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Comment

Interactive comment on “Autotrophic and heterotrophic metabolism of microbial planktonic communities in an oligotrophic coastal marine ecosystem: seasonal dynamics and episodic events” by O. Bonilla-Findji et al.

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Comment by authors: we thank the referee#1 for the thorough comments, which have strongly improved the manuscript.

Anonymous Referee #1

Comments This manuscript presents interesting seasonal data of planktonic metabolism in the Western Mediterranean Sea, at one station. The yearly evaluation of the planktonic metabolism month by month is rare in published reports but essential

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and important to understand how the planktonic communities maintain and control their metabolic processes through the seasons and through the years. However, there are some general aspects that could improve the manuscript.

1. In general, there is a considerable lack of statistics along the results and discussion that could allow to strengthen the interpretation of the results.

ANSWER: It is true that there is not much statistics in this study except correlation analysis. However, this was also dictated by our approach: to assess seasonal and episodic variation. So, there is not much more choice than a description of the data during the events. For example, a dust event is singular; what kind of replication could we use for that? However, the strength of such a study is that this is the real system and that have no biases from experimental approaches. Nevertheless, we have tried to do more statistics. For that we used changes in sigma-t to define hydrological periods differing in water stability. For these periods (stratified, mixed and semi-mixed (Bustillos-Guzman et al. 1995) we present statistics in the revised version of the manuscript.

2. There is also a lack of references along the Introduction and the discussion.

ANSWER: We have added more references

3. A general description of the sampling location is missing: water masses, current, blooms?, . . . etc.

ANSWER: A description has been added.

4. In the discussion, the part treating on the impact of the forest fire dust and Sahara dust on the planktonic communities were interesting and I think that need to be improve (how the dust were quantified? How did you differentiate dust from Sahara and others?).

ANSWER: The dust was not quantified and we do not claim that in the paper. For the forest fire period, we used just data on forest fires in the region (obtained for national web page). As winds are typically westerly and the study site in the east of the forest fire area considered, it is likely that deposition also occurred in the bay. At one

occasion, the deposition of fly ash into the bay could be observed directly. We argue that the enhanced forest fires in summer 2004 could have caused the observed high phosphate concentrations (which fall usually below the detection in summer). The Sahara dust event in February was clearly documented by meteorological services (e.g. EUMETSAT) and the development of the dust storm from the Sahara across Europe was documented by these services (apart from the fact that the red stain of this heavy Sahara dust event could be seen on houses and cars for weeks). In addition, some data collected during the Sahara dust event at the Cap Ferrat meteorological station adjacent to the bay of Villefranche were made available by Christophe Migon and presented as personal communication.

Specific comments: Introduction: P2035, L20-24: Please, add some references

ANSWER: Added

P2035, L20: “in temperate systems, a spring bloom is typically followed by . . .” in general? For Coastal ecosystems? Open ocean? Please, precise.

ANSWER: Corrected.

P2036, L1-3: Please, add some references (e.g. Dachs et al, 2005)

ANSWER: added

Methods: Study site and sample collection In my opinion, a section treating about the description of the sampled location “Point B” is missing (environmental properties, biological activities, current, water masses. . .), especially considering that this “Point B” is well studied.

ANSWER: We added some information on Point B.

P2036, L18-19: How often did you sample during phytoplankton blooms and specific events?

ANSWER: During the bloom 2003 we sampled ca. weekly. For the other events, we

sampled afterwards whenever this was possible. Sampling points for different parameters are given in Fig. 2, 7 and 9 (revised version).

P2036, L20: Why didn't you measure the metabolic rates (GPP, CR and NCP) during those events? It could have been interesting to have those data and compared it with the other measurements. Nutrients dissolved organic carbon particles and Chl a

ANSWER: It would be very interesting but could for logistic reasons not be done. The last sentence seems incomplete

P2037, L5: Please, precise if the TOC, DOC, Chl a and nutrients were measured for each sampled depth or more?

ANSWER: It is not clear what is meant here. We do not claim that we have measured TOC and DOC. Chla and nutrients data are also available for deeper water but no other (biological) data.

