REVIEWER 1 (Enma Garcia Martin)

EE Garcia-Martin (Referee)

enma.garcia-martin@uea.ac.uk

"Dust deposition in an oligotrophic marine environment: impact on the carbón Budget" tries to link together the bacterial respiration data presented in Pullido-Villena et al. 2014 and the primary production reported in Ridame et al. (2014) plus an extra attempt to calculate the carbon budget for an oligotrophic region in the Mediterranean Sea as a response to dust inputs. Without a doubt we are dealing with an interesting manuscript of high interest which presents very useful data from bacterial respiration. However, this data could not be considered novel as much of what is said in the present article is presented in companion ones in the same special issue (Pulido-Villena et al. and Ridame et al. the same issue).

Response. We first would like to thank you for your review that was really an excellent work. Indeed all the suggestions and comments that you made, along with the mention of several issues that you pointed out helped us a lot to reconstruct the manuscript. We really appreciate the time you have spent on this review to help us reconsidering many missed aspects.

To answer to this 1st comment, not only BR and Production but POC Export; some of the data are indeed in companion paper give the detail here] but not all of them [give the detail here]; the attempt to use those data to understand in term of carbon what is the effect of dust input to oligotrophic environment was indeed the novelty of the approach (vertical dimension) that we wanted to highlight in this paper.

In consequence, the introduction section was modified and one final section was added that clearly states what data (unpublished and already published) are presented here; we also added a section to explicitly show that the large in situ mesocosm approach are improving a lot the classical "homogeneous microcosm" approach

There are several problems related with the MS, and it should be subjected to a thorough revision before accepting it. I will detail some concerns. The title emphasizes the impact on the carbon budget, but only net primary production and bacterial respiration is measured forcing the authors to make a lot of assumptions and estimations on other variables that intervene in the C budget (Community respiration, DOC production, etc).

Response. We also have the POC export measurements that are really new in this type of study, as we say in our previous response. But we agree that the title was not appropriate. We propose to change it for: Impact of dust deposition on carbon budget : a tentative assessment from a mesocosm approach.

The MS is not well structured making difficult a fluent reading. I will recommend rewriting the whole article and considering renaming the subheadings of the different section. There are paragraphs inside the result section that belong to the discussion part (i.e Page 1716 lines 20-25; Page 1717 lines 1-10, line 11-22; Page 1718 lines 6-10, 13-23) while others could be part of the material and method (Page 1717 lines 25-27). Calculus of the carbon balance is presented and discussed in the discussion section, but I would recommend, for a better understanding, moving the equation and the different parameters implied to the M&M section, and all the different terms involved should be explained. Results from this carbon mass balance are absent in the result part, and this is the heart of the article.

Response. We totally agree on that important comment and have changed the structure of the paper following these recommendations (that were also very similar from the 2 other reviewers). We agree that the paper was not properly written with a lot of mixing between methods and results that were very confusing. We hope the new structure is acceptable.

The different terms involved in the equation of the carbon mass balance should be revised and defined (i.e. Net community production and gross community production). Do the authors consider that gross primary production is the same as gross community production? Clarify it by defining the terms. Depending on how the authors define GCP, it might be possible that the term GCP= NPP +DPP is wrong (Page 120, line 25). NPP is, by definition, the fixed carbon available for other processes, so then, it is the difference of the organic carbon fixed by autotrophs and the respiration associated to these organisms. Therefore, autotrophic respiration in Equation2 is considered twice: in the NPP term and again in the 2BR (that represents the community respiration, considered as the respiration of the autotrophs + bacteria + heterotrophs>0.8 μm).

Response. We agree that this section was particularly confusing and that some terms were not properly used. The main problem was that we used GCP instead of GPP leading to the use of x2BR instead of x1BR in the equation. This was entirely corrected but consequently we had to introduce a new term that is zooplankton respiration. The new section describing the estimates of the different carbon pool is now in the Methodology section.

Authors perfectly remark in M&M that BR could be overestimated as previously reported (Aranguren-Gassis et al. 2012) and this overestimation could be enhanced in the equation as the term is being multiplied by two. Furthermore, at the end of the discussion authors are aware of their results advising that BR could have not been homogenous throughout the water column and that the integrated data should be taken with caution.

