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Biogeosciences Discussions

Interactive comment on "The Biogeochemistry from the Oligotrophic to the Ultraoligotrophic Mediterranean (BOUM) experiment" *by* 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

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.

MK : General Comments: This has been a very difficult manuscript to review. It is an attempt to provide both a biogeochemical synthesis of important processes in the MS using the previous work by Moutin and Raimbault (2002) as a model, and an overall synthesis of the most important results of the entire BOUM cruise. I feel they have got the balance wrong with too much space being spent on the synthesis and not enough summarising the important results from the cruise.

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.

MK : I think overall the manuscript needs a considerable rewrite for two fundamental reasons. The manuscript as written, attempts to answer the question of how the Mediterranean Sea (MS) system "to determine the actual efficiency of the biological pump and to forecast its future efficiency, major challenges today concern both biogeochemical, physical and biological oceanography. Major questions arise of which one is central: What is the balance between production of organic matter in the photic layer and remineralisation in the upper layer." I would argue that such an ambitious question cannot be answered by a single cruise to the MS particularly when the cruise took place at during the time of year when primary productivity and hence carbon uptake and export are at a minimum. As such the text for the first 30 pages is too long.

The authors : We agree with the considerable rewrite. It is clear that the ambitious questions addressed in the “introduction” cannot be answered by a single cruise. We find it surprising to see that MK reduces the questions to the MS. It is not because it was done in the MS that the results will not be interesting for people working elsewhere in the Ocean. Indeed, many reviewers of the other manuscripts of the BOUM special issue didn’t work in the MS. This introduction was to present the general context at the scale of the open ocean, to present some particularities of the MS, and to specify which specific questions could be addressed in the MS with regards to this general subject. The term “introduction” was probably not well-chosen. The specific objectives of the BOUM cruise were detailed in section 3 “objectives of the BOUM experiment”. We agree with MK that the first pages are too long and will shorten them. Fig. 2 will be moved and some parts will be omitted as described below.

MK : In addition the authors have failed to take advantage of previous work to put their study in context. Indeed in several places they have ignored or misrepresented previous work particularly on the Eastern Mediterranean Sea (EMS).

The authors : Our general context was voluntarily not to the scale of the MS and it was not an objective of this introductory manuscript to give an exhaustive overview of the numerous works done in the MS. Nevertheless, 5 papers from M. Krom and 1 from S. Brenner dealing specifically of the eastern MS were cited, i.e. considerably more of the abundant literature from the western MS , which could have been misrepresented in our manuscript. We really do think that the term “ignored” was very harsh and does not correspond to reality. We will nevertheless follow this remark and will add more details of previous works in the eastern MS.

MK : This has both produced some very odd inconsistencies in the manuscript con-

cerning particularly the relative importance of nitrogen fixation. It has also meant that their attempt at creating annual budgets include some incorrect assumptions which would not have existed if the previous literature had been used to support their study.

The authors : MK is right that there are inconsistencies in the manuscript, particularly concerning the relative importance of nitrogen fixation. This is because the role of nitrogen fixation was one of the initial focuses of the BOUM project and was hypothesized to play an important biogeochemical role in the MS before 2008. Very high rates were “measured” during the CYCLOPS experiment in 2005! Therefore, we proposed a possibly important role of nitrogen fixation in the introduction, even if several papers published after 2008 have since shown very low levels of nitrogen fixation, as has also been found by us (Bonnet et al. this issue). We recognize that it is not what have to be done, and will follow the correction proposed by MK.

MK : Conversely the very important results of the BOUM cruise (24 papers in all) are summarized in only 3 pages of double spaced text. As a result many papers get no more that a reference with no information given on what important information the paper reaches. This is an important opportunity missed. Not only does it mean that as a reader we know what information we could get by reading the individual papers but also the opportunity to use the conclusions of the BOUM cruise to help to carry out the ambitious questions set out at the beginning of the introduction has been lost.

The authors : as previously said and specified in our introduction and abstract, it was one of the objectives of this introductory manuscript to summarize the main results of the papers that will follow in the special issue. We never planned to make a general synthesis of all the work done during the BOUM cruise. We plan, as proposed by the other reviewer, to do that later, in another paper. We merely proposed a very simple first order budget.

MK : In addition there are some problems in the way the manuscript is organised and the data presented which should be dealt with.

The authors : We have dealt will this and propose some modifications which are detailed below. The most important are the separation into 2 manuscripts and the creation of a material and method section for the 2nd manuscript.

