

Interactive comment on “The Biogeochemistry from the Oligotrophic to the Ultraoligotrophic Mediterranean (BOUM) experiment” by T. Moutin et al.

T. Moutin et al.

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Introduction : We appreciate the important work done by the 2 reviewers of this manuscript and before giving detailed answers for each question, we propose, as was suggested, to divide the manuscript into two. The first manuscript will be a short one introducing the BOUM experiment and synthesizing its main findings. Our aim is to present in this introductory paper the general context which is not restricted to the scale of the Mediterranean Sea (MS). The second manuscript will focus on the influence of anticyclonic eddies in the biogeochemistry of the MS (description of the eddies, the property distributions and their internal dynamics, first order budgets). The

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two manuscripts are intended to become the first and second papers of the BOUM special issue. As was also suggested, the overall synthesis of the BOUM results will be postponed.

After a general introduction, our initial manuscript described the physical conditions encountered during the BOUM cruise and gave the main biogeochemical trends of the MS. It has been available for the other authors of the BOUM special issue for a long time on an internal web site and it has thus been cited by almost all of them. The division of the manuscript into two will require that all these prior references now be checked in order to ascertain their source as being the first or the second manuscript, of course if they are accepted.

Considering the changes requested and our very tight timetables, we will not be able to propose the new manuscripts until, at best, the end of February.

MRA : This long manuscript has several objectives: 1. to illustrate the objectives of the BOUM experiment and how the cruise plan was implemented 2. to describe the water mass properties and their distributions as well as the general biogeochemical trends encountered, building on the description of the sections of different parameters 3. to analyze the main physical characteristics of the three anticyclonic eddies studied during the experiment 4. to compute first order biogeochemical budgets within the eddies 5. to provide a general overview of the 24 other papers published in the special issue devoted to BOUM experiment BOUM experiment had as a general theme: the interactions between planktonic organisms and the cycle of biogenic elements in the Mediterranean Sea (MS), in the context of global climate change and, more particularly, on the role of the ocean in carbon sequestration through biological processes with three main objectives: (1) the longitudinal description of the biogeochemistry and the biological diversity of the MS during the strongest stratified period (2) the study of processes at the center of three anticyclonic eddies (3) the representation of the main biogeochemical fluxes and the dynamics of the planktonic trophic network. Having read the comments by Mike Krom, the other reviewer, most of which I agree with I will

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only highlight which other parts of the manuscript are not convincing and what can be done to solve the problems raised by Mike Krom and myself. I will list minor things (e.g., typos, problems with references, etc.) which may be of some help to the authors, at the end of this report. My first comment is that the objectives of the manuscripts are too many, especially because they reflect different levels of information and synthesis, which are not hierarchically linked. The manuscript is too long and quite difficult to read and likely reflects the different style and expertise of the authors. My suggestion is to make at least two manuscripts, one focused on the description of the eddies, the property distributions and their internal dynamics, and the other with the scope of introducing BOUM and synthesizing its main findings.

The authors : We recognize that the manuscript was not simple to review and apologize for that. We explain the major changes proposed in the above introduction. We respond to MK comments in a separate document.

MRA : The synthesis of BOUM findings is a weak point of the paper, with some schematic statements that contradict other parts of the manuscript. The authors may choose, as it happened for other large experiments, to postpone the synthesis to another time, producing a synthesis paper for a prominent journal and to restrict the introductory paper to the main questions, the strategy and a short description of the other papers of the special issue.

The authors : The contradiction - which is not really a contradiction, but rather a bad introduction of the N2 fixation question - will be completely reworked. As proposed by the reviewer, we have chosen to postpone the overall synthesis to another time and will follow the schedule proposed for the introductory paper.

MRA : The introduction is too long and the text written would be more suited for a review. The points raised in the first paragraph may be easily summarized in short sentences, especially because some of the topics, e.g., complexity of the food chain, the role of mesoscale dynamics, which in the cited references is mostly sub-mesoscale,

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ratio between production and remineralization, are not specifically discussed in the text.

