

Interactive comment on “Synergistic effects of UVR and simulated stratification on commensalistic algal-bacterial relationship in two optically contrasting oligotrophic Mediterranean lakes” by P. Carrillo et al.

Anonymous Referee #2

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Review of the manuscript by Carrillo and colleagues submitted to BG

General Comment: Carrillo and colleagues present here results on the effect of UV radiation on phytoplankton primary production and extracellular release and bacterial heterotrophic activity in two oligotrophic lakes that differ with respect to water transparency. The authors apply an exhaustive experimental setup, including different types of radiation regimes and intensities. The originality of the present work lies in the combined investigation of phytoplankton and bacterial parameters in contrasting lake ecosystems, and these data merit publication. I do, however, have one concern that I

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recommend to be considered in a revised version of the manuscript. Photochemistry, in particular the photochemical transformation of DOM, is more or less neglected in this manuscript, both in the Introduction and the Discussion of the results. I fully understand that it was not possible to include yet another process to the already dense program. But I think the authors should consider these abiotic processes, such as the photochemical consumption of dissolved oxygen or the transformation of DOM to more or less biologically labile forms, in the interpretation of their results. There is extensive literature on this topic that can be used as a basis for discussion. The paper is well-written and the results of this overall complex experimental setup are clearly described and illustrated in figures and tables.

Specific Comments Abstract : L. 13-14 This sentence is not easy to follow, because the type of relationship between algae and bacteria is not defined. I suggest the authors explain more explicitly their understanding of strong or weak relationships between bacteria and algal exudates. In a general manner, I prefer the term “phytoplankton” to “algae”, because this latter could also make reference to macroalgae. Introduction. The Introduction focuses on the direct effects of UV radiation on phytoplankton and bacterial activity. I was missing a short description of the effects of UV-induced photochemical processes of DOM that will certainly play an important role in the context of the present study. p. 12592, line 25: The authors expect the readers to be familiar with terms like “B1 and A1 FI scenarios”, which is probably not the case. I suggest reformulating this sentence. p. 12595, line 25-28: This sentence is vague. If the authors want to point out this issue, I suggest they explain in a little more detail the arguments of the paper in question.

Material and Methods. p. 12597, line 3-5. Can you consider the food web as “simple”, just because autotrophic picoplankton are missing? Further, this sentence is not clear: What do you mean by size overlap? p. 12597 and 12598: I find it very difficult to follow so many different abbreviations: HBP, TPR, BR, PAB, PA, P, MIR. I suggest the authors change at least some of them, e.g. PAB to UVB+UVA+PAR, PA to UVA+PAR, P PAR

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to facilitate the reading of the manuscript. p. 12602. Respiration rates. How do the authors deal with the photochemical oxygen demand that occurs concomitantly with bacterial or plankton respiration? Do the authors have any previous estimates on this process in their lakes? Neglecting the photochemical oxygen consumption could lead to an overestimation of the respiration rates in the light bottle incubations. This might affect some conclusions as that stated on p. 12608, line 16-17. See for example the recent paper by Kitidis et al. (2014) in *Limnol. Oceanogr.*

Figures. Fig. 2. Can the authors use different symbols for the yield and chl_a in figure a and b? Fig. 3. It is not explained in the legend what the different letters stand for.

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