Response. Those 2 important points (1) BR data and (2) integration of the BR data over the mesocosm are detailed below. We hope that our response will meet your requirements.

In summary, the great number of assumptions involved in the equation presented, the number of concerns and the limited numbers of variables measured (and few samples) make it difficult to accept the MS and not to be aware of the results obtained for the organic carbon mass balance.

Response. We agree that the paper was not presenting correctly the data and we realize that some of the comments that were made in the text about the methodology used were interpreted as issues on the data presented in the paper and this is not the case. We hope that the way the paper is presented now and our responses to your comments will convince you that this data set is actually a good data set, (in part already published). The writing of the early version of the paper was really very awkward.

Specific comments

Introduction.

Consider to include Bonilla-Findji et al. 2010 in this section as it reports metabolic balance (GPP and CR) after some episodic events (Sahara dust deposition) in a similar area. Marañon et al. 2010 also explore the metabolic balance in Atlantic ocean after the addition of Sahara dust, so the sentence (Page 1711 Line 21) “the balance between the different main processes involved in the C cycle has never been explored” is not adequate. Please rephrase it. *Bonilla-Findji O., Gattuso J.- P., Pizay M.D. and Weinbauer M. G. 2010. Autotrophic and heterotrophic metabolism of microbial planktonic communities in an oligotrophic coastal marine ecosystem: seasonal dynamics and episodic events. Biogeosciences, 7, 3491–3503.*

Response. A number of references including Bonilla-Findji et al. 2010 have been added to the introduction that have been, like the rest of the manuscript profoundly reworked. Marañon et al. 2010 was already clearly mentioned as one of the most extensive studies where impact of dust deposition on trophic balance was studied in a number of contrasted

environments. What we meant by 'never been explored' was that never the POC export was considered in those experimental or in situ studies allowing to possibly 'close' the budget. We hope that the way this section was rephrased is acceptable now.

Material and Methods section.

In section 2.1, page 1713 line 11, authors mention a fourth mesocosm seeded with EC-dust that is not presented or discussed in the result part or discussion.

Response. We agree and this information was removed.

We have added some important information on the mesocosm methodology in the Method section. Although all these information can be found in the Introduction paper and the Methodological paper of the DUNE project, we agree that some important points deserve to be summarized here again. The new sections concern: (1) the study site; (2) the mesocosm setup; (3) the simulation of the mineral dust deposition; (4) how were done the different sampling. Also as a supplementary material, a short movie shows the different steps of the field work including the preparation of the dust before the seeding, the seeding itself and the sampling inside the mesocosms and the sampling of the sediment traps.

Moreover, authors comment about bad quality of DOC measurements in the different experiments and decided not to use them. If the data are not going to be used, it would be better not commenting it as it confuses the readers. Authors could state that DOC samples were collected in situ during DUNE P to have an idea of the DOC concentration in the studied region. However, this concentration should be only valid for the DUNE-P experiment and not for the whole set.

Response. For the DOC measurements, we have follow your advice and mention now only in the result section the DOC data acquired are used as initial DOC concentrations for DUNE-P experiment.

Page 1713, line 6. P, Fe, N and HNO3. It is the first time that the inorganic compounds are cited in the text, so please change them for phosphorus, iron, nitrogen and nitrate. As they are not used in the rest of the text there is not needed for their abbreviations.

Response. This was changed accordingly.

Page 1714. Line 5, "We are aware, however, that absolutes values of BR or net CO2 fluxes must be taken with caution". The paper is only based in BR and NPP. If you are aware of BR results your calculus of the carbon budget should also be taken with caution, and this is the main aim of the present paper. Reconsider your title and the paper if you are aware of your data. The great BR variability reported in here (and in Pullido-Villena et al. 2014) might be due to the poor replication between samples (SD data is not presented in the present MS but a great variability between replicates can be seen in Fig.3 from Pulido-Villena et al. 2014).

