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Interactive comment on "Differential response of planktonic primary, bacterial, and dimethylsulfide production rates to vertically-moving and static incubations in upper mixed-layer summer sea waters" *by* M. Galí et al.

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We thank R#1 for his/her positive comments on our manuscript, and reply below to some of the criticisms made, which surely will help to improve the revised version of the paper. We have replied mainly to those questions that can stimulate scientific debate. The technical details will be dealt with after the discussion phase. (R = reviewer; A = authors).

SPECIFIC COMMENTS

Title:

R: I propose (optionally) the following title because, as acknowledged by authors in conclusions, "the irradiance dose-response in mixing bottles was distinct (though subtle) in each of the processes measured. . ."; besides, it may reinforce the idea of static vs. dynamic light field: "Subtle differential response of planktonic primary, bacterial, and dimethylsulfide production rates to static vs. dynamic incubations in upper mixed-layer summer sea waters"

A: We will consider the proposed changes. Adding the word 'subtle' and 'dynamic vs. static incubations' can help the reader understand that the novelty of our study is the comparison of static and dynamic incubations rather, than the gradients in static incubations.

Introduction:

R: Good review of the scientific background, and interesting questions posed. However, I do not feel entirely comfortable with the statements in pg. 8855 lines 18 to 21. I think that mixing treatment resembles more realistic conditions than fixed-depth incubations within UML (because water and organisms indeed experience vertical movement and dynamic light exposure in real UML), even though the experimental mixing times were faster than current mixing times, according to calculations and statements in pg. 8861, lines 10 to 11. Hence, I would include this point of view in the Introduction (e.g., in pg. 8855, after lines 18 to 21).

A: In fact, it is very difficult to assess the actual mixing rates and even more to simulate them experimentally. Perhaps in lines 18-21 it should be stated that both the dynamic and the static treatments represent a perturbation. They possibly lay at the dynamic and static end of the conditions found in the upper mixing layer (UML), and the prevalent mixing conditions are somewhere in between.

Methods:

R: Pg. 8856, lines 12-16: Mixing times were distinct between coastal (C1, C2) and oceanic experiments (O1, O2). This introduced a different fluctuating light regime between these environments, even though in C1 and C2 the bottles were incubated at shallower depths to approximate the equivalent in situ optical depths. This could make the two types of environments less comparable between them, and perhaps making less appropriate the pooling of results from all experiments. This would deserve some discussion, particularly regarding to the apparent different behaviour shown by C1 for most of variables (even different to C2).

A: I agree in that experiment C1 displayed a different behaviour, especially in terms of primary and bacterial production (but not so much in terms of gross DMS production). However, note that the initial samples in experiments O1 and O2 came from different light histories (due to a storm) and their experimental response was also slightly different. We decided to pool all the samples together to give more statistical power to our inferences or, in other words, to focus the discussion on the common trends. Discussing the differences between individual experiments can be interesting, but one can get lost in subtleties caused by a number of unknown, uncontrolled factors, that are inherent to working with natural samples.

R: Pg. 8857, lines 24 to 26. Describe better how this calculation was made e.g. were expressed as rates per hour or per (incubation) period? How was integrated the 2 h of incubation under dark in the presence of tracer with the prior incubation time under light?

A: The rates were expressed in pmol leu/h. We assumed the first dark incubation (done after the first 2h of light exposure) to represent the 2h period, and the second dark incubation to represent the subsequent 4h period. If BP1 is the rate in the first period and BP2 that in the second period, BP_final = (2/6)*BP1 + (4/6)*BP2. We are aware that this is a subjective approach, but we found it more consistent with the treatment of C1 and C2 experiments. This will be more clearly sated in the revised version.

Statistical analysis:

R: Pg. 8860, line 2: How the integration was calculated? Please, detail further.

A: The integration was calculated as the sum of trapezoids formed by 'depth' (in the vertical axis) vs 'rate' (in the horizontal axis) data points.

R: Pg. 8860, lines 5-6. It would be worth to use modern robust statistical methods instead or complementarily to classical non-parametric statistic to corroborate differences among treatments when assumptions for parametric tests are not met (e.g. ANOVA based on percentile bootstrap method; see Erceg-Hurn & Mirosevich 2008, Rose et al. 2009).