The authors : All these topics are the subjects of the other papers of the special issue and, as such, need to be introduced. Section 1 is not the introduction of the paper is merely but the general context introducing the BOUM special issue. The term introduction may not be the most well-chosen term in English and will be replaced by "General context". This paper will be the first of the special issue. We understand that it may not be easy to understand why it has been submitted last and why it has been cited by the authors of the other papers in the special issue. However, ours is the last paper to be submitted because we have had to wait until all the papers were submitted in order to present them. Our paper was cited by the other contributing authors of the special issue because we had made it available for consultation by the BOUM "community" on the BOUM web site a long time before.

MRA : The section on Mediterranean biogeochemistry can also be heavily shortened, trying to highlight the issues that can be addressed on the basis of BOUM findings. The paragraph on the sections can also be shortened if authors agree on providing only a general overview of BOUM scenario, thus discussing the detailed analysis on the eddies in a separate paper. This also because the only needed information coming from the sections is the hydrographic context at the time of the cruise. In fact all the other aspects confirm what has already been found in other Mediterranean cruises, e.g., the W-E gradients, the deepening of the DCM etc.

The authors : we will reorganize the description of the biogeochemistry as this description is central for the other papers. Numerous citations which are integral to the other papers of the BOUM special issue are drawn from this part of the manuscript: for this reason this section cannot easily be shortened.

MRA : Then in the description of the other papers I stimulate the authors to recall the BOUM questions and mention the other papers and their 'take home messages' in close link with those.

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The authors : This was done according to the 3 main objectives of the BOUM cruise for reasons of simplicity. We will proceed according to the questions in the revised version.

MRA : They should also devote some text to reconcile or at least discuss contrasting results on the in situ Nitrogen fixation and presumable P limitation and on deck experiments.

The authors : yes

MRA : This paper should be relatively short (around 10-12 pages) and guide the reader to explore the other papers. I would then extract part of section 5.3, section 5.4 and section 6 to merge them in another paper.

The authors : As suggested, we will separate the paper into 2 papers : Paper 1 : introduction to the BOUM experiment Paper 2 : Influence of anticyclonic eddies on the biogeochemistry along a longitudinal transect in the Mediterranean Sea (BOUM cruise). The sections will be described specifically in order to show the influence of the mesoscale eddies. Please note that numerous other manuscripts of the BOUM special issue will refer to this 2nd paper in order to present their work in the context of the main biogeochemical trends observed during the BOUM cruise.

MRA : I suggest to carefully consider the comments of Mike Krom for what vertical dynamics within the eddies concerns, and I would focus the discussion on the role of those eddies in Mediterranean biogeochemical dynamics. I highlight again that for part of the community mesoscale is more related to instabilities, meanders, ageostrophic processes than to those long lasting, basically geostrophic structures and this aspect would deserve some comments.

The authors : Yes, this will be done in the discussion of the 2nd manuscript. Eddies may come from instability of the basic flow : the result is a long-lasting mesoscale structure. The size of a mesoscale eddy is in the order of magnitude of the internal radius of deformation.

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MRA : I also suggest to remove all the analysis on water mass transformation from the synthetic paper and to analyze them in a third paper. How much and how young LIW is present, here and there, is an interesting aspect for clarifying the internal dynamics of the EMED, but does not add to the main focus of the experiment.

The authors : We are disinclined to put this part into a separate manuscript. Instead, it will be rewritten and the figure will be presented in more detail in order to show the main water masses encountered during the BOUM cruise. Although we think that it is interesting and necessary to give such a description, we will not focus our discussion on LIW and the internal dynamics of the EMS.

More specific issues MRA : On p.4 l.6-7 Sargasso sea is described as a low-P low chl area? Again so on p.5 l. 20-21. Would it be possible to add a references showing that it is similar to MS, as assumed by the authors?