Response. The paper is not only based on BR and PP as it is well detailed now in the text and in the tables. Concerning BR, we are all aware that measuring in vitro respiration rates is not trivial and that there are controversy about the methods used and how this could have important consequences on our view of ocean autotrophy vs heterotrophy functioning. Interesting recent papers in the Annual Rev, Marine Sci*, highlight this on-going controversy showing quite well that there is no consensus so far. So usually, in their papers, the authors stay cautious in presenting their data, keeping in mind that methodological bias are possible. We didn't mean that our data were not good, we only wanted to refer to the on-going debate. We have been too cautious and this caution finally served badly the paper, going in the opposite direction of what we wanted to say. Although we are ok to change the title of the paper (because several parameters have to be estimated based however on solid assumptions), we believe that our PP, BR and POC export data are solid.

****: Ducklow H W. and Doney S C., What Is the Metabolic State of the Oligotrophic Ocean? A Debate, Annu. Rev. Mar. Sci.. 5:525–33, 2013.***

Duarte C M., Regaudie-de-Gioux A, Arrieta J M., Delgado-Huertas A, and Agusti S: The Oligotrophic Ocean Is Heterotrophic, Annu. Rev. Mar. Sci.. 5:551–69, 2013.

Williams P J. le B., Quay P D., Westberry T K., and Behrenfeld M J.: The Oligotrophic Ocean Is Autotrophic, Annu. Rev. Mar. Sci.. 5:535–49, 2013.

The whole data set is reported in Table S1 (that could be if necessary put in the paper rather?). We do not really agree concerning the 'poor replication' of BR considering that they come from triplicate large mesocosms. Indeed, if we calculate a coefficient of variation (triplicate mesocosms) from BR data (from Table S1), we have the following (see table below). I would rather say that most of the variation coefficient obtained indicate a good reproducibility of the measurements considering that the data are obtained in 3 distinct large in situ mesocosms.

	CONTROL-meso	***DUST-meso***
P	***7-13%***	***7-11%***
Q	***20-21%***	***10-22%***

R	***17-70% (and this high % is only due to one higher BR in one of the CONTROL-MESO (C3) at the last sampling time)***	***22-46%***

Page 1714, line 1-6. Please cite which respiratory quotient factor the authors have used to convert oxygen to carbon. Ex.; Oxygen consumption rate was converted to carbon respiration assuming a respiratory quotient of xxx. It does appear neither in here nor in Pulido-Villena et al. 2014.

Response. The following sentence was added: To convert oxygen consumption to carbon respiration, a respiratory quotient of 1 was assumed (del Giorgio and Cole, 1998).

Page 1715 Line 16-21. This paragraph could be considered part of results and discussion.

Response. Agree: done!

Page 1715 2.3. Data integration. BR data from one depth should not be representative for the 12 m integration performed. This is further commented in the discussion part (page 1722, "…overestimation of the BR whose value for the whole mesocosm was extrapolated from the rate measured at the depth of 5 m, meaning that BR was not homogenous, contrary to what hypothesized in Sect. 2"). If the authors have noticed that integrating the BR led them to suspicious results, why are they using them? If the authors are aware of the integration validity in one experiment, could it be possible that the other experiments could have undergone the same problem? Please reconsider this point, as it is one of the main pillars of your article.

Response. Our mistake in the equation presented in the previous version of the manuscript (taking into account 2BR) led to a wrong discussion, trying to explain the quite high DOC consumption deduced from the numbers. Again, that was very awkward, questioning the quality of data. This discussion was removed from the present version. This being said, we added a justification of the extrapolation of BR measurement at 5 meters depth to the ~15m depth of the mesocosm. "Based on the homogeneity of bacteria abundance for DUNE-P, -Q and –R (Pulido-Villena, 2014 and pers. Com.), the fluxes were integrated over the mesocosm depth assuming that the measurement at 5 m is representative of the flux over the mesocosm. Heterotrophic bacteria have been shown to be uniformly distributed with depth within the euphotic zone, usually corresponding to the layer between the surface and

the deep chlorophyll maximum (Tanaka et Rassoulzadegan, 2002). Therefore, within the 15-m depth surface layer enclosed by the mesocosms, little variations in bacterial activity may be expected".