A: I will explore (and be happy to learn) these techniques. Bootstrap confidence intervals seem to be implemented on Matlab R2009b. I agree that confidence intervals based on bootstrapping can make the statistical tests more robust.

Results and discussion:

R: Good description of results and discussion, conclusions, and the arrangement of the sections. Nevertheless, I miss a discussion about broader ecological implications of the results. I feel the valuable responses to solar radiation found, particularly of variables with biogeochemical relevance (e.g. primary and bacterial production, DMS production...) through the depth gradient (fixed incubations) and the subtle effects of mixing, deserve a more extensive discussion focused on their implications in the context of global warming, and within theoretical frameworks of (controversial) CLAW hypothesis, summer DMS paradox, and Earth-system theory (after Vallina & Simó 2007; Quinn & Bates 2011, Lana et al. 2011, 2012, Galí et al. 2013). Thus, as an example, the results found at surface and middle static incubations jointly may mimic the scenario of expected prolonged shallower stratification due to global warming, confining plankton long within hypothetical photoactive UVR damage layer (Fig. 1). In this way, (i) the maximum PPp found at middle depth, offset by mixing (resembling values from surface

depth, i.e. subjected to inhibition), (ii) the absence of significant variation with depth (and mixing) of LIR measured under complete light exposure in presence of tracer (Fig. 5B) that may be judged as very realistic measurement of bacterial activity, and (iii) the sharp vertical gradient of gross and net biological DMS production (increasing with irradiance, but largely compensated by DMS photolysis), with a neutral or slight reduction due to mixing, are results that, in overall, give room to discuss in the context of shallower stratification (global warming) and the CLAW hypothesis, particularly after controversies introduced by Quinn & Bates (2011). In this line, not only DMS but also organic matter (dissolved and particulate, of biological origin) or even (volatile and non volatile) photodegradation products of DMS (also of biological origin) can affect the formation of cloud condensation nuclei (by bubble bursting at the ocean surface) at large scale. The underlying idea of this claim is that the integrated operation of biotic and abiotic variables can reinforce regulation of Earth-system (after Cresser et al. 2008, Kleidon 2010, 2012). I strongly encourage authors to include some of these aspects in discussion to reinforce the implications of their results.

A: I thank the reviewer for the comprehensive discussion. I agree with him/her that these implications are very interesting regarding climate change/regulation, but I do not want to overextend the implications of our data. In my view, our results only allow to assess short-term responses of the plankton community. On the mid term (days, weeks) I would expect that the plankton thriving in a given water mass would be be replaced by better adapted communities if a change in the mixing conditions persisted. On the long term, I would expect that persistent changes in the mixing regime would change the distribution of biogeochemical regimes (provinces). That is, I believe that performing time-for-space substitutions should be more useful than manipulating a given community to make predictions about the effects of climate change.

Tables and Figures:

R: Figs. 4 and 5: If all variables (except DMS production rates) were measured in duplicate incubation bottles, as stated in pg. 8859 lines 24-25, there are enough variability

to perform the statistics...?

A: We wanted to analyse all the results within the same statistical framework. In fact, considering replicates in the ANOVA will increase its statistical power. A first problem we encountered is how to include “technical replicates” or “subsamples” in the Kruskal-Wallis tests at a hierarchical level below that of truly different samples (C1, C2, O1, O2). Depending on the solution we adopt (that is, using bootstrap CIs for statistical tests) we will use the replicates or not.

R: Fig. 7: Report values of R-squared, slope and significance of regression, for fixed depth and also for mixing (either alone or fixed-depth + mixing) incubations, in order to show to which extent mixing treatment disrupted photoacclimating and photodamage processes.

A: “Showing to which extent the mixing treatment disrupted photoacclimating and photodamage processes ” was the purpose of Fig. 8. We will calculate the confidence intervals of a linear least squares regression, including or excluding mixing treatments, if the reviewer judges it more appropriate.

Interactive comment on Biogeosciences Discuss., 10, 8851, 2013.