MK : Detailed comments: Abstract: The abstract is only about what was planned in the manuscript and not what was actually found. What are the major conclusions of the manuscript?

The authors : We will follow the schedule proposed by Claustre et al. 2008 in the introductory paper of the BIOSOPE special issue in Biogeosciences (http://www.biogeosciences.net/5/679/2008/bg-5-679-2008.html) for the BOUM special issue. It will be a very short paper, as suggested by reviewer 2. The results included in the manuscript will now be presented in a 2nd manuscript which will be set out like a classical paper.

MK : Introduction: This reads like an unmodified introduction to a grant proposal not a manuscript. An introduction is supposed to specifically set the background for what the manuscript is about, what the problems are so that the reader can follow the arguments which follow. This introduction does not follow this fundamental rule.

The authors : it is an introduction for a special issue with 24 other papers, not for one paper. We understand that the term introduction was not well-adapted and propose to change it to “general context”. We think that we have to introduce why there is, for example, a paper on “parasitism”, another on “vertical eddy diffusion measurements”, another on “optical measurements allowing to quantify primary production from space”. . . The overall goal of the BOUM experiment was a better understanding of the biological carbon pump in oligotrophic environments, not a better understanding of the functioning of the MS, even if we can expect to partially achieve the second objective by reaching the first. We think that the separation in two ms will make this clearer.

MK : Page 2 line 15-16 The timescale for reaching equilibrium within the upper layer is about a year (Kleypas and Langdon, 2000). Is this not location dependant. I cannot

find the original paper since it is a book.

The authors : the paper is available via the following link (http://www.isse.ucar.edu/staff/kleypas/docs/PUBS/kleypas_langdon_AGUmono_CH05_200). A year is an order of magnitude. It is location dependant because it depends on the time the waters stay close to the surface before being transferred to the ocean interior, which is not the same everywhere. It is known, for example, that the subantartctic water (AAIW) stays less than one year close to the surface, which explains why the equilibrium between the upper water and the atmosphere is never reached at this location.

MK : Page 2 line 28 There is no such word in English as ‘oligotrophication’. What they mean is that the global ocean is likely to get more thermally stratified.

The authors : Not exactly, fewer nutrients are available as a consequence of increasing stratification. oligotrophication : The process of nutrient depletion, or reduction in rates of nutrient cycling, in aquatic ecosystems. In : “A Dictionary of Plant Sciences” from Michael Allaby, 1998.

MK : Page 4 line 8. They suggest that there will be a decrease in phosphate availability as a result of climate change. This is based on Moutin et al (2008) paper on P availability in the central Pacific, an area with high nitrogen fixation and low dust input. This is profoundly different from the Mediterranean which is an area of high dust input and low to zero nitrogen fixation. Globally I would expect P supply to increase both due to increased dust input, increased fertilizer and sewage input and changes in the bioavailability of atmospheric supplied P. Whether there might be changes in the relative availability of N and P (inputs) as a result of these environmental and climate change is an interesting question – which in any case is not considered in this manuscript.

The authors : The assumed main source of nutrients in the photic zone is the mixing of upper water with deeper water in the open ocean, as in the open MS. The expected increasing thermal stratification will decrease this input – and therefore phosphate availability – in the upper layer. It is true that following thermal stratification increases, the relative importance of external sources may increase, but in our opinion, this may be relevant for P in the coastal areas only (see Moutin, 2010 available here: http://www.ciesm.org/online/monographs/Tunis.html). It is clear that it is an interesting question but not a question considered in this manuscript. Following the paper by Moutin et al. 2008 which is centered on P availability in the central Pacific but which presents results from the world ocean, it appears that the MS is characterized by a very low P availability. Our intention was to explain that it is interesting to work in the MS even if it is a puddle in the scheme of the world ocean for many known reasons (deep water formation, strong oligotrophic gradient) and one lesser known reason: the fact that low P availability will probably concern a larger extent of the global ocean in the near future. Climate change may modify the relative importance of external to the total input but obviously will not be able to change the P depleted status of the MS.

MK : Page 4 line 23 Do the authors really mean to suggest that the surface waters entering the Mediterranean are 'nutrient rich'? All papers I have ever read suggest there is a net export of nutrients from the Mediterranean. They refer to a paper by Antoine and Morel (1995) which is about coastal zone scanners in the Eastern Mediterranean. Please clarify?

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. The following sentence is confusing:"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." will be modified as follows: ":"This is mainly related to hydrological conditions and particularly to higher winter convection in the western basin." We refer to Antoine et al. (1996) and not Antoine and Morel (1995). This concerns the world ocean.

