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Biogeosciences Discussions

Interactive comment on "Effect of Ocean acidification on growth, calcification and recruitment of calcifying and non-calcifying epibionts of brown algae" *by* V. Saderne and M. Wahl

Anonymous Referee #3

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The paper by Saderne & Wahl addresses an interesting question of how calcifying and non-calcifying epibionts respond to ocean acidification (OA) and results are interesting as they show divergent responses to OA. However, the manuscript needs some major revisions and re-shaping is needed that should be more to the point of what can really be addressed and answered with the experiments and the analyses carried out.

The question of the function of "boundary layer" and chemical microenvironment produced by photosynthesis and respiration of Fucus is interesting, but cannot really be answered with the methods applied. Already, no respiration/photosynthetic rates were

measured nor their influence on the boundary layer. The authors wanted to test the hypothesis "that the diurnally fluctuating physiologcal activity of the host alga will create conditions in the boundary layer which are favorable or disfavorable for calcification" . . . As neither the physiological activity of the host alga was assessed nor the difference in the chemical conditions of the boundary layer, this hypothesis can not be tested with the experiments/analyses carried out. Although it is very interesting to see the divergent response of Spirorbis juvenile growth in light and dark can be an indirect evidence for a more favorable chemical environment in light due to photosynthetic performance of Fucus, but this is not necessarily so and is actually contradicted by the fact, that there was no pCO2 effect in the 2-way ANOVA. The light-dark divergence might simply be due to other diurnally controlled constraints such as feeding patterns and thus energy supply. There is no assessment or even estimate of how different the carbonate chemistry in the boundary layer might be between light and dark treatments. The authors give an indication of a shift in pH to up to 9 during day light in general (thus presumably under ambient conditions). If discussing any likely effect related to carbonate chemistry in the boundary layer, the authors should at least try to give an estimate pH range that may be expected and consequently of other parameters of the carbonate chemistry at each of the three pCO2 levels if they want to address this issue. Anyhow, it is not very likely that the growth divergence between light and dark is (solely) due to the carbonate chemistry. It is more likely that other factors related to light/dark cycle are important and are even overlaying any pCO2 effect and thus might play an important role. Otherwise there should be a clear negative effect at higher pCO2 in the dark treatments, but this is not the case... Therefore the statement "At a finer temporal resolution, the tubeworm recruits exhibited enhanced calcification of 40% during irradiation hours compared to dark hours, presumably due to the effect of photosynthetic and respiratory activities of the host alga on the carbonate system" postulated in the abstract line 17-20 does not hold and should be re-phrased.

The principal question and hypothesis that OA disadvantages calcifying epibionts to the benefit of non-calcifying ones should be properly discussed and properly addressed in

the conclusion. The conclusion states that their study does not suggest a shift from calcifying to non-calcifying as found in other studies. This conclusion is not supported by the authors own data as Spirobranchus adults and recruitment are clearly affected. For bryozoans the picture is not that clear, however, here recruitement has not been studied which might have given a further indication if a shift might take place due to divergent recruitment patters as the growth response is ambigious... again – here statements should be based on the findings and should also consider the methodological/analystical constraints of the study. The last sentence in the conclusions does not hold as this can not be shown with the analyses carried out (as outlined above) and should therefore be deleted.

Other comments:

chapter 2.2: The term acclimation is arbitrary – how do authors know that 7 days acclimation is sufficient and then did they allow for "acclimation" after transferring to high pCO2? Was the preparation - "cutting" of fucus thalli and 5-day staining - within this 7-day "acclimation". Then this sounds like stress, rather than acclimation... please clarify and do not use a terminology like "acclimation" when it is actually the time for sample preparation or preparation of the set up.

chapter 2.4: the carbonate chemistry should have been determined in the incubations for different epibionts and mixing tanks separately and not as a mixed "randomized" replicate. It is likely that changes during the 3-days take place in the seawater carbonate chemistry and this would have been interesting to document according to epibiont taxa studied. As 3 pCO2 levels are very different this might not be very important, but it would be good however, to demonstrate how variable the pCO2 levels were within the 3 "replicates" nevertheless.

chapter 4

p. 3749 line 21f – what means "marginally affected" ! Is that really so –> authors should critically consider their finding, that in E. pilosa the ambient is neither significantly dif-

ferent from either 1200 or 3150 μatm, while due to the fact, that due to a slightly more pronounced difference between the 1200 and the 3150 μatm there is a significance – the question would be justified if here the statistical result needs to be considered with caution?

It would have been interesting to test a "species effect" if data between species can be normalized accordingly as this addresses the main hypothesis.

The figures are well illustrating results, specifically by including the statistical results. Note, that in Figure 2B the significance level indicated by the asteriks can not be correct as the one with bigger difference has a lower significance level?

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