion to the manuscript

34. Spatial evolution of the DCM is now shown on Fig. 2

35. We have now given the different values with references

36. The whole discussion has been re-written and divided in subsections with titles to clarify the text.

37. This point has been corrected according to the reviewer's remark

38. We have now developed this idea

39. The Figure 4 (now Figure 5) is a contour map, which means that extrema are generally slightly decreased. The value of 0.08 is the highest isoline indicating that values were higher at the station located in the gyre (only 1 station but 5 depths). The exact concentrations (max/min for each parameter) are indicated in Table 1.

40. The idea is now explained in the text. We meant a same biological functioning.

41. We have now explained this point, the impact of deep water in surface layer is due to winter convection.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

42. The DIM has been defined (dissolved inorganic matter = inorganic nutrient) We have excluded data with concentrations of nitrate and/or phosphate equal to 0. With the criterion of the minimum of oxygen concentration, equations of linear regression are all significant, even in BL. The non-significant relationships are indicated in the Figure 9. The discussion line Page 7336, line 8 has been removed. Note that the regression parameters for BL, ML and DL were presented on figure 9.

43. “It is achieved” in line 16 means “to reach, to fulfil, ...”. We have modified this in the text.

44. We have added a reference to the works of Marty and Chiavérini (2010) in the Ligurian Sea.

45. We changed “budget” by “a synthetic illustration of the biogeochemical functioning of the Mediterranean Sea during the 2008 summer period”

46. Sorry for this misunderstanding, we did not mean that there were not deep nutrient maxima and oxygen minima in the eastern basin (we used this criterion to divide the water column). We have re-written the discussion part and we have also modified this paragraph in taking into account the judicious and pertinent reviewer’s remark concerning the potential role of the EMT in the eastern DL biogeochemistry.

47. Heterotrophic microbial activity consumed DOP in the eastern but also in the western ML. We did not say that DOP is not consumed in the western part, we just differentiated the western basin because the ratios DOC:DOP and DON:DOP continue to decrease in the DL whereas they did not in the eastern basin where ratios are similar in the ML and the DL. We have clarified this statement in the revised version. We do not speak anymore about “threshold” as we have re-written the section. . .

48. These remarks have been taken into account and have been pointed out in the revised manuscript.

49. Indeed, we have taken into account the DOM consumption that goes towards

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

growth, especially in the case of DOP. We didn't draw these arrows in the schematic representation in Fig. 9 to clarify the figure, but these fluxes have been considered in the discussion.

50. We have rewritten the discussion and tried to avoid moving and backing from one compartment to another or one layer to another. We think that the description is now clearer and more focused.

Interactive comment on Biogeosciences Discuss., 7, 7315, 2010.

BGD

7, C4967–C4974, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C4974