MK : Page 4 Line 28 The text discusses figure 2. It says that upwelling areas with high production and 'high nitrate concentration' would be found on the y-axis. But this figure is a plot of depth at the top of the nitricline vs primary production. It does not mention or show nitrate concentration at all.

The authors : It was ambiguous and not necessary for the objectives of the BOUM experiment. The text will be modified as follows: Figure 2 shows integrated primary production vs depths at the top of the nitracline for areas with nutrient depleted upper waters. The following sentence : "In this figure, upwelling areas with high primary production and high nitrate concentration would be found on the y-axis, as would HNLC areas with low primary production and high surface nitrate concentration" will be deleted, and Fig. 2 modified to indicate only Oligotrophic to Ultra-oligotrophic areas.

MK : Page 5 line 3 The MS presents the main oceanographic features of contrasting environments characteristic of the oligotrophic ocean.' What features of contrasting environments?

The authors : This sentence was the conclusion of the previous paragraph which seemed to lack detail. It will be modified as follows: Figure 2 shows integrated primary production vs depths at the top of the nitracline. Oligotrophic to ultraoligotrophic areas are presented from the left to the right as scaled by the deepening of the top of the nitracline. This was associated with a decreasing trend in primary production during a previous cruise transect in 1996 (MINOS cruise), with a western basin oligotrophy (Mean depth of the top of the nitracline around 50-100 m and primary production rates around 200-400 mgC m-2 d-1) as generally observed in the oligotrophic open ocean (Moutin et al., 2002), and an eastern basin oligotrophy (depth of the top of the nitracline below 150 m and primary production rates below 200 mgC m-2 d-1) found to be as extreme as that observed in the South Pacific gyre (Moutin, unpublished result), which is considered as the most oligotrophic area of the ocean (Claustre and Maritorena, 2003). On a regional scale, the MS presents the main oceanographic features of contrasting environments characteristic of the oligotrophic ocean.

MK : Page 5 line 10 When is 'late winter and spring'?

The authors : February, March and April, May, June. It is a citation.

MK : Page 5 line 11 The term 'spring' bloom is misleading here (and elsewhere in the text). The bloom in the EMS starts in late autumn and finishes in March-April. What they mean (throughout) is annual phytoplankton bloom.

The authors : we agree to change spring bloom to annual phytoplankton bloom, which seems here an appropriate term.

MK : Page 5 paragraph starting on line 14 Does this discussion on the use of phosphate turnover time refer to data from the BOUM cruise or pre-existing studies?

The authors : it refers to pre-existing studies in the oligotrophic ocean as synthesized in Moutin et al. (2008). The reference appears at the end of the paragraph but will be added also to line 19 to clarify this point.

MK : Page 6 line 18 Abundant evidence exists for the uncoupling between nitrogen and phosphate cycles? I don't understand this comment. In all marine systems N and P are coupled. If this refers to nitrogen fixation, then as you will see below I do not find their argument convincing.

The authors : N and P cycles may be coupled by several processes like production and mineralization or uncoupled by other processes like nitrogen fixation which consists of an uncoupled input of N versus P into the pelagic ecosystem (Karl 1997). In any case, all this introductory part will be deleted.

MK : Page 7 line 2 Krom et al (2005b) is about nutrients measured in the SE Levantine basin and has nothing to say about nutrients exported at the Straits of Sicily. Maybe they mean Krom et al., 2004 which does mention such export.

The authors : yes, it will be modified as proposed.

MK : Page 7 line 5 "The large P deficiency in the external input, both riverine (Ludwig et al., 2009) and atmospheric (Guerzoni et al., 1999) combined with exchanges at the straits of Sicily has been hypothesized to explain P deficiency in the eastern basin (Krom et al., 2010)." That is not what Krom et al (2010) wrote. In the abstract to that paper, Krom et al., write 'the unusual nutrient ratio is due to high N: P values in all the external nutrient inputs to the EMS, coupled to low denitrification rates within the ultra-oligotrophic basin.'

The authors : this part will be deleted.

