Review of Timmins-Schiffman "Evidence in Pacific oysters (*Crassostrea gigas*) of short term compensatory mechanisms to deal with decreased calcium carbonate availability in acidified conditions."

Timmins-Schiffman et al. present a laboratory based study of the response of C. gigas larvae to elevated CO_2 during the first few days following fertilization. The authors note that in the first day there is no difference in calcification rate, and that it is not until later days that differences emerge between CO_2 treatments. The study as designed, seems adequate to examine the role of CO_2 on larval C. gigas growth. My largest issues with the manuscript arise from a lack of depth in interpretation, careless use of terminology in the writing, and what the actual evidence of a compensatory mechanism is provided. Overall, I think the MS suffers from a lack of time to delve into some of the issues raised by the authors, and also lack of organization. The discussion seems to wander and a careful rewrite would help tremendously. I know the lead author is a graduate student, and my comments below are directed to help her revise the manuscript for improvement, rather than discourage the ongoing work. I will outline below general concerns/issues with limited examples, and provide some additional detailed comments below. Unfortunately, I do not believe the MS is publishable as written, and requires significant revision to address the issues throughout.

First, there are several instances of statements that are either poorly justified or suffer from terminology misuse. For example, line 68, the "highest concentrations of pCO_2…" are in fact in the deep waters of the world's oceans or along shelf habitats, not in near-shore surface waters. The near-shore is certainly enriched with CO_2 due to respiratory processes, but the very surface waters can equilibrate with the atmosphere, thus why the algorithms of Juranek, Feely, and Alin are not useful in the surface waters, and only currently work at depths of 40m or greater. I will highlight more examples of these sorts of issues, but the authors need to carefully revise the MS to address this problem throughout.

Line 73- The reference to Gazeau here is incorrect, that study illustrated that saturation state did not matter, rather it was carbonate ion concentration (as the authors correctly state later).

Line 78- CO_2 does not dissolve in seawater, it equilibrates in the gas phase, and quickly hydrolyzes to form carbonic acid.

Line 80- The shell buffering observations stem back several decades, and it would be worthwhile to acknowledge some of this earlier work.

Line 250- I think this comparison to Talmage and Gobler is oversimplified, as they followed development to a later stage, and found the greatest effects occurring at later stages. The no negative impact noted in the current study only followed larvae to 3 days. Then to note that different experimental manipulations may to blame is slightly ingenuous given all the studies note prior bubbled CO_2, Gazeau's study was the only one in which conditions were manipulated differently. I would also caution putting too much weight on the Gazeau study's conclusions, as the amount of Ca added in their study likely had other physiological impacts that were never really addressed. I am not debating it is suggestive, but I do not believe it is conclusive. And again, in Gazeau, larvae were followed for only a couple days.

Line 280- High Mg-Calcite can be more soluble than aragonite, again, please be careful with oversimplified statements, these sorts of errors in the OA literature continue to propagate ideas and concepts that are not entirely correct and muddy understanding of the problems.

Following this, the discussion about compensatory mechanisms is lacking a key reference to Widdicombe and Spicer that showed higher calcification rates came at the expense of tissue formation. So would this be a compensatory mechanism if the increased calcification does not result in increased growth or fitness later? Furthermore, the biologically controlled calcification in mollusks allows these organisms to calcify in conditions that are corrosive to primary minerals. The authors should explore some of the calcification literature present (that was published prior to the OA publishing blitz). Again, this section is oversimplifying a complex and studied process. A recent paper by Weiss highlights how controlled this process is in fact, and another by Ries proposes a physiochemical model that describes how proton pumping efficiency may be crucial to understand in this context.

It is clear however that some of these processes are less understood in the larval stages, but there is enough literature to help better explore this issue.

I would also encourage the authors to explore the several papers on C. gigas larval energetics, there are several possible explanations that would be very useful for the authors to consider in the context of their findings.

In the conclusions, it seems out of place to be acknowledging a possible maternal effect, this should be incorporated into the discussion. Furthermore the statement that a compensatory mechanism allowed oyster larvae to acclimatize is completely unfounded in the current study. In fact, I would say it is the opposite, as by 3 days the larvae were not growing well, so the compensatory mechanism only provided short-term relief, and did not allow larvae to overcome stress.

There are some methodological issues that should be addressed in revising the MS.

I understand several people use the polarized light methods to determine calcification in larval bivalves. The authors may want to acknowledge some of the very possible issues with this method, given it is subject to interpretational error by different microscope operators. Determining a “fully” calcified larvae is not always as straightforward as the cross is sometimes poorly defined and requires interpretation. It would be good to know if the authors took any measures to cross-validate those microscope calcification classification.

A second issue that may need to be addressed with the calcification data is whether it needs to be transformed, given it is proportional data. Often these types of data are arcsine squareroot transformed because they do not fit a normal distribution. I assume that maybe this is why a binomial distribution was used for the analyses. Some additional description in the methods may help to resolve this issue.

With regard to the CO_2 manipulations, more information is needed here as well. The methods for controlling CO_2 injection are not well described. As