

Interactive comment on “Experimental evidence of the potential bioavailability for marine heterotrophic bacteria of aerosols organic matter”

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Response to the 1st referee We would like to thank the referee for her relevant comments and suggestions on our submitted manuscript. Here below, we address our response (in bold and in italic) and we highlighted the text modifications in the revised manuscript (in italic between inverted commas). The pdf file is attached below.

Referee 1 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”.

We agree with the reviewer. The title of the manuscript was shortened in the revised manuscript following the suggestion of the referee.

BGD

Modified title (lines: 1-2): 'Potential bioavailability of organic matter from atmospheric particles to marine 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. We may disagree with the referee in this point. In the case of wet deposition events, a substantial fraction of atmospheric particles components gets dissolved in rainwater before deposition in the ocean and, therefore, organic carbon arrives in the environment as a mixture of DOC and POC. In this context, by artificially leaching aerosols in ultrapure water prior to amendments, the experimental design applied in this study mimicked a wet deposition event and focused on water-soluble organic carbon. We have clarified this point in the Methods section and in the discussion as suggested by the referee.

Modified text (lines: 130-136): 'To do so, particles from two whole Saharan dust filters and from 7.9 x 7.9 cm² of the anthropogenic aerosol filter were firstly leached, each in 650 mL ultrapure water. After being sonicated for 40 min, the suspended particles were filtered through pre-combusted GF/F filters (450 °C, 6 h) to recover the dissolved fraction. A volume of 200 mL of each leachate was finally introduced in the corresponding aerosol amended treatments (D and A). Thus, this protocol simulated an input of atmospheric water-soluble organic carbon through aerosol wet deposition, i.e. an input of atmospheric DOC'.

Modified text (lines: 413-416):

'Note that this assumption would apply to the case of wet deposition events in which an

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important fraction of atmospheric particles is solubilized in rainwater before deposition. In the case of dry deposition events, the labile and refractory fractions would depend on the potential solubility of atmospheric organic matter in seawater and its residence time in the euphotic layer.'

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. We would like to thank the referee for this suggestion. This publication was added in the introduction. Modified text (line 65): 'This percentage reaches more than 50% of the total carbon export in the oligotrophic oceans (Carlson et al., 1994; Guyennon et al., 2015; Letscher and Moore, 2015; Roschan and DeVeries, 2017; Bif et al., 2018).' 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. We agree with the referee. This paragraph was clarified in the revised manuscript as follow: Modified text (Lines: 82-84) 'By bringing new nutrients to the upper waters, atmospheric deposition plays a key role in some oligotrophic regions such as the Mediterranean Sea and the Northern Atlantic and Pacific gyres, particularly under stratified conditions (Guieu et al., 2014; Letelier et al., 2019).' 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.

BGD

We understand the concern of the reviewer. However, dust may have provided not only Fe, but also other trace metals that could be potentially stimulating or toxic to bacteria. Given the difficulty to reconstitute artificially the mixed trace metal composition of dust, we decided not to amend the C and G incubations with metals. Please note, however, that even without iron addition, the utilization of carbon in the glucose treatment was higher than that observed in the dust treatment, suggesting that the availability of iron did not constrain carbon utilization in the glucose treatment. This might be explained by the lack of Fe limitation of heterotrophic bacteria in the Mediterranean Sea (Guieu et al., 2002). In addition, the inoculum may have certainly provided Fe to the C and G incubations. Although our experimental conditions were fully adapted for the analysis of nutrients and organic matter, they did not meet the requirements for trace metal analysis. Indeed, trace metal clean conditions would have been necessary to explore this possibility.

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

Yes, DOC concentrations were analyzed following the procedure described in the section (2.3). In the revised manuscript, a sentence was added to refer the reader to the analytical procedure.

Modified text (line: 124):

‘The DOC concentration in the artificial seawater was 6 μ M (see section 2.3 for the analytical method).’

Line 178: Replace “online from” with “in line with”. We corrected it in the revised manuscript. Modified text (Line 188): ‘Samples for DOC analysis (10 mL) were filtered in line through pre-combusted (450 °C, 6 h) 47-mm GF/F filters.’ In section 3.1. I see an overall DOC increase in the C treatment instead of a 5_M decrease, observed in

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Figure 2, Panel A. The decrease of DOC was calculated by considering the observed maximal and minimal values in the control. This clarification was added in Table 2. In the control treatment, the decrease of DOC was calculated between $T=5.7$ d and T_{final} as an increase was observed at the beginning of the incubation experiment. Please see Table 2 in the revised manuscript.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-187/bg-2020-187-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-187>, 2020.

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