Bacterial and planktonic community metabolism P2038, L10-13: Don't you think that a filtration process may affect the bacterial community and so, its metabolism? Do you think that another method could be better to use to measure the bacterial respiration without affecting the community by a prefiltration?

ANSWER: There are now attempts to measure respiration using flow cytometry but this method was not available when our study was performed and is certainly not standard yet (if it ever will be).Also, dilution+regrowth approaches have been used, however , such (long) incubations certainly change bacterial community composition. That prefiltration can cause problems for estimating bacterial respiration is well known (for decades) but there seems to be no real alternative so far.

Results P2039, L15-16: How did you evaluate the depth of the mixed layer? Could you please, give us the mean \pm SE of the mixed layer depth, globally and for each season?

ANSWER: We used temperature, salinity and sigma-t data to evaluate stratification. In the bay of Villefranche,density changes are mainly temperature driven. So, Fig.2

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allows for an evaluation of the depth of the mixed layer. We have explained that in more detail in the revised version. The requested data are not really necessary for the purpose of the paper.

P2039, L18-19: What was the depth of the thermocline during the winter?

ANSWER: There was no thermocline for most of the winter period.

P2039, L22: Please, make the figure larger.

ANSWER: We will transfer this request to the publisher. We used an entire page for the figures.

P2040, L2-3: Are you talking about daily precipitation? Please precise.

ANSWER: No, this is the sum of precipitation. Corrected.

P2040, L10-15: Did you try to collect the dust? If yes, please explain in the methods section the protocol and in the results section, the analysis of the dust collected.

ANSWER: We did not collect dust of this study and did not claim so.

P2040, L17-24: Statistical tests are missing here. Please compare statistically your data.

ANSWER: Most of that is just description, e.g. by stating when maxima or minima occurred. The upwelling event is just a short event, we cannot do statistics for that. For the forest fire period there are also not enough data to perform statistic (except for nitrate and phosphates; this statistics has been added in the revised version). Nevertheless, we have added in Fig. 4 standard errors originating from replicate sampling dates during the forest fire period. In contrast, we have done statistics for three hydrologically defined periods.

P2040, L24: Please, add in the figure caption the units of each parameter.

ANSWER: The data are given as stimulation and therefore presenting the units for

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parameters would not be appropriate. We have extended the legend of the figure, since there seems to be a source of potential misunderstanding.

P2040, L23-24: How can you explain that the nitrate is not affected by the bloom and by the dust inputs?

ANSWER: These lines do not deal with the bloom. Nitrate was actually elevated during the dust inputs.

P2041, L13: Please precise the year of the second peak observed in fall.

ANSWER: Done

P2041, L14-16: Please precise the year.

ANSWER: Done

P2041, L19-21: “. . . but values were typically also low in summer and in autumn.” Please, precise if it was in general low in summer and in autumn or just in 2004.

ANSWER: Done

P2041, L22: “the time and depth integrated” if it is depth and time integrated, the unit of the metabolic rates should be mol O₂ m⁻² d⁻¹ and not μmol O₂ l⁻¹ d⁻¹.

ANSWER: We used a misleading description of what we did. We did not integrate per area but averaged data through the water column and then -in case several time points per period were available- we averaged per time.

P2041, L23: “clearly lower”, significantly? Please compare with statistical test and precise it in the text.

ANSWER: Tests were not possible (see above).

P2042, L5-L20: I am confused about the term that you used as “integrated”. L5 “the integrated R. . .” . . . integrated to what? Depth? L8, you talked about “depth-integrated” here it’s clear. How did you calculate the depth-integrated metabolic rates?

Please add a line in the method's section about it, and if the term "integrated" in L5 did not treat about depth-integrated, please, precise it and explain it in the method's section.

ANSWER: see comment above

P2042, L5-7: ". . .when it exceeded up to 7 times . . ." does its significantly different? Please precise it using statistical test. Please add to your values their corresponding SE.

ANSWER: See above. Error bars were added

P2042, L9: The figure cited here doesn't correspond.