MK : The sentence which follows on page 7 line 8 that there are "two fundamentally different internal processes to explain the typical NO3:PO4 ratios observed in deep waters" is thus wrong and the logical conclusions which follow are also wrong. In the present text they note correctly that two of these possible processes are a) nitrogen fixation and b) adsorption of phosphate on dust particles but do not mention the third (and correct) explanation of skewed external supply coupled with very low denitrification rates due to the ultraoligotrophic nature of the basin. They correctly say that adsorption of phosphate on dust cannot explain the high N: P ratio but then incorrectly conclude 'thus Nitrogen fixation appears to be the key factor in explaining the high NO3:PO4 ratios'. What makes this statement especially odd is that Moutin is an author on the paper by Bonnet et al., (2011) which shows there are very low nitrogen fixation rates in the EMS. They even state on page 17 line 9 that there are low nitrogen fixation rates in the EMS (Bonnet et al., 2011)! The text continues by stating that 'very few measurements are available for nitrogen fixation rates.' This is no longer true. There are now extensive (very low) nitrogen fixation numbers in the EMS from Ibello et al., 2009, Yogev et al., 2011 ($\sim$ 100 measurements) and their own BOUM data in Bonnet et al., 2011. The text goes on to state the δ15N from fossilized chlorophyll from the MINOS cruise 'provides geochemical evidence for extensive N2 fixation in the EMS (Sachs and Repeta, 1999)'.(Pantoja et al., (2002) is in the reference list but not in the manuscript). It has been shown by Mara et al., (2009) reviewed in Krom et al., (2010) that this δ15N signal was most likely due to atmospheric input of fixed N2 and not to N2 fixation at all.

The authors : As explained above, we recognize our mistake and will delete this part in the introductory manuscript.

MK : Page 8 paragraph 1 Since the previous paragraph is wrong, this next paragraph starts with a fallacy. Yogev et al., 2011 conclude that N2 fixation in the EMS is P limited. However the authors are correct that understanding the new phosphate is critical to understanding new production in the MS. However the manuscript calculates new production based on a N cycle (section 6). The paragraph finishes by pointing out that P has no redox chemistry while N does (which of course is well known). The paragraph concludes by writing that 'it is possible to envisage coupling with production and the establishment of a budget from a different angle.' This sentence is very unclear. The introduction concludes by saying that the MS has a wide range of oligotrophic conditions and provides a case study for observing the links between C, N, P, Si and Fe cycles. That is indeed true but that is not what this manuscript as written is about.

The authors : this paragraph will be deleted

MK : Page 8 line 23 3 Objectives of the BOUM experiment This is a rather odd title and section. If this is relevant to this manuscript, then you would expect to be able to see that these objectives have been addressed and at least in part answered.

The authors : the objectives have been addressed in the introductory manuscript and are globally answered in the 24 other papers of the special issue described in section 7. Some answers were also included in other parts of the introductory manuscript, which

was not appropriate. The confusion will hopefully disappear with the 2 manuscripts proposed.

MK : Page 9 line 11 I assume the text was supposed to read (LD) stations at the centres of anticyclonic.

The authors : It will be corrected as suggested

MK : Page 10 line 7 The main Mediterranean water masses are not shown in Figure 4.

The authors : The TS diagram will be modified to include the Mediterranean water masses encountered along the BOUM cruise-transect.

MK : Page 10 line 9 If we need to see EMT then this data should be shown.

The authors : yes, this change will be made. MK : Page 10 line 13 All non-surface waters in the world's oceans are formed in winter. What is almost unique about the Mediterranean is that this is simultaneous with the annual phytoplankton bloom whereas normally the two are offset in time.

The authors : MK's remark is interesting but it seems that the annual maximum phytoplankton production is usually observed during the winter period in subtropical gyres as well. There is no error in our sentence and we see no point in adding such a remark here, in a description of a TS diagram.

MK : Page 10 line 14 The 'straight lines.' What straight lines?

The authors : we will add a dotted line on the TS diagram and label it accurately.

MK : Page 10 line 20 As a consequence, it can be assumed that the anticyclonic eddy C was an area of Levantine water formation during winter 2008. This is the first time I have ever heard any suggestion that LIW forms in the Cyprus eddy. Furthermore the authors later assume that the eddy is a closed system which means that it cannot at the same time be a source of any extensive water mass.

The authors : It is true that it is the first time we evidenced an anticyclonic eddy being an area of Levantine water formation, but such a possibility has already been suggested in Paola Malanotte–Rizzoli and Hecht (1988, Oceanologica Acta) and in Malanotte – Rizzoli et al 1999 DAO. Later in the text, we consider the eddy as a closed system to give a first order N-budget. It is probably not far from reality at the scale of the life time of this type of eddy which varies from several months to several years. If an eddy with a diameter of around 100 km lasts for around one year in a given area, i.e. that an eddy is formed and destroyed every year, it could be a contribution of LIW. In this manuscript, we merely show that eddy C is a source of LIW and do not discuss the volume of water formed, which is outside the scope of this manuscript.