Page 1716. Line 1-2, the relative changes of a treatment in relation with a control use to be expressed as (Xtreatment – Xcontrol)*100/XControl. Your denominator factor is Xtreatment. Check whether it was a typing mistake and not a calculus problem as the numbers obtained will mean different things.

Response. This is of course a typo in the text. The ratio were indeed calculated with (Xtreatment – Xcontrol)*100/XControl.

This was corrected in the text.

Results I suggest reading and including López-Sandoval et al. 2011 article who presents data of dissolved and particulate organic carbon production in the Mediterranean Sea, and whose results could modify the calculus of the carbon budget. They reported an average contribution of DOC production to total production (POC and DOC production) of 37%, higher than your 10% assumed from Lagaria et al. (2011) paper. López-Sandoval D.C, Fernández A. and Marañón E. (2011). Dissolved and particulate primary production along a longitudinal gradient in the Mediterranean Sea. Biogeosciences, 8, 815–825.

Response. We indeed now calculate the DOC production using 37% as those recent measurement in the whole Mediterranean Sea are very consistent along the whole BOUM transect in the oligotrophic water during the summer (same year as DUNE2). The new paragraph to justify the estimation of DOC production has been totally rewritten.

(All this section has been moved to the Method section as recommended).

Page 1718 lines 6-9. Authors comment "In the literature, the NP/BR ratio is commonly used to quantify the metabolic status of aquatic systems (see for ex. Del Giorgio et al. 1997, Duarte and Agusti, 1998)". Duarte and Agusti (1998) paper presents GPP/CR ratios not NPP/BR. Consider to remove reference from here.

Response. Sorry for the mismatch. The correct references are indicated.

Tables and figures. Be consistent along the text and figures in relation with:

report the different variables in the same units (table 1. Integrated data in mg C m-2 d-1, and volumetric data in μg C l-1 d-1, that correspond to mgC m-3 d-1);

Response. Done.

The names given to the experiments (call them always DUNE-Q, DUNE-P, DUNE-R, and not Q, R and P)

Response. Done.

The significant numbers (use always one decimal or none, i.e. Table S1

DUNE-P, DUNE-Q do not have significant decimals, but DUNE-R experiment has them for PP and POC).

Response. Done.

In Table S1 name the variables as in the rest of the test. I suppose that P_PP is NPP and P_POC is POCexport, but it is not explained anywhere.

Response. Done. The labels were changed.

Table 1. Present the data for the initials conditions for each experiment (DUNE-P, DUNE –Q and DUNE-R). As reported in Ridame et al. this issue, the hydrographic conditions were different (thermal stratification, transition period. . .), so the great SD and coefficient variation could be due to putting all the data together. **Include the number of data as another variable** (mean, SD, CV, n).

Response. The table was modified accordingly.

Figure 2. This figure contains similar information than figure 3 which is more complete. In figure 3 readers could see the evolution of the NPP/BR at the different days. I recommend not including this figure.

Response. We agree and removed figure 2.

Figure 3. I suggest modifying the graphs and representing the data with their standard errors. It is more accurate than representing the three individual data for each mesocosms.

Response. This was done.

In Graph a, control series has three points at times =-17, 48, 168 h, while in Table S1 there is only concurrent data for two of them (NPP was measured at -17, 24, 48, 96 and 168 h, while BR at -17, 48 and 120) so the calculus of NPP/BR could only be done at -17 and 48 hours. Change Table S1 or Figure 3 as correspond.

Response. This is due to the extrapolation of one data for control (very stable): see

explanation in new legend of Table S1. This explanation was missing and is now in the Table S1. "The PP data in the 3 MESO-CONTROL were not measured at t120 (p6). Examining carefully the data, we see that PP is very stable in MESO-CONTROL between p5 and p8, with averages values at p5 = 75 ± 6 mg C m-2 d-1, equivalent to PP at p8 = 72 ± 6 mg C m-2 d-1. We can assume that the value at p6 is similar to the value at p5 and p8; using the average of all the MESO-CONTROL data at p5 and p8, we deduced a control value of 74 mg C m-2 d-1 for p6".

