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Discussions

Interactive comment on "Experimental evidence of the potential bioavailability for marine heterotrophic bacteria of aerosols organic matter" *by* Kahina Djaoudi et al.

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The original manuscript entitled "Experimental evidence of the potential bioavailability for marine heterotrophic bacteria of aerosols organic matter" investigates the bioavailability of organic carbon from aerosols to marine heterotrophic bacteria in the Mediterranean Sea. The authors performed dark incubations using natural microbial assemblages exposed to three different treatments under similar initial organic carbon concentrations (glucose solution, Saharan dust-derived carbon and Anthropogenic-derived carbon) and one control treatment (artificial seawater amended with inorganic nutrients). The organic carbon contained in the dust was dissolved into dissolved organic

carbon prior to the experiments. Although the bioavailability of organic carbon from the Saharan dust treatment was similar to the Anthropogenic treatment, the quality of organic carbon was different and reflected on the microbial metabolic response. The bacterial growth efficiency was higher in the Saharan dust treatment than the Anthropogenic treatment, suggesting that organic matter derived from Saharan dust had a higher quality with a significant portion being incorporated into biomass. On the other hand, most of the carbon consumed in the Anthropogenic treatment was catabolized. Interestingly, this contrasting outcome has local impact in the marine carbon cycle, since more or less contribution from the two sources will either stimulate biomass or respiration, ultimately contributing to the carbon pump efficiency.

Overall, the manuscript is very well organized and written. The introduction is complete with a fair literature revision that points out the lack of studies involving the lability of organic carbon from aerosols to marine microbes. The methodology is well developed and it did not raise any concerns from my side. The outcome is really interesting and will for sure contribute to the marine biogeochemistry field. I do have a few comments that would overall contribute to the manuscript and will be addressed below.

1 – The manuscript title is a bit too long, consider rewriting. Two suggestions: "Experimental evidence of the potential availability of organic matter from aerosols to heterotrophic bacteria" or "Potential bioavailability of organic matter from aerosols to heterotrophic bacteria".

2- I would be careful when addressing DOC budgets, extensively mentioned in the introduction and discussion, and comparing with budgets estimated from the experiments. The dust sources used in this experiment were artificially dissolved prior to incubation, but they arrive in the environment as POC. Is it realistic that, in the environment, dust particles would stay enough time at the ocean's surface to be dissolved to that proportion found in Table 1 before sinking? This could be addressed better in the discussion.

3- Introduction: in Line 65: “Nutrient availability and microbial community structure regulate the accumulation and the remineralization of DOM, influencing export efficiency”. I do not feel comfortable suggesting papers of mine, but we addressed this issue through incubation experiments with nutrient amendments and observed DOC and DON dynamics. Bibliography: Bif, M.B.; Hansell, D.A.; Popendorf, K.J. Controls on the fate of dissolved organic carbon under contrasting upwelling conditions. Frontiers in Marine Science, v.5 (463), 2018. I think this can contribute to both introduction and discussion sections.

In Lines 76-88: Although atmospheric dust is of undoubted importance in oligotrophic regions of the Atlantic and Mediterranean Sea, it does not play a role in other oligotrophic regions such as in the South Pacific Subtropical Gyre. This could be better addressed by adding one or two sentences as it gave the impression that dust deposition is important in every oligotrophic region. Example of bibliography: Jickells, T. D., et al. 2005. Global iron connections between desert dust, ocean biogeochemistry and climate, Science, 308, pp.67–71.

4- 2.2.Experimental design:

In the control and glucose treatment, why didn’t the authors add Fe to the solution? This is a limiting nutrient for heterotrophic bacteria, is probably found in very high concentrations in the dust treatments and could make a difference in the C and G incubations.

In the paragraph starting in l.122, was DOC analyzed using the method described in the paragraph starting in l.178? Please clarify.

Line 178: Replace “online from” with “in line with”.

5- 3.Results:

In section 3.1. I see an overall DOC increase in the C treatment instead of a 5μM decrease, observed in Figure 2, Panel A.

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