MK : Page 11 line 3 What does 'de-salinated' mean?

The authors : the term 'de-salinated' will be deleted in the sentence. We will rewrite this part and as requested above, add more details on the TS diagram and explanation.

MK : Section 5.2 Main characteristics of the stations This section introduces a series of measurements and calculations from the BOUM cruise. In almost none of them is sufficient detail given for the reader to know the basis for the original data presented. Either this manuscript should have a sampling and methods section or possibly if these are all derived from specific other BOUM cruise papers, then a table showing where that information can be found.

The authors : we will add a "Material and methods" section in the proposed 2nd manuscript. Most of the methods detailed in other papers of the special issue will be only referenced.

MK : Page 13 line 5 The sentence starting 'After omission. . .' it is not clear what is the basis for the averages given. This should be more explicit.

The authors : this is indicated p12 line 22 : (4) Ichlo a, Integrated total chlo a concentration (mg m-2) between 0-150 dbar. All details will now be available in the Material

and methods section and so will be easier to find.

MK : Page 13 line 9 'This value remains somewhat constant.' What 'value' remains constant?

The authors : This will be rectified to "the integrated chlorophyll a concentration remains somewhat constant".

MK : Page 13 line 10 What observation?

The authors :This should be clear now in the light of the previous response.

MK : Page 13 line 23 The depth DNO3 is not shown in figure 1

The authors : it is figure 2, sorry.

MK : Page 14 line 23 You should show the algorithm used. This is because the elemental ratios in the MS are often different from the usual values

The authors : the reference added is not for an elemental ratio, it is for the oxygen solubility algorithm used, and also, we don't think it is necessary to add more than a reference here.

MK : Page 15 line 16 This seems to me to be incorrect. Oxygen from the atmosphere can only influence waters at the surface and in the immediate mixed layer. LIW, which is usually 200m or more deep in the EMS, and thus cannot be influenced by atmospheric oxygen.

The authors : MK is right. The end of the paragraph will be rewritten to give more details as, for example, the link between LIW and the atmosphere during the maximum winter mixing.

MK : Page 16 1st paragraph Much of the sections which follow discuss in detail station C, which is known in the literature as the Cyprus eddy or the Shikmona gyre. The text does not build on the existing extensive knowledge about this system and its biogeochemical processes. I have noted specific ways in where information is available but exactly how the text should be modified would depend on reading these articles and then rewriting the text appropriately.

The authors : In this paragraph we presented the main characteristics of the eddies sampled during the BOUM cruise. It was only a description. We propose to add more references to previous work in the discussion part of the 2nd manuscript.

MK : Page 16 2nd paragraph and following. It is a real problem using the 'standard' calculations based on the Redfield ratio (Rr?) in the MS because the ratios found are so different.

The authors : in the present case, no calculations are made. It was only a scale chosen in order to see the specific nutrient ratio of the MS. This choice will be discussed in the discussion of the 2nd manuscript.

MK : Page 16 line 16 The maxima of nutrient concentration do not correspond to the maxima of organic matter remineralisation; they correspond to the maximum of nutrient accumulation. The fastest rate of mineralisation is always higher in the water column.

The authors : This will be modified as proposed. It is clear that the oldest waters reveal the maximum mineralization even if the rate of mineralization is lower.

MK : Page 16 line 19 The variable P* has very little meaning in this system where the N: P ratio changes from 23:1 (WMS) to 28:1 (EMS). This results in the odd negative P*'s which are presented. They are negative because the system is not Redfieldian.

The authors : We disagree. We think that this representation allows to clearly show that the system is Redfieldian as soon as ïĄĎN:ïĄĎP changes in deep water are considered. It was noted as an interesting hypothesis by the other reviewer. This part will be treated in more detail and better explained in the 2nd manuscript.

MK : Page 31 last sentence. "The most probable explanation for this result is that mineralisation of organic matter in deep water follows the Rr and that the deep exported

material has probably relative N and P concentrations close to the Rr, as previously hypothesized by Redfield et al., (1963)". This is incorrect. Firstly Redfield never worked in the MS so they had nothing to say about this system. Furthermore the mineralisation of organic matter is rarely Redfieldian at all depths in the water column. A coherent explanation for the odd N: P ratios at depth is given in Krom et al., (2010) which is consistent with previous results for the EMS. They state that the high ratio of N:P at depth is due to a combination of preformed nitrate but not phosphate in deep water (deep water formation occurs simultaneously with the annual phytoplankton bloom which consumes all the phosphate but has residual nitrate) and preferential recycling of P relative to N in the POM and DOM in the EMS. This is confirmed by measurements of POM and DOM made both by Krom et al., 2005b and subsequently by Pujo-Pay et al., (2011) in the BOUM cruise data.