References. Check cross references. Guieu et al. 2013, Pulido-Villena et al. 2013 and Ridame et al. appears as 2013 throughout the text and then as 2014 in References.

Response. Done.

Correct López-Sandoval, D. C., Marañón, E., Fernández, A., González, J., Gasol, J. M., Lekunberri, I.,Varela, M., Calvo-Díaz, A., Morán, X. A. G., Álvarez-Salgado, X. A., and Figueiras, F. G.: Particulate and dissolved primary production by contrasting phytoplankton assemblages during mesocosm experiments in the Ría de Vigo (NW Spain), J. Plankton Res. 32, 1231–1240 doi:10.1093/plankt/fbq045, 2010

Response. Done.

REVIEWER 2

The manuscript by Guieu et al. titled “Dust deposition in an oligotrophic marine environment: impact on the carbon budget” aims at linking two datasets presented in companion papers by Ridame et al. (2014), for primary production, and by Pulido-Villena et al. (2014), for bacterial respiration, to determine changes in carbon budget following dust additions in samples from the Mediterranean sea.

Although carbon budgets are of great scientific interest, the manuscript in its present form contains many flaws:

(1) This manuscript does not bring new data to the ones described in the two main companion papers. Only POC export data may be original to this manuscript although Ridame et al. (2014, companion paper) discuss those results, and Bressac et al. (2014, part of the special issue) present POC export data and discuss the results in a paper dedicated to POC export. The authors end up presenting results for POC export, NPP and BR (pages1716 – 1717) that belong to the companion papers;

Response. We hope that the end of the new Introduction section make the point clear about the data. Yes indeed, most of them are published (or in the process to) (and this is the reason why the data base is in the supplementary information rather than in the main text) but the goal of the present work is to integrate all of them to examine how the dust deposition impact carbon stocks and fluxes and this specific objective was indeed one of the main goal of the DUNE project. “Here we report on primary production (PP), bacterial respiration (BR) and particulate organic carbon export (POC_{export}) data acquired during DUNE-P, -Q and R experiments. All the PP data are from Ridame et al., 2014; BR for DUNE-P and Q are original data whereas BR data from DUNE-R are from Pulido-Villena et al., 2014. POC export data from traps measurements are from companion papers (DUNE-P-Q-R in Desboeufs et al, 2014 and DUNE R in Bressac et al., 2014). We first explore how the balance between bacterial respiration and net primary production is altered following the dust deposition. We then attempt to use the numbers measured (stocks and fluxes), along with estimates, to examine how the carbon budget, likely modified by the introduction of dust, can be balanced”.

(2) The carbon balance is only described in the discussion. Because the main goal of this paper is to report a carbon balance, it is fundamental to detail the calculation, provide results and discuss findings in the appropriate sections of the manuscript. The carbon

balance calculation should be detailed in the methods section instead of the discussion and the terms involved should be fully explained. The results from the carbon mass balance should be reported in the result section, not in the discussion. There are also parts of the results section that belong to the discussion.

Response. All the reviewers agree that the structure of the manuscript had to be changed. This new version of the manuscript was profoundly rearranged following the advices we have from all the reviewers, in particular, moving the calculation of the different terms of the budget in the methods section and then discuss the results in the Results section make sense.

(3) The important DOC measurements cannot be used (Page 1715, lines 5 to 10: "Samples were taken for DOC but we decided to not use the results as unexpected high concentrations and/or variability (either among the 3 depths in a same mesocosm or at the same depth in the triplicate mesocosm were found for many samples, ran- domly. Unfortunately, the same was observed for filtered samples either transferred in combusted glass ampoules (P and Q experiments) or in acid-washed HDPE bottles (R experiments)");

Response. This remark is in agreement with suggestion from reviewer 1 and the DOC data are now presented as initial conditions for the DUNE-P experiment and used in the discussion to evaluate the DOC consumption along the course of the experiment.

