

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

### **Anonymous Referee #1**

Received and published: 23 November 2010

In the manuscript the authors present the concentrations of C, N and P on a transect across the western and eastern Mediterranean sea in the summer of 2008. The concentrations were measured in the particulate and dissolved compartments and included the total inorganic and organic species. It is the first time, to my knowledge, that these concentrations were measured simultaneously in both basins of the Mediterranean sea and worth publication. *However, the manuscript should not be accepted prior to a very thorough review as specified below.*

### **GENERAL COMMENTS**

1. The English should be thoroughly revised. There are certain places in the C3982

manuscript that it is impossible to understand due to poor language.

2. The discussion section is unclear, too long and lacks a logical flow. Parts of it should be in the introduction, parts in the conclusion and some parts should be deleted. Also, it should be split into sub-sections .
3. The authors do not cite any previous published data in the area. One may think that nothing is known previous to this research. There is a wealth of data that can be used to compare the findings in this manuscript
4. The authors do not cite new and relevant literature on the subject
5. Figures 2, 4 and in particular 7 are not clear, with too many contours, values in small fonts.

### **MAIN REVIEW**

1. **Abstract-** Should be rewritten after the objective and discussion of the paper are clarified and the main points identified (see below)
2. **Introduction.**
3. The introduction is not well composed, being too long explaining for example the circulation in the Mediterranean and not touching at all on the available and published C,N,P data in the two basins
4. There are many outdated references and many statements are not backed up by references.
5. Some statements are not precise (for example page 7320 line 8 to 12) .
6. The authors also tend to generalization, equating the hydrography of the eastern Mediterranean with the Western Mediterranean (page 7320)

7. While citing sources of nutrients the atmospheric component is ignored (page 7321, line 7)
8. The aim of the paper is unclear
9. **Materials and Methods**
10. Please give in detail the "whole set of biogeochemical parameters" (page 7322, line 16)
11. How was nutrient accuracy determined? Maybe the authors mean precision? (page 7323, line 2).
12. DIC- is it total inorganic carbon or dissolved inorganic carbon (page 7323, line 14)

13. **Results**

14. It does not address, even briefly, the physics during the cruise and should be given here. The mesoscale features in particular are very important to describe the distribution in the upper water column. In addition, the Levantine intermediate water (LIW), formed in the Eastern Mediterranean, enters the Western Mediterranean through Sicily and was shown to affect the distribution of the physical and chemical parameters in the Western Med. Changes in deep water circulation in the eastern Mediterranean affected the chemistry of the whole water column as shown in many published papers. Therefore, the statement "the present study is not discriminating for different areas of precise water masses. ...." in my view is incorrect and the results should be presented based on the physics (Page 7325, line 2, see also comments on English language)
15. **Dissolved inorganic nutrients section.** There are many terms used and not defined or without data shown. For example, depth of thermocline is mentioned  
C3984

at the first sentence of the section but only in the next paragraph we see the data on figure 3. DCM is mentioned without data or depth. The nutrient depleted layer, the nitracline, the top of the phosphocline are not defined at all. The authors should add the information on how those were determined and if they are equal for both basins. Also, in figure 3 what is the meaning of limits?

16. The deep concentrations of phosphate and nitrate are not very homogenous across the basins (page 7325, line 24)
17. Page 7325, line 25. Sicily is shallower than 1000m . Also, explain what is the anomaly: higher or lower concentrations.
18. Page 7326, lines 11-15 is unclear.
19. **Dissolved and particulate organic matter.** There is mention of DOM and POM but no explanation of their meaning in the paper. Please add. Is DOP below detection limit below 300-500 m also in the eastern Mediterranean?
20. How were the average deep concentrations of DOC and DON calculated? What is the definition of deep waters? (Page 7327, line 1)
21. DIC is mentioned but no concentrations are given in the paper, only ratios. The concentrations should be given (Page 7327, line 3)
22. Table 1 do not show the proportions (ratio) of mineral to organic forms (Page 7327, line 23)
23. "The highest concentration for C,N. . .". Please give values (Page 7327, line 13)
24. The rationale of comparing the integrated values over the whole water column and the average T and S is unclear (Page 7327, line 24-26). It makes no sense to integrate over different water masses that occupy different water levels at the

different stations. Moreover, the same trend can be shown using concentrations, or average concentrations at water masses. I recommend deleting the paragraph and figure 6.