The authors : MK first writes that "Redfield never worked in the MS so they had nothing to say about this system". We, on the contrary, found that the general trends first observed elsewhere in the ocean by Redfield are also true for the deep MS, and are therefore, interesting to underline: spatial change in deep MS waters follow the Rr ratio. Secondly, MK writes that "furthermore mineralisation of organic matter is rarely Redfieldian at all depths in the water column". This is true, and not in opposition with our observation that deep mineralization (below 250 m) seems to follow the Rr. The changes in deep water follow the Rr ratio, where Rr stands for ïĄĎN:ïĄĎP along some isopycnal lines to prevent the inclusion of preformed nutrients in the ratio. It is also true that deep N:P ratios are high compared to similar ratios in other oceans: this may be explained by preformed nitrate (but not phosphate) and preferential recycling of P (relative to N). This part will be treated in more detail and better-explained in the 2nd manuscript. To avoid the difficulty of talking about negative values, we propose to use and present N* instead of P*.

MK : Page 17 lines 8-10. This sentence about low N2 fixation rates (actually Yogev et al., 2010 found some of the lowest rates ever measured) is incompatible with their text about the importance of N2 fixation in the introduction.

The authors : we have already explained the reason for this inconsistency and will correct the ms as proposed above.

MK : Page 17 line 15-16 and then line 24-25. These two sentences are incompatible. Either the DCM is directly related to the winter mixing depth (wrong) or it is directly related to the euphotic layer depth (correct).

The authors : We wrote: line 15-16 "The strong relationship between the top of the nitracline and the DCM has already been described (Herbland and Voituriez, 1979), and is directly related to the winter mixed layer depths (Table 1a) with the exception of inside the eddies and in the NW MS." line 24-25 "Thus, the DCM depth, the euphotic layer depth and the top of the nitracline depth are clearly correlated, and deepen from the West to the East of the MS." We do not understand how observations can be wrong!

MK : Page 17 line 30-32 The net input of nitrate to the photic zone is not the same in the core of the Cyprus warm-core eddy as outside the eddy (see Krom et al., 1992 and 1993).

The authors : this working hypothesis will be discussed in the light of MK's papers in the 2nd manuscript.

MK : Page 18 lines 3-5 Here the eddies are considered as closed systems while elsewhere they are the source of LIW

The authors : yes and this is not incompatible. The eddies are considered as closed systems during their life time, and open systems at the end of their life time. The outward spread of LIW water may occur when they are destroyed, i.e. at the end of their life time. It might occur each year for eddy C.

MK : Page 18 line 7 During winter, LIW was located at the surface in the Levantine basin' How do you know? You were there in July?

The authors : We were there in July and left Argo floats which remained in the area until the following winter. For the previous winter, we proposed in this manuscript a new method for calculating the previous Winter MLD. A Winter MLD of 340 m was observed by an Argo float inside eddy C during the winter 2009. This is not far from the 400 m depth calculated with our method for the winter 2008. This remark is to explain why we consider that it is possible to give a first order annual budget with our single cruise data.

MK : Page 18 line 15 When is spring? Again you have no data from spring?

The authors : as proposed before, spring will be replaced by annual.

MK : Line 20 W-MLD is not shown on Figure 7.

The authors : The white line shows the W-MLD. It will now be indicated in the legend, sorry.

MK : Page 20 line 17 to Page 21 line 20. This is a manuscript aimed at biogeochemists. This section is written for a Physical oceanography audience. A much shorter summary such as that given in Moutin and Raimbault (2002) would be adequate.

The authors : this manuscript is aimed at biogeochemists and also at physicians interested by Biogeosciences. Other papers in the special issue, like the one submitted by Cuypers et al. is aimed at physicists and also at biogeochemists interested by Biogeosciences. We are at the interface between fields and understand that the manuscript may not have been easy to read. Hopefully, the separation into 2 manuscripts and the addition of a material and method section in the 2nd manuscript will be helpful.

