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Interactive comment on “Ecology of aerobic anoxygenic phototrophic bacteria along an oligotrophic gradient in the Mediterranean Sea” by D. Lamy et al.

Anonymous Referee #2

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General comments. Lamy et al. studied the ecology of aerobic anoxygenic phototrophs (AAP) along the Mediterranean Sea, using the existing trophic gradient from west to east to depict which are the main factors that control the AAP community. The authors have prepared a well-written paper in which they show the abundance and distribution of AAP using three different methods, and additionally they performed some nutrient addition experiments. The paper reads well, it is clear and in general the results are properly organized and discussed. I only have a few minor comments listed below. Specific comments. P. 326. L. 1-5. Refer also to their fast growth, which is actually a relevant aspect of these bacteria regarding their role in aquatic food webs. P. 328. L. 8. Change ‘dissolved inorganic phosphate’ to ‘soluble reactive phosphorous’ which is

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what is being measured by the Rimmelin and Moutin procedure. P.336. L. 21. The low phosphate concentrations and short turnover times indicate that the whole microbial community is subjected to P scarcity, not only phytoplankton as mentioned by the authors. This affirmation should be changed. P. 336. L. 24. Looking at the data presented in Table 1, there are some parameters such as Chla, POP and POC that indeed reflect the increasing west-to-east gradient in oligotrophy as mentioned by the authors. However, it's surprising to me that phosphate concentrations and phosphate turnover time data indicate that cells were more limited by P in the west and in the east, although the Eastern Mediterranean is typically known by its P-limitation. I think this is also the reason why the authors did not observe a strong stimulation on bacterial growth when adding P, but they did when adding N and glucose, and it should be discussed in page 339 (see comment below). P.338. Discussion. The results show that AAP numbers decrease when increasing oligotrophy, which clearly goes against Kolber's original paper in 2000, in which it was proposed that AAP would perform better under oligotrophy. This should be clearly reflected in the first paragraph of the discussion (L5-15). P.339. L.16-22. The initial conditions of the experiments show that in fact bacteria were more P-limited in eastern than in the western stations, and this might explain that there was no stimulation of AAP when adding P. These initial conditions do not reflect the usual conditions of the Med Sea, where the eastern part is one of the more P-limited areas of the world. I would like to see a discussion on this in the last paragraph of page 339. Also the authors are right that to see a clear effect of nutrient additions it's better to use experiments where most of mortality sources are reduced by dilution or filtration for example. I wonder why the authors decided to use whole seawater incubations? P. 348. Table. 1. Change the order of the columns to St. A, 17, 9, C. Every other figure is organized with stations from western to eastern Mediterranean and the current order of the table makes it a bit confusing. Page 350. Figure 1. Make a difference between stations sampled and not sampled. The methods section says that 8 stations were sampled, but looking at this map it seems that the conclusions of the paper are based on all these stations, and I believe it's misleading. Typing errors. P. 335. L 4. There is

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a comma instead of a dot after stations.

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