The authors : On p. 4, we will add the references of Wu et al., (2000) and of Lomas et al., (2010) , already cited further on in the text. Wu, J., Sunda, W., Boyle, E. A. and Karl, D. M.: Phosphate depletion in the western North Atlantic Ocean, *Science*, 289, 759–762, 2000. Lomas, M. W., Burke, A. L., Lomas, D. A., Bell, D. W., Shen, C., Dyrman, S. T., and Ammerman, J. W.: Sargasso Sea phosphorus biogeochemistry: an important role for dissolved organic phosphorus (DOP), *Biogeosciences*, 7, 695-710, doi:10.5194/bg-7-695-2010, 2010.

MRA : On p.4 l.18-20 ‘..approaches zero.’ is ambiguous and there is no mention to a decreasing depth which could link the explanations that follows

The authors : approaches zero (i.e. below the classical chemical detection limit of 50 nM). Unfortunately, we do not understand the 2nd part of the sentence.

MRA : On p.4 l.23 Atlantic water is considered nutrient rich. I don't know on which data the authors base this statement. A significant part of the nutrient that re-enter the basin come from entrainment of the exiting IW. The authors also mention the Rhone as

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a major source of nts. Indeed it is lower than Po contribution (Ludwig et al., PO, 80, 199, 2009), even if scaled to the volume of the basin. Therefore it cannot be the only explanation for the different trophic regime of the WMED vs.EMED.

The authors : Yes, the surface waters of Atlantic origin entering the Mediterranean are 'nutrient rich' when compared to the surface Mediterranean waters which are more depleted in nutrients: this is clearly shown by the decreasing concentration in the upper layer from the Atlantic to the MS (Moutin et al., 2002). It is nevertheless true that there is a net export of nutrients from the MS because deeper waters flowing outside the MS at the Straits of Gibraltar are 'nutrient rich' compare to surface waters entering in the MS.

For the sake of clarity, we have rewritten the following sentence : "This is related to hydrological conditions (higher winter convection in the western basin) and is also the result of the two major external sources of nutrients, the Rhône river input and the entry of the nutrient rich Atlantic surface waters, being located in the western part of the MS." The revised version follows: "This is mainly related to hydrological conditions and particularly to higher winter convection in the western basin."

MRA : On p.6 l.19 please clarify that N:P is the ratio in the stocks

The authors : we will add concentrations after deep Mediterranean waters

MRA : On p.6 l.28-31 the authors write that: 'As far as deep water is concerned, nutrient exchanges at the Strait of Gibraltar and at the Strait of Sicily, in combination with the large vertical variation of nutrient concentrations, appear as key factors in the understanding of the nutrient budget of the MS (Moutin et al., 2002)' This sentence sounds very unclear to me. At least they should clarify that they refer to the intermediate water. There is no exchange of DW among the basins and with the Atlantic, besides the entrainment in the intermediate flow

The authors : Indeed, this is not clear. We will delete the beginning of the sentence

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:"As far as deep water is concerned".

MRA : on p.6 l.32 why anti-estuarine circulation is unusual?

The authors : This is a citation. It is unusual because normally less dense waters flow outside. This is usually not the case for adjacent seas where circulation is estuarine with large inputs of fresh water (Black Sea, Baltic Sea. . .)

MRA : On p.7 l.1-8 the authors stress that the export at Sicily is an explanation of low P concentration and of the large P deficiency. I believe that as written this statement may be misleading, because low P concentrations are due to the low inputs (main cause) and to the internal dynamics of the basin that allows a very limited number of P turns (between surface and subsurface layer) before exporting it. In other words it is not that the statement is wrong but it would be better to formulate it differently.

The authors : yes. In any case this part will be deleted.

MRA : On p.7 l.13 I don't think that the P scavenging hypothesis was proposed by Krom and colleagues in the 1991 paper

The authors : last sentence from Krom et al., (1991):" Our preliminary findings suggest that removal of P by absorption onto mineral particles may be an important process in the eastern Mediterranean and demonstrate the need for a rigorous test of this hypothesis."