MK : Page 21 line 9-11 Here is a sentence which quotes Krom et al. (1992) as justification for an aspect of physical circulation when that paper is not about that. The relevant paper(s) about physical circulation in Cyprus eddy (which is not quoted at all here is Brenner et al., (1990) or Zodiatis et al., (2005) in the CYCLOPS volume.

The authors : Krom et al (1992) indicates "Beneath this surface layer a similar T/S structure existed to that found in February, with a themostad (isothermal, isohaline layer) from 100 to 450 m, a deep pycnocline (450-650 m) and LDW at depth" p. 471 line 13. We use this reference to indicate that a pycnostad in eddy C had already been observed. It was also because nitrate and phosphate properties were close to our observations. We propose to add Brenner et al. 1991 (DAO).

MK : Page 22 line 15 Here the authors seem to be speculating about the timing of processes which they have no direct evidence and actually are not crucial to their argument anyway. Thus why do the authors say the eddy was formed at this location during the summer of 2008 and not say 2007 or 2006?

The authors : We do have arguments to say that eddy C was formed during a previous winter. We cannot affirm that the eddy was in exactly the same location and that it was formed during winter 2008, but the eddy was in the area in late winter 2008 when the Winter MLD was maximum (February, 29 2008). This part will be discussed in more detailin the 2nd manuscript. The same method was used for the 3 eddies.

MK : Page 23 line 23 to page 24 line 14 Here is a section which should build on the detailed previous knowledge of the Cyprus Eddy (Station C). The principle relevant previous work which carried out a rather similar treatment of biogeochemical cycling and New Production based on an entire seasonal data set rather than a single sampling are Krom et al., (1992 & 1993). This was used as the background for the CYCLOPS P addition experiment which was carried out in the Cyprus eddy. This work summarised in Krom et al., (2005).

The authors : yes, it will be used in the 2nd manuscript.

MK : Page 26 line 19 "New N-input corresponds to the sum of these two fluxes, first because other possible inputs are neglected." Well that is obviously true but unfortunately that creates a real problem in this calculation because the authors are ignoring atmospheric input which is by far the largest external input of N to the MS.

The authors : atmospheric input will not be ignored in the proposed revision of the ms.

MK : Page 27 Line 1 The statement that N-input by N2 fixation during the strongly stratified period may represent a significant part of new production is only true if you ignore other external inputs like atmospheric input. It is also odd though possibly correct that it is said on line 20-21 that N2 fixation is extremely low inside eddy C in the eastern MS. Krom et al., (1992) carried out a full estimate of the annual new production in this eddy which includes all possible sources. It would be good to compare these calculations.

The authors : yes, we will do so.

MK : Page 27 line 4 The authors quote Krom et al., (1991) as discussing eddy diffusion which it did not at any time. Krom et al., 1992 included eddy diffusion but never suggested the diffusion coefficient was a major error in the budget.

The authors : We said: "The use of estimated eddy diffusion coefficients for the calculation of diffusive fluxes is considered as a prime source of inaccuracy (Krom et al., 1991; Moutin and Raimbault, 2002). This is a mistake and indeed not in Krom et al., (1991), but in Krom et al., (1992). The latter indicate in the introduction p. 468: "In this approach, the principal errors arise from uncertainties in estimates of the nutrient transfer by lateral advection and in the value of the eddy diffusion coefficient used to estimate the nutrient transfer rates through the nutricline to the euphotic zone." And in the discussion p. 476: "The value for the eddy diffusion coefficient is a modelled value and taken together with uncertainties in the nutrient gradient, leading to some uncertainty in this calculated flux." We have decided to cite the original reference given by Krom et al., (1992): Codispoti et al. 1986.

MK : Page 27 line 25 Annual N-budget at the LD stations This is really problematic and I suspect should be omitted entirely from the manuscript. I do not see how you can calculate a meaningful annual budget with no seasonal data? This calculation has had to make a series of assumptions which may or may not be valid such as 'how long deep convection occurred' or the seasonal changes in N2 fixation although it is said on line 20-21 that N2 fixation is extremely low. Furthermore atmospheric input which is a major source of fixed N to the system (60% of the entire external N to the basin) has been omitted.

The authors : A significant part of the ms was dedicated to the validation of a method allowing to determine the W-MLD from one CTD profile and calculations. This will be reinforced in the 2nd proposed manuscript. As was explained in the introduction and confirmed by our results, the previous W-MLD is a fundamental criterion for biogeochemical and particularly biological pump budget. We recognized that proposing an annual budget from one temporal cruise may be problematic but firstly, we clearly underlined the assumptions, and secondly we clearly indicated that it is a first order budget. In the new version of the ms, we propose to take into account the atmospheric input and to compare with the budget reached by Krom et al. (1992) from several cruises.

