Biogeosciences Discuss., 10, C3707–C3709, 2013
www.biogeosciences-discuss.net/10/C3707/2013/

Biogeosciences Discussions
Open Access

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.

Anonymous Referee #1

Received and published: 24 July 2013

I thank authors their kind and valuable reply to my comments. Nevertheless, before the end of discussion phase, I briefly reply only to the authors' responses for which I do not completely agree or I judge relevant to stimulate scientific debate...

Pg. 8855 lines 18 to 21: I agree with every idea of the authors' reply but I continue thinking that they also should recognize in the ms. that experimental mixing represent conditions nearer to reality than the experimental fixed-depth treatments (because of water movement within UML), even though actual mixing rates in UML were distinct than those simulated by experimental mixing... The results found from fixed-depth

treatments would serve rather as "experimental controls" for the comparison with the planktonic responses to UVR found under mixing.

Pg. 8856, lines 12 to 16: I agree in that discussing the differences between individual experiments (e.g. C1 vs. whatever other) is out of focus of the paper and may lead reader to (perhaps) futile questions difficult to solve. However, the major differences found between C1 and the rest of experiments, at least for PPp, BP (but not so much for the light exposure incubations, fig. 5B), and Net bio. DMS production, deserve some justification to be included in the pool of all experiments. This should include not only the different light history (already justified in the ms.) but also the different mixing time imposed (and hence a different fluctuating light regime) with respect to the oceanic experiments. Besides, I think that this concern may satisfactorily be solved by showing statistics results, both including and excluding C1 (as was displayed in fig. 4A, PPp), for the response variables for which C1 behaved more divergently.

Pg. 8857, lines 24 to 26: I may agree with how time-weighted of BP rates were calculated, i.e. how much weight is given to each of the two incubation periods. However, I disagree with the reason given by authors in their reply to justify the calculations, i.e. that second dark incubation represents the subsequent 4h period. My disagreement is based in that these incubations were performed during the entire light exposure, i.e. 6h, to be consistent with that stated in pg. 8857 lines 25 to 26. Therefore, this issue should be better clarified and justified in the final version. Besides, authors should be aware (and reflect it in the paper) that the 2h of dark incubation with the tracer represent an important share of the entire incubation period, and a time when net repair of photodamage can be operating.

Results and discussion: I disagree with authors' reply in that their results only allow to assess short-term responses of the plankton community to avoid dealing with regulation/climate change implications. I think that their results deserve the inclusion of some broad-scale implications, as pointed out in my former comments. I am aware of the risk to fall in speculative discussion, and I agree and recognize the reasons

given in the authors' reply to make them reluctant for including implications of this type. However, I think that the point of their experiments and results would be to go somewhat further than a mere description of concrete experiments, even though the study of short-term planktonic responses is an interesting theme "per se". I think that some of their short-term results are extrapolable to scenarios of shallowing stratification, which may follow an intermittent or fluctuating pattern (i.e. transitorily returning to original mixing conditions) rather than a permanent modified (shallowed) mixing regime, under global warming, at least at early stages. This could partially refute the argument of the replacement by a better adapted plankton community as basis to reject the inclusion of mid-term implications. Following a phylosophy of science perspective, you may extrapolate the short-term results at future scenarios, "ceteris paribus". The latter could refer here, for example, to unchanged plankton community, plausible with still non-permanent changes in mixing regime. Definitely, I insist encouraging authors to include some of these implications (and mentioning warnings, caveats, cautions) to provide the paper a broader perspective, if editors agree with this idea.

Fig. 7. Yes, please, I prefer to see R-squared, slopes and significance of the regressions (fig. 7), even though fig. 8 deals with a similar purpose.

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