MRA : On p.7 l.14-32 the authors discuss the N-fixation, See the extensive comments by Mike Krom. I only remind that another paper by Sandroni et al. (Deep-Sea Research I 54, 2007, 1859–1870) conducted a more extensive discussion on the Dyfamed N-fixation time series.

The authors : In our response to MK's comment, we explain the reason why we presented Nitrogen fixation like we did and how we will modify the text as requested.

MRA : On p.9 l.7-10 the cruise plan is presented with the opposite direction than at p.1

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I.25. It would be better to use the same criterion

The authors : OK

MRA : On p.10 I.10-12 could the authors clarify what was defined by Lacombe et al. in relation to the Aegean Deep Water? As it reads it seems that they had predicted such an event, which to the best of my knowledge is not the case.

The authors : Lacombe et al. did not predict EMT events but they announce the episodic existence of water masses forming a scorpion tail at the lower extremities of the TS diagrams.

MRA : On p.10 I.18-21 it is not clear in respect to what no decrease in salinity was observed. I also note that the current view of LIW formation is that it is not restricted to one place, with the edge, not the interior, of the Rhodes gyre being among the most important sites.

The authors : yes, this will be modified.

MRA : On p.11 I.14 and I.24 the use of 2 days in the suffix and of 3 days afterwards may be confusing. Likewise why on p.11 I.24-25 you assume that the time is 200 days? It obviously depend on the time course of buoyancy fluxes

The authors : The method allows to calculate the MLD for each minimum of cumulated surface buoyancy flux for a given window of time(2 days, 3 days or 200 days). This will be clarified.

MRA : On p.13 I.9 the authors define as somewhat constant the depth integrated chlorophyll. To me it seems to vary in the same order of magnitude than the other variables that are not considered constant

The authors : we wrote:“This value remains somewhat constant, as already observed by Moutin and Raimbault (2002)”.We didn't propose the same kind of figure, but in Moutin and Raimbault (2002), fig. 7 shows clearly that depth-integrated chlorophyll is

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somewhat constant, unlike the other variables. The same kind of figure could be drawn with BOUM data, but we tried to condense the information.

MRA : To solve the problem of the layers below 150 m (p.13 I.11-12) calibrated fluorescence could be used, and this would overcome the uncertainties.

The authors : we chose 150 m for integration of stocks as was already done for the MINOS cruise data. We are aware that this choice may be debatable, but we felt it was the best compromise to give a first order relationship between biomass and nutrient.

MRA : On p.15 I.13-16 it is written that: 'On the contrary, the layer with a maximum in AOU corresponds to the layer of LIW flowing into the western MS through the Sicilian Channel. This is because LIW is closer to the surface in the eastern MS and not completely isolated from the influence of atmospheric oxygen.' This is hard to understand to me, because after leaving the site of formation there is no more exchange of oxygen with surface, even in the EMED.

The authors : We will rewrite the sentence. In the eastern Med, LIW is a young water with low AOU and is located relatively closely to the surface. Maximum of salinity are close to the photic zone depth and may be ventilated during winter. By spreading in depth toward the West in the Ionian Sea, this water receives organic matter which is partly mineralized.

MRA : On p.15 I.23-25 it is highlighted that AOU is high in the new EMDW. Indeed, what is worth noting is that AOU is still lower than the old EMDW

The authors : yes, we agree and this will be clarified.

MRA : The text at p.16 I.1-9 is quite confusing. It is hard to evaluate differences just using the palette in fig. 5. However it is not clear why the authors believe that the new waters which are very dense should affect the 0-1000 layer

The authors : yes, we agree. We will add more details to figure 5, 6 and 7 and reorganize this part in order to help the readers. A discussion of this part will be added.

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MRA : On page p.16 l.28-31 the authors note that P* suggest an utilization rate close to the Redfield ratio in deep waters. They attribute this to deeply sinking POC (diatoms) with a composition closer to Redfield ratio. This is an interesting hypothesis, but having the large data set of BOUM they should have the tools to test the hypothesis more in depth.

