

Interactive comment on “Biogeochemical cycling and phyto- and bacterio-plankton communities in a large and shallow tropical lagoon (Terminos Lagoon, Mexico) under 2009–2010 El Niño Modoki drought conditions” by Pascal Conan et al.

Anonymous Referee #1

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GENERAL COMMENT : Overall an interesting case study. Our understanding of such coastal ecosystem, at the interface between the continental and the marine environments in a context of anthropization and global change depends on such studies. The main force of this work is the link between the biological and geochemical points of view. However, such link could be reinforced by improving the discussion section. Moreover, several more or less severe issues need to be addressed all along the manuscript before publication. No additional data is needed, but complementary data analysis could easily promote the quality of the work.

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SPECIFIC COMMENTS

Abstract: - The top-down control is not demonstrated in this work, you cannot mention this so securely as a major driver, neither as a result of your work. Same remark for the discussion section.

Introduction: - The part related to climate change is really oversized when compared to real scientific questions addressed by this paper in its current version. Moreover, the consideration of PAH rather balance the study towards local changes because of human activities. This introduction should at least be less oriented towards climate change and present the challenge of understanding the functioning of such continental to marine environments interface in a context of growing anthropization, which seems to fit better to the design and results of the study.

- The consideration of PAH is not mentioned in the introduction. It is surprising to see it appear in the methods section without any mention in the objectives! The results and the discussion related to PAH are interesting, but you need to include it in your objectives, otherwise it is really hard to see how useful it can be to consider PAH regarding to your scientific objectives. This comment and the previous one are really connected, they should be taken into consideration together. The way PAH are mentioned in the abstract, the importance of oil-related compounds is obvious, this should be presented in this way in the introduction.

Material and methods:

- Section 2.6 and later all along the manuscript: flow cytometry cannot distinguish bacteria from archaea, thus most of the “bacterial” should be replaced by “prokaryotic” or “heterotrophic prokaryotes”.

- Section 2.9: Could you explicit the calculation of this MPN? The way such calculation is performed after two weeks of incubation sounds weird, how is the number of bacteria enumerated after two weeks related to the initial sample? Also refer to my

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corresponding comment in the result section.

Results: - Lines 279-280: very consistent, but could you indicate the approximate value of the ratio in this area?

- Lines 292-294: the correlations concerning aminopeptidase activities are very weak, it should not be presented at the same level than the ones concerning phosphatase activities, which are stronger. This probably means that there is indeed a correlation between these variables, but they are not linear and Spearman's correlation tests the linearity of the relation. You should try non parametric and non linear correlation analyses to further precise the link between these variables.

- Lines 296-299: Need to explain how such percentage is obtained. As it is explained in the current version of the manuscript, one could understand that you counted bacteria in your 2 week enrichments and divided this number by the number of bacteria in your initial samples... This does not sound correct.

- Lines 300-303: I don't understand the cause-to-consequence relationship you mention here (second part of the sentence). You should split the sentence into two very descriptive ones. Moreover, the PAH distribution should be presented at the beginning of this paragraph, then followed by PNM counts, and finally the correlation analysis. Thus the two components of the title should also be switched.

Discussion: - Lines 346-348: The corresponding figures are puzzling because, yes, the normalized productivity per cell seems to be higher, but since the CHL concentration is also lower, one could think that the productivity of the area expressed by unit of volume could be lower. Since you have CHL concentration per liter and since the productivity is expressed by unit of CHL, I suggest that you calculate a productivity by volume of water in order to better compare the productivity of the two sites. According to your graph, you have less than a 2-fold difference in CHL concentration between the two sites but a 6- to 7-fold difference in C fixation rates. Thanks to such analysis, your conclusion will be more robust.

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- Lines 352-353: Could you provide references to support such hypothesis since you did not measure it?

- Lines 359-361: This sentence should be rewritten in a more prudent way since it is only speculation, you have no clue for the top down control and even though you have PAHs concentrations, the direct link with phytoplankton productivity remains to be demonstrated.

- Lines 378-380: sounds weird to justify a recent study by an old one... The old reference could be removed without making the sentence meaningless or doubtful.

- Lines 383-386: These Redfield ratios were not presented in the results sections. They are meaningful and should be presented extensively!

- Lines 410-413: That could appear contradictive. If you suppose that higher concentrations of DOC but lower aminopeptidase activity suggest a higher amount of labile organic matter for bacteria, you should clearly state it.

- Lines 425-427: So vicinity of Palizada river = phosphatase activity but P-depleted zone, meaning very low P-availability for phytoplanktonic growth, which seems consistent with the smaller C fixation rates observed in this zone, am I right? If yes, such a link between nutrients and biological productivity could be added to this discussion, this would greatly strengthen the end of the first sub-section which was up to now very speculative. Moreover, this would also strengthen the links between the geochemical and the biological sides of this paper which appear very few connected for the moment.

- End of section 4.3: A small discussion could be added about distance-based effects and community turnover: according to your data, one could think that strong local selective pressures led to strong shift in the community composition, instead of having progressive gradients and/or fast dispersion that would rather just promote a turnover within the active fraction. Such thinking points out the existence of strong driving pressures that you did not measure in your study, since you do not have such strong

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correlations.

- Lines 460-466: These two sentences should rather be placed at the beginning of the previous paragraph, before discussing about community structure.

- Lines 475-477: Adding a short discussion about "distance-based" similarities in present and active communities, as proposed in a previous comment would perfectly fit with this discussion.

- The link between the severe drought and the potential predictive aspect of this study is not mentioned all along the discussion section, whereas it represents most of your introduction. You need either to remove this "climate-change" part in the intro, or discuss it in the discussion.

Conclusion - Lines 512-514: You cannot say that as it was unambiguous, you did not even measure any top-down parameter.

- Lines 514-517: This sentence does not sound correct since you measured C fixation, thus phytoplankton activity. To measure growth, you need repeated measures in time, that you do not have. So you can definitely not state that there is no growth. I understand it is probably not what you wanted to say, but your sentence could be misinterpreted

- Lines 528-530: again, you are over confident in your hypothesis: "seems to support", you do not have any temporal data to support this hypothesis.

TECHNICAL CORRECTIONS

L 101: replace bacterial by prokaryotic, except if the enzymatic activities have been demonstrated to be specific to bacteria and not to archaea...?

L 112-116: split this long sentence into two, after the first "respectively".

L 185: rather indicate the final concentration like "0.5X" and precise the name of the company which provided the solution

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L 187: provide the optical parameters used to discriminate heterotrophic prokaryotes from the rest (side scatter and green fluorescence?)

Section 2.10: the software used to perform the correlation analyses is not précised.

L 253: "... either in front of the Chumpan river OR. . ."

L 253-254: Rephrase: "Maximal DOC and DON concentrations (82% and 95%, respectively) were measured in front of Candelaria river, whereas the maximal DOP concentrations was observed in front of the Chumpan river"

L 256-257: here and latter on: "Spearman's rank correlation", and use the lowercase greek letter "rho" instead of R.

L 273-274: rephrase: "...were found between CHL and Phae (R=0.82) or PP (R=0.74)". And use PP instead of POP (which was not defined but I assume it means the same as PP).

L 277: "...(<44%)...": since it is a maximal value that you mention here, it would be more logical and precise to indicate "superior to..." (>) instead of <44%, because in agreement to your sentence this ratio is always <44%.

L 303-304: correlation between PP or CHL and which other variable? MPN counts? need to specify it

L 323: "intermediate" (no "d")

L 356: change "the latter also evidencing. . ." in "who also evidenced. . ."

L 358: "relativeLY"

L 472: "where" => which

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