MK : Page 28 line 27-28 Annual (I don't like the term spring when they occur in winter) phytoplankton blooms are visible from space. There are numerous papers which show this (e.g. D'Ortenzio and d'Alcala, 2009). They occur when the nutrients from depth are mixed into the photic zone and in the MS there is a simultaneous plankton bloom. Crombet et al., (2011) speculate that when there is a DCM there might be a deep glass forest which is a very interesting idea. However this is in summer. They have no information on the diatom distribution in winter during the bloom.

The authors : we only said that it is probable that all blooms may not be visible from space. Crombet et al., (2011) dealing about the study by D'Ortenzio and d'Alcala wrote: (1)-However, this study concerns surface Chl-a distribution as observed by satellites and ignores the potential deep phytoplankton accumulation, which we will show in this study, could modulate this clear cut pattern." (2)-Considering the definition of phytoplankton bloom by Wilson (2003) as sustained Chl-a values>0.15 μg L?1, it would be adequate to characterise the MS as hosting a deep summer bloom that is not visible by satellite imagery and that would lead to modulate the characterization of the Eastern

basin as a "non bloom" region as suggested by D'Ortenzio and Ribera d'Alcala (2009). Please note that the other reviewer of this manuscript, M. Ribera d'Alcala commented: " 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."

MK : Page 29 line 9 'Atmospheric deposition of dust was omitted because of its high spatial and temporal variability.' This is a problematic assumption when carrying out an annual budget since atmospheric deposition is the largest single external source of N to the MS (Guerzoni et al., 1999; Krom et al., 2004, 2010). The authors could have used the measured fixed N inputs from Crete (Mihalopoulos and co-workers) or Israel (Herut and co-workers) to estimate its importance.

The authors : we propose taking that into account in the 2nd ms

MK : Page 29 line 21 to page 30 line 17 According to the introduction as written this is the purpose of this manuscript. If it is then this is very weak.

The authors : Once again, the general introduction was for all the papers of this special issue and of course this part called "towards a carbon budget" merely pointed out the interesting findings from a first order budget. We are not yet at the stage of of a general synthesis of all the data gathered during the BOUM cruise. We are now beginning to work on a 3D biogeochemical and physical model.

MK : Page 29 Line 27 "assuming a similar primary production inside and outside, the eddies will be more efficient..." That is a very large assumption and since the major PP is not in July and therefore outwith any data collected in BOUM.

The authors : yes it is a very large assumption, but it was made only to reflect on the fate of this production in and outside of an eddy.

MK : Page 30 line 20 to page 33 line 16 As stated in my general comments, I would have liked to read a more comprehensive summary of the really interesting and important results from the really important BOUM cruise papers.

The authors : We will add more details in the 1st proposed ms, as suggested.

MK : Figures: Many of the figures are difficult to follow either because of the way they are plotted or the details given or not given.

The authors : the figures will be modified as suggested

MK : Figure 1 It is unclear what is being shown here?

The authors : Fig. 1 was described p. 2 from line 15 to line 27. We will add (Fig. 1) after carbonate ions line 15. We will also modify the legend as follows : "Fig. 1. Major carbon fluxes for a biological pump budget. Biological pump : carbon transfer by biological processes into the ocean interior. DIC : Dissolved Inorganic Carbon, POC : Particulate Organic Carbon, DOC : Dissolved Inorganic Carbon. Modified from Moutin et al., (2000).

MK : Figure 2 What is AG? Why is HNLC on this figure – it is never discussed? Why is oligotrophic presented at right angles to Eutrophic? What is SP?? Ramdom?

The authors : AG will be replaced by anticyclonic eddies, HNLC will be deleted as explained above, Eutrophic will also be deleted, SP Gyre will be replaced by South Pacific gyre. Ramdom means that mesoscale activity was not considered before sampling during the MINOS cruise in 1996. This picture will be presented in the previous section 5.4 (i.e, in the 2nd manuscript).

MK : Figure 3 Almost impossible to read the details of the three small figure below.

The authors : as this will now be explained in the legend: "All figures could be increased in size on the screen to get all the details". This has been verified.

MK : Figure 4 No water masses are identified on the figure nor are stations identified properly.

The authors : The figure will be modified by adding the main Mediterranean water masses.

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