The authors : it is not the purpose of our introductory paper to go into this hypothesis in more depth. Please note that MK does not seem to share an interest in this observation. We prefer to stay on the level of the hypothesis.

MRA : On p.17 l.23-24 the authors assess that nitrate removal can be only due to photosynthetic activity. Did the authors consider heterotrophic assimilatory uptake of nitrate (e.g., Andrew Allen PhD thesis, NITRATE UPTAKE BY HETEROTROPHIC BACTERIA AND THE DIVERSITY OF BACTERIAL NITRATE ASSIMILATION GENES IN MARINE SYSTEMS , 1996)?

The authors : The $^{15}\text{NO}_3$ isotopic method did not allow to differentiate what is assimilated by bacteria and phytoplankton. Nevertheless, it is generally admitted that nitrate is not the preferred substrate for bacteria in the well-oxygenated photic zone. Similar uptakes were observed on GF/F and 0.2 μm Anopore filters in an experiment conducted during the OLIPAC cruise (P. Raimbault, pers. com. and references below) although biomasses were different. Around 40% of bacteria were considered to pass through GF/F filters while they are retained on 0.2 μm filters (D. Vaultot, Pers. Com.). Raimbault P., Slawyk G., Boudjellal B., Coatanoan C., Conan P., Coste B., Garcia N., Moutin T., Pujol-Pay M., 1999. Biomass, new production and export in the equatorial Pacific at 150°W: Evidence for intense nitrogen recycling. /J. Geophys. Res./104, 3341-3356. Raimbault P., Slawyk G., Garcia N, 2000. Comparison between chemical and isotopic measurements of biological nitrate utilization: further evidence of low new production levels in the equatorial Pacific. /Mar. biol./, 136 :1147-115

MRA : On p.24 l.12-14 the authors state that mesoscale is strong enough to delete the

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West-East gradient in trophic conditions. This came from the observations in the three eddies. I think they should synthesize here what is their view on how it may work.

The authors : This will be done in the 2nd manuscript

MRA : On p.24 l.22-25 vertical stability is taken as an expected feature of an anticyclonic eddy. I think that it is not always the case, because stability, in a wide sense, is related to the density gradient and kinetic energy input. I would interpret the observed stability more as due to the latter (low kinetic energy input).

The authors : we did not understand this remark, there is nothing about stability p. 24 l 22-25. We only speak about the vertical density gradient.

MRA : I did not find any definition of C_p which is analyzed on p.24

The authors : It is presented in the legend of Fig. 11. It will now be presented in the method section.

MRA : I agree with the comment of the authors on p.28 l.30-31 that subsurface blooms may be missed by satellite, which implies that more information is needed to improve the characterization of the Mediterranean trophic regimes. But a parallel reconstruction of the dynamics of the likely sub-surface bloom, may help in figuring out the relevance of such episodes

The authors : we agree with the reviewer that a parallel reconstruction of the dynamics of the likely sub-surface bloom may help in figuring out the relevance of such episodes

Typos and biblio

p.23 which leads leading p.24 l.25 shown Please change j (jour?) on p.27 l.7-9 with d (day)? p.6 l.21 it is not 1991a p.7 l.4 Bethoux et al 1998 not in the ref. p.10 l.17 Lascaraetos is Lascaratos p.14 l.11 Mauriac 2011 which one? p.14 l.18 It should be Krom et al., 2005b p.17 l.19 Cuyppers in preparation ??? p.29 l.15 Minas is Minas et al Brenner et al 1993, Moutin et al, 2005 are in the list but are never cited in the text

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There are a few citations of papers in preparations, e.g., Mauriac et al, 2011. I think that the policy of BGD is to avoid citations of papers in preparation which cannot be accessed. It would be better to refer to personal communication with the coordinates of the person to refer to.

The authors : All these remarks will be taken into account in the new manuscripts.

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