Referee report on “The Biogeochemistry from the Oligotrophic Ultraoligotrophic Mediterranean (BOUM) experiment” by T. Moutin, F. Van Wambeke and L. Prieur

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

More specific issues

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?

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

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 a major source of nuts. 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.

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

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

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

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.

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

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.

On p.9 l.7-10 the cruise plan is presented with the opposite direction than at p.1 l.25. It would be better to use the same criterion

On p.10 l.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.

On p.10 l.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.

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

On p.13 l.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

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

On p.15 l.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.

On p.15 l.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 text at p.16 l.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

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 the test the hypothesis more in depth.

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)?

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

On p.24 l.22-25 vertical stability is take 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).

I did not find any definition of Cp which is analyzed on p.24

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

Typos and biblio

p.23 which leads leading
p.24 l.25 schown
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 Cuypers 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
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.