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Biogeosciences Discussions

Interactive comment on "Decreased calcification affects photosynthetic responses of *Emiliania huxleyi* exposed to UV radiation and elevated temperature" *by* K. Xu et al.

Anonymous Referee #2

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General comments

This paper presents an interesting set of experiments looking at how degree of calcification may affect susceptibility to UV damage (particularly of the photosynthetic apparatus) in a cultured strain of the coccolithophore E. hux. The degree of calcification of the cells was controlling by manipulating Ca^{2+} in the medium. This work follows on previous work by the same group looking at interactions between UV and calcification in coccolithophores grown at different CO_2 levels. The experiments appear to be carefully done and the paper will not require major revisions to be acceptable for final publication. In some cases, all that is missing is appropriate qualifying text. I have

listed my biggest general comments here, followed by a number of minor edits and other smaller issues:

I agree that the overall experimental design using high- and low-Ca^{2+} medium to investigate changes in calcification is "physiologically... an effective way to investigate the role of calcification"(p. 860). Ecologically though, other than in the Black Sea (the Cokacar et al. 2001 reference that is given in the text here), I am not familiar with many places where coccolithophore blooms occur at low salinities. Some other references here would bolster the ecological relevance of this methodology- for instance, have coccolithophore blooms been recorded in the Baltic or other estuarine systems?

Another consideration is that the physiological and geochemical consequences of controlling calcification by lowering seawater [Ca^{2+}] could be different than those of other limiting factors, such as changes in the carbonate buffer system. The authors have published some nice experiments on UV and CO_2 interactions in the past (referenced here), but they should still be cautious about extrapolating too freely between these two different ways of limiting calcite production. Perhaps adding some text to the discussion to recognize this would be a good idea.

These experiments exposed the cells to relatively intense levels of UV-A and UV-B for a very short period of time (2 hours, p. 861, Methods). This shows the responses of heavily- or lightly-calcified cells to a single traumatic UV stress event. How would their responses differ if UV irradiances were less intense, but maintained over much longer time periods (generations)? This type of lower level chronic exposure is certainly also potentially environmentally relevant. Perhaps it would be appropriateto add some consideration of this issue in the discussion section as well.

Minor comments

P. 860 line 8-9: this is the wrong reference for the Aquil medium formulation. Instead please use:

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Price NM et al. (1988/89). Preparation and chemistry of the artificial algal culture mediumAquil. Biological Oceanography 6: 443–461.

P. 860 line 12: Coccolithophore is mis-spelled.

p. 861 line 9: The watts units for irradiance are obsolete and probably not needed, just present the SI units, μmol photons m^2 sec^{-1}

p. 864: Which carotenoids (or xanthophylls) from *E. hux* would you expect to be included in these measurements using a classic spectrophotometric method from Strickland and Parsons? I assume they include 19-hexanoyloxyfucoxanthin, and what else?

p. 864, line 15 and Fig 1a: This SEM appears to show a lysed or ruptured cell- is there a better picture of an intact cell without coccoliths availablefrom this treatment?

Fig 3 and p. 865 of the text: The decreased inhibition in HCa compared to LCa treatments is not very obvious for either UVA or UVB alone, it is most noticeable in the UVR graph. Even here, the differences in inhibition are relatively small. The same is true for the declining (Fig 3a) and increasing (3b) trends with time, they may be statistically significant, but they are not very big. It would be good to mention this in the text here.

P. 865, lines 26-29, and figure 4: The difference in NPQ values for HCa and LCa cells is not only “more evident” early in the incubation, it actually seems to disappear almost completely by 2 hours (Fig 4c). A more careful description of the data trends is needed here.

p. 866, line 18: Define C/P ratio for readers here. You mean Calcification to Photosynthesis, but it could be read as Carbon to Phosphorus.

p. 867 and Fig 7: Since there were no significant treatment-related trends in the BWFs, this graph is not very useful to the paper. It could be left out and this could be stated briefly in words.

p. 868, line 27: This reference appears to have a typo. Is “Adams III” a correct sur-

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name?

p. 869, line 22: “lose”, not “loose”

p. 869, line 27 to p. 870: This sentence is long and awkward and should be re-written.

p. 870, line 26: How were enzymes like phosphatases “affected” by low Ca in Shaked et al? Some elaboration is probably needed here.

p. 870-871: The authors need to be careful about drawing string parallels between these experiments with coccolithophorecultures, and calcification by corals on the Great Barrier Reef. This may be over-intepreting your results a bit.

p. 871, line 12: This is an excellent point, yes E hux is very cosmopolitan species and is tremendously diverse genetically and morphologically, and various strains and species will differ greatly in their responses to temperature. The authors may want to speculate whether this could also be the case for responses to UV radiation.

Interactive comment on Biogeosciences Discuss., 8, 857, 2011.