Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-187-RC2, 2020

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

Anonymous Referee #2

Received and published: 24 August 2020

This manuscript reports and contextualizes the impact of atmospheric nutrient additions (water soluble fraction, WSF) of natural (Sahara dust, D) and anthropogenic (A) origin on the heterotrophic bacteria of the surface Mediterranean Sea. The experimental approach consisted on dark microbial incubations in artificial seawater with controlled additions of atmospheric nutrients and a common bacterial inoculum. Apart for the two atmospheric treatments (D and A), glucose (G) and control (C) treatments were also conducted for comparison. Chemical (DOC, DON, DOP) and microbial parameters (BA, BP, EEA) were followed during 2 weeks.

The experimental approach is adequate and the results –contrasting LDOC (%) and

BGE (%)– are interesting and potentially relevant in the context of the increasing fluxes of organic nutrients from the atmosphere to the surface ocean, particularly in oligotrophic seas close to densely populated areas (e.g. the Mediterranean Sea). However, there are some issues that should to be clarified / commented / discussed by the authors:

1) Inorganic nutrients were added to the microbial cultures to avoid inorganic nutrient limitation. Final concentrations were 1 uM-N and 0.3 uM-P, which are well above the expected concentration in the surface layer of an oligotrophic region. Given that inorganic nutrients have been added in the same concentrations to all treatments, comparison among treatments is valid but. . . what about extrapolation of your results to oligotrophic conditions in the field?;

2) The WSF of Sahara dust and anthropogenic aerosols do not contain only DON and DOP but also inorganic N (ammonium, nitrite and nitrate) and phosphate. Therefore, even if you have not added inorganic nutrients to the microbial cultures (see point 1), inorganic N and P would be added in the WSF. In this regard, it is relevant that the concentrations of inorganic N and P in the WSF are presented and discussed;

3) It is reported that the DOC concentration in artificial seawater was 6 uM and in the WSF <0.3 uM (once diluted in the ASW). However, the average DOC concentration in the control treatments was 19 uM (calculated from Table 2). What caused this difference? Should we assume that it also occurs in treatments G, A and D?;

4) The estimates of LDOC(%) are obtained for the 4 treatments (C, G, A and D) comparing the initial DOC and the DOC decrease of each treatment. However, a considerable part of the initial DOC is already present in the control treatment (see point 3). Therefore, how should we interpret the LDOC(%) numbers in Table 2? For example, if the DOC decrease in the control treatment is 5 uM and in the G treatment is 22 uM, the DOC decrease exclusively due to the glucose addition should be 17 uM. Given that the DOC of the glucose addition is 40 uM and in the control is 19 uM, the LDOC(%)

of the glucose addition would be 81% (=(22 – 5) / (40 – 19)). This is very different from the 55% in Table 2. For treatments A and D the difference is not so large but it is conceptually important;

5) The same reasoning is applicable to the BGE(%) calculations (LDOC is in the denominator of the formulae). For example, for the G treatment BGE(%) should be 9% (=(1.7 – 0.7)/ (22 – 5) and for the D treatment 26% (=(1.19 – 0.17)/(9 – 5)). It really makes a difference;

6) Also concerning BGE (%), it should be better to use the bacterial biomass, calculated from BA with a conversion factor, rather than BP; and

7) Extrapolation of your results to the entire Mediterranean Sea is a bit risky. The fluxes of organic nutrients to the surface layer of the Mediterranean Sea are (probably) an overestimate. Your daily and average annual atmospheric fluxes are obtained from just to two points, off Marseille and in Lampedusa. Do you think that they are representative for the entire Mediterranean Sea? I do not believe that. Section 4.3 is very useful but you must prevent to give the impression that your calculations can be extrapolated to the entire Mediterranean.

MINOR DETAILS

Lines 224 and 226. You are referring to Figure 2, not Figure 1. Please, correct.

Line 422. These numbers should be divided by 1000 or expressed in mol C.

Line 680, Table 2. Please, add a column with the initial concentrations of DOC even if it is redundant.

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