(4) The carbon budget relies on too many assumptions, extrapolations and estima- tions instead of measurements (e.g. page 1720, line 11: "estimates of unmeasured parameters");

Response. We are quite happy to provide relevant numbers for 3 important carbon pools that are: BR, NPP and POC_{export}. This is true that – as in many other studies – not all the terms were measured; our estimates are based on relevant hypothesis that we believe are now even better justified. It has also to be noted that a lot of those parameters estimates do not represent important terms for the carbon balance. To emphasize this point, the following sentence has been added to the new section III.2. (results) Induced changes in the carbon pools. " It is important to note here that although some of the terms have been estimated in the absence of direct measurements, those terms represents only a small fraction of the dominant pool represented by BR. Consequently, the errors potentially induced by these estimations have a little impact on the final estimation of the changes induced in the organic carbon pool."

(5) The authors recognize that important data are not reliable (e.g. Page 1714, Line 5, "We

are aware, however, that absolutes values of BR or net CO2 fluxes must be taken with caution”).

Response. Please refer to our response to rev 1 to the exact same question.

The reader is thus left questioning the validity of the carbon budget and as a reviewer I wonder how useful will be this paper for potential readers: will it be cited? Because of the major flaws listed above, **I cannot recommend this manuscript for publication**. Nevertheless, I recognize that establishing a carbon budget is a difficult but much needed endeavor and appreciate the authors’ effort to overtake this challenge. My suggestion would be to include the carbon budget as part of the discussion in one of the compan- ion paper

Response. This paper was a main objective of the DUNE project: how the carbon budget is impacted by a dust event? We do not wish to include this tentative budget in one of the companion paper as we believe that it is really standing by itself. It was indeed awkward to present the data with so much caution whereas no reviewers even mention the possibility that the data could be questioned in the companion papers where they are first presented! So we changed the title (new: “Impact of dust deposition on carbon budget: a tentative assessment from a mesocosm approach”) of the paper because indeed some actual parameters are missing to be able to do the whole calculation but the data we are using are robust.

We hope that the new structuration and justification of the use of the data will be acceptable to you.

REVIEWER 3

This manuscript reports a valuable effort to integrate the results of a mesocosm dust deposition experiment. However, as it stands, the work presents several problems.

General comments

Much of the data on which the manuscript is based are already reported in other papers (Ridame et al., 2014, Pulido-Villena et al., 2014), while the main potential added value of this manuscrip, which is the attempt to derive a car- bon budget, is based on many assumptions, some of them shaky, and/or not very reliable data (e. g., BR).

Response. A justification of the meaning of this paper in addition to the companion papers that present 'individual' data has been done at the end of the introduction.

b) The results of microcosm and mesocosm experiments are influenced by the initial conditions of the enclosed community (e.g., composition and seasonal/successional stage, phytoplankton biomass in relationship with nutrient concentrations, etc.). All the DUNE experiments were carried out in June-July; this aspect limits the scope of the conclusions and should be adequately addressed.

Response. In the new Introduction section, a section explains the meaning of having several experiments conducted with same initial conditions. Indeed this was made on purpose. "Two campaigns to study the impact following different scenario of dust deposition were conducted in the frame of project DUNE: DUNE-1 campaign in June 2008 and DUNE-2 campaign in June-July 2010. DUNE-1 consisted in two distinct 8-day experiments: a first simulation of a Saharan wet deposition event (hereafter named "DUNE-P") and a second simulation of a Saharan dry deposition event (hereafter named "DUNE-Q"). DUNE-2 consisted of a single 16-day experiment (hereafter named "DUNE-R") with 2 successive dust wet deposition simulations with 7 days between each seeding (respectively named "DUNE-R1" and "DUNE-R2"). The purpose of having 2 campaigns (2008 and 2010) at the same period (beginning of summer) was to test different scenario of deposition with similar in situ conditions. For that purpose, in 2008, we indeed performed 2 distinct experiments to investigate whether dry and wet depositions were followed by the same impacts; in 2010, we tested if 2 successive deposition fluxes of similar magnitude and duration result in similar impacts, and if so, why? This strategy of two successive seedings was decided following DUNE-1 results. Etc."

c) The manuscript is difficult to follow, in part due to deficient organization (see other comments) and in part because much of the necessary information (characteristics of the study site, initial conditions, methodology, etc.) needs to be sought elsewhere.

