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Interactive comment on "Dust deposition in an oligotrophic marine environment: impact on the carbon budget" *by* C. Guieu et al.

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"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). There are several problems related with the MS, and it should be subjected to a thorough revision before accepting it. I will detail some

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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. 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. 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). 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. In summary, the great number of assumptions involved in the equation presented, the number of concerns and the lim-

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ited 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.

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. 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. 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. 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. 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

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

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. 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.

Tables and figures. Be consistent along the text and figures in relation with: (i) 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); (ii) the names given to the experiments (call them always DUNE-Q, DUNE-P, DUNE-R, and not Q, R and P); (iii) 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). 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. 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). 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. 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. 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. 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.

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

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Spain), J. Plankton Res. 32, 1231–1240 doi:10.1093/plankt/fbq045, 2010.

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