ANSWER: Corrected

P2042, L14-15: The figure cited here doesn't correspond, I think you referred to the Fig. 7 a, and not b, no? Please add the equation of the regression in the graph or in the caption corresponding and precise on the caption the regression model that you used for.

ANSWER: Done.

P2042, L18-20: Did you evaluate the threshold of integrated GPP? Please determine it. What did you mean when you said, ". . . exceeding by 2.4 times the average measured GPP"? What is your explanation to this observation?

ANSWER: Corrected

P2042, L22: Bacterial abundance has as unit cell ml⁻¹, no?, please correct it.

ANSWER: Corrected

P2043, L1, L6-7, L10-11: Significantly higher? Please precise it.

ANSWER: see above

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P2043, L7-9: How can you explain that only BP changed with the dust? What is your explanation about the lack of effect of the dust on the BR, BA and other biologic parameters?

ANSWER: As discussed in the appropriate section, one explanation for that could be that P-limitation of bacterioplankton is relieved. Also, BP might have been just affected stronger than other parameters. Stimulation data were only used for parameters which showed a stimulation during the forest period compared to both the month before and the month afterwards (except for GPP, which was shown as an important parameter not affected). BR was e.g. stimulated in July-Aug compared to September but not compared to BR.

P2043, L24: “. . . at all depth” it is not true, look the figure, at 20 m depth. Please correct it, or correct the month (March, no?)

ANSWER: Corrected

P2044, L6: “BR decreased and BP increased with BGE”, it was expected no, as BGE depends on BR and BP?

ANSWER: This sentence is not clear: If the reviewer means “it was expected, as BGE depends on BR and BP” the answer is: BGE could also be driven by change of only one of the two parameter

P2044, L10: “GGP”, you mean GPP, no? GPP increased with R increasing, it is expected no? What other interpretation could you give to this observation? (slope)

ANSWER: A lot of things can be expected. Following practise in research, we present what we found.

Discussion P2044, L13: “GPP, CR, BR and BR. . .” you mean BP, no?

ANSWER: Yes, thank you for the thorough reading (here and elsewhere).

P2045, L3-4: “total DOC” did you measure it with 14C?

ANSWER: This misleading sentence has been removed.

P2045, L17: Please give a reference of the threshold GPP of the global ocean.

ANSWER: Done

P2045, L25: “horizontal gradient” did you mean “vertical gradient”?

ANSWER: Well, it is both. In the revised version we do not use any adjective.

P2046, L13: “ in the the bay. . .” please correct it

ANSWER: Corrected

P2046, L12-14: Did you observe a P/R ratio positive during the upwelling or/and during fires?

ANSWER: No, during upwelling P:R was 1 and in August P:R was < 1 (Fig. 8).

P2047, L5, L10-11, L14: Please add some references

ANSWER: Added where appropriate

P2048, L19: You talked about the light limitation on phytoplankton, please discuss more.

ANSWER: Here, we discuss the summer situation, where there is no light limitation.

Figure 1 “Irradiance”, PAR irradiance, PAR+UV irradiance? Please precise it. Oct, Nov and Dec 2002 data are missing no? How can you explain that the irradiance in April 2003 is twice higher than in 2004?

ANSWER: It is UV+PAR; 2002 values were mistakenly excluded. Differences in irradiance are likely due to differences in cloud cover.

Figure 2 Please make it larger.

ANSWER: See comment above

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Figure 3 Please correct “No”, write Nb or N.

ANSWER: Corrected

Figure 4 Please add error bars. How can you explain a $GPP < 0$? It is not possible! The y-axis title is not clear, stimulation of what? Fire? Please change it.

ANSWER: It is stimulation of the parameters during the forest fire period. We have given more details on the calculation in the legend.

Figure 5 Please change on the legend of the figures “January” or “February” by “Sahara dust event” “ No Sahara. . .” or other. The unit of bacterial abundance in cell ml⁻¹, no?

ANSWER: Corrected

Figure 7 GPP:P (a) represent the ratio of the volumetric values or depth-integrated? Please precise. Please add the regression equation in the figure or in the caption.

ANSWER: Corrected

Figure 8 Please add units of BA

ANSWER: Corrected

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