Response. See also reply to reviewer 1. Basically, the whole paper has been rewritten. In particular, a summary of the basic information have been done now and a short video was added as supporting material.

A new section devoted to summarize the main characteristics of the site and the initial conditions at the time of the experiments have also been added.

Specific comments

What is the rationale for the expression GCP=NPP+DPP (page 1720, line 25)? Please, explain. As mentioned by another referee, depending on the definition of GCP (and NPP), this expression may be wrong. Apart of the problem with the double consideration of autotrophic respiration (also mentioned by the referee), the carbon calculations of Table 2 include a large number of assumptions and extrapolations. This could be acceptable as a complement to other basic information, but not as the main message of the manuscript.

Response. As we said to Rev 1, this section detailing the different carbon pool was confusing and some terms were not properly used. The main problem was that we used GCP instead of GPP leading to the use of x2BR instead of x1BR in the equation. This was entirely corrected but consequently we had to introduce a new term that is zooplankton respiration. This detailed section in now in the Methodology. The numbers found for the different pool are presented in the result section. As said above to a similar comment concerning the terms that have been estimated, they "represents only a small fraction of the dominant pool represented by BR. Consequently, the errors potentially induced by these estimations have a little impact on the final estimation of the changes induced in the organic carbon pool."

A large part of the Results text in pages 1716 and 1717 should be placed in the Discussion section (e. g. comparisons with data from other authors, etc.). On the other hand, some information given in the Discussion (like the data shown in Figs. 3 and 4 and the details of the carbon balance calculations) could be better presented in the methods and Results sections.

Response. We took into account the pertinent suggestion from the 3 rev to re-structure the MS. We hope that this new structure is suitable.

Other comments

Unify abbreviations: DUST-Meso or DUST-mesocosms, not both.

Response. Done

P. 1712, lines 13-15. Give here information on the depth of the mesocosms (it is given 3 pages later),

Response. Done

P. 1713, line 24; improve the explanation of the method.

Response. The following section has been added: "Calibrations were performed daily between 200 and 250 µmol O_2 L^{-1} using a KIO^3 standard. The regression between O_2 concentration and absorbance at 466 was performed using standard software to obtain the slope. The intercept corresponded to the reagent blank and averaged 0.25 µmol O^2 L^{-1}. The detection limit was 0.4 µmol O^2 L^{-1}."

P. 1715, lines 23-27. Given the differences in light conditions, the assumption that the NPP measurement at 5 m is representative of the NPP for the whole mesocosm water column should be used with some caution.

Response. This extrapolation was based on tests performed and detailed in Ridamed et al., 2014. A short synthesis of what was done and the results was added to the method section: "Based on (1) the significant similarity ($p>0.05$) of the Chl a concentrations measured at 0.1, 5 and 10 m depths in the 3 experiments and (2) the comparable results (± 4 %) found for depth-integrated PP taken into account PP measured at 0 and 5 or 0, 5 and 10 m (DUNE-1) and measured at 5 or 0, 5 and 10 m (DUNE-2) on selected sampling days (see details of this test in Ridame et al., 2014), the depth-integrated fluxes of PP were estimated assuming that the measurements at 0 and 5 m (P, Q) and at 5 m (R) were representative of the flux over the entire mesocosm (Ridame et al., 2014)."

P. 1716, line 11. Why the title "Orders of magnitude of the . . . " rather than "Magnitude of the . . . "?

Response. Changed

P. 1716, line 18. What was the depth of the sediment traps?

Response. This is specified now in the Methodology section: "Sediment traps located ~14.3 meters above the surface of the mesocosms erc."

P. 1719. The first two paragraphs are difficult to read; please, clarify. I could not find the work Desboeufs et al. (2014).

Response. The structure was totally changed; these paragraphs no longer exist.

Desboeufs et al. was published in BGD since.