25. Evolution of elemental stoichiometry should be part of the discussion and more concise and relevant published data cited. Also, the explanation of the division into BL, ML and DL is too long and confusing.
26. Figure 7 is unclear. There are two white lines that determine different limits. Also dissolved oxygen concentration is given in two different units:  $\mu\text{M}$  (Table 2) and  $\mu\text{mol/kg}$
27. Table 2 – Sigma - I assume is standard deviation. It should be explained in the table.
28. The results that eastern waters are “warmer, saltier, . . . poorer in nutrient . . .” is trivial and have been published widely. If the authors want to keep the sentence they should add – as expected, as published before and cite some of the references. (Page 7329, line 5)
29. Figure 8. What is the rationale of combining the east and west data into one? I would recommend deleting the combined graphs.
30. “The intercepts indicate a concentration of  $0.3 \mu\text{M}$ . . . .”. (Page 7330, line 14) This is true for the eastern Mediterranean in the winter and not in the summer as shown by Krom et al., 1991 and others afterwards. The authors should discuss this.
31. Please explain what is the rationale to look at DIC:NO<sub>3</sub> and DIC:PO<sub>4</sub> ratios
32. **Discussion and Conclusions.** The section should be split into Discussion alone and conclusion alone. The first paragraph should be part of the conclusion and/or the abstract, without citing the references that should be part of the introduction.

C3986

33. The discussion should be divided into sections. It is very hard to follow the arguments and the logic of the discussion at the present state
34. (Page 7331, line 25) west east transect instead of spatial. Also, DCM data were not shown in the manuscript
35. “The nutrients reached classical concentrations. . . .” (Page 7332, line 3). Cite references and give values. The same is true for the other variables measured
36. Page 7332 line 13 to page 7333 line 18. This whole section is long and unclear. Do the authors want to emphasize the location of the nutriclines? The differences between the basins? The supply from below? The authors should emphasize the point they would like to make and cite the relevant results and literature.
37. Page 7334, line 5. “Aerobic decomposition. . . .” Incorrect. It should be preferential remineralization and fast utilization rates
38. Page 7334, line 20. “At very low nutrient concentration, DOM could accumulate. . . .” What do the authors mean by DOM? DON, DOP? Why would DOM accumulate in surface waters? It is known that the microbial community can utilize DON and DOP instead of inorganic nutrients when the latter are not available.
39. Figure 4 does not show DOP values of  $0.1 \mu\text{M}$  at the surface as written in Page 7335, line 4. The maximal value is  $0.08 \mu\text{M}$  at one station only.
40. What are “biological” similarities? Page 7335, line 12.
41. How can the deep water impact the surface layers? Please explain Page 7336, line 2
42. Page 7336, line 3. What is DIM? How were the C:N:P calculated? Most of the phosphate and nitrate data in the BL were below detection limit. In addition, the

C3987

authors do not show the regression for BL, ML and DL alone but discuss it (Page 7336, line 8)

43. "It is achieved. . .". What is achieved? (Page 7336, line 8)
44. Sentence in page 7336, lines 22-26. No reference to the assumption
45. Page 7336, line 27. Define "budget"
46. Paragraph starting in Page 7336, line 27 ending at the end of page 7338 and figure 9. I have many problems with the statements made here. For example, page 7338, line 25 –' . . . there is no clear nutrient maxima in the ML of the eastern basin . . . . and OMZ diffuse towards east. . ." these statements are wrong. There are nutrient maxima in the eastern basin and oxygen minima, that are a result of the changes in circulation in the area following the EMT (Roether et al, 2007, Klein et al, 2003, Kress et al 2005 to cite a few of the numerous papers published). Also, in page 7338, line 3, the depletion of P in the west compared to the east could be due to the transient EMT that introduced younger waters to the deep layers. Otherwise, it is not reasonable that the west should be poorer in P than the eastern basin
47. How (by whom) is DOP consumed in the ML in the eastern basin? Why not in the western basin? And what to the authors mean by threshold? (page 7338, line 8)
48. Paragraph starting in Page 7336, line 27 ending at the end of page 7338 and figure 9. The authors should point out that the proposed scheme is true for the summer alone for the BL and part of the ML. It is know that the upper layers are very dependent on the season. Moreover, the calculation of the ratios for the BL is very problematic, due to the large (I assume) data points that were below detection limit.

C3988

49. The authors do not take into consideration the utilization of DOP and DON that goes towards growth – DOP and DON that was shown to occur at many areas of the oceans.
50. Figure 9 and the discussion are hard to follow. In the discussion, the authors move among compartments (mineral, DOM and POM) and ratios (C:N, C:P, N:P) without a logical pathway.

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C3989