The biogeochemistry from the Oligotrophic to the Ultraoligotrophic Mediterranean (BOUM) experiment. By T.Moutin, F. Van Wambeke and L. Prieur

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.

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.

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). This has both produced some very odd inconsistencies in the manuscript concerning 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.

Conversely the very important results of the BOUM cruise (24 papers in all) are summarised 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.

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

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?

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.

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.

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.

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.

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?

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.

Page 5 line 3

The MS presents the main oceanographic features of contrasting environments characteristic of the oligotrophic ocean.' What features of contrasting environments?

Page 5 line 10

When is 'late winter and spring'?

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.

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?

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.

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.

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 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 (~ 100 measurements) and their own BOUM data in Bonnet et al., 2011.

The text goes on to state the $\delta^{15}N$ 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 $\delta^{15}N$ signal was most likely due to atmospheric input of fixed N2 and not to N2 fixation at all.

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.

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.

Page 9 line 11

I assume the text was supposed to read (LD) stations at the centres of anticyclonic.

Page 10 line 7

The main Mediterranean water masses are not shown in Figure 4.

Page 10 line 9

If we need to see EMT then this data should be shown.

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.

Page 10 line 14

The 'straight lines.' What straight lines?

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.

Page 11 line 3

What does ‘de-salinated’ mean?

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.

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.

Page 13 line 9

‘This value remains somewhat constant.’ What ‘value’ remains constant?

Page 13 line 10

What observation?

Page 13 line 23

The depth D_{NO3} is not shown in figure 1

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

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.

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.

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.

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.

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.

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.

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.

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).

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).

Page 18 lines 3-5

Here the eddies are considered as closed systems while elsewhere they are the source of LIW

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?

Page 18 line 15

When is spring? Again you have no data from spring?

Line 20

W-MLD is not shown on Figure 7.

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.

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.

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?

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).

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.

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.

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.

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.

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.

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.

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.

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.

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.

Figures:

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

Figure 1

It is unclear what is being shown here?

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?

Figure 3

Almost impossible to read the details of the three small figure below.

Figure 4

No water masses are identified on the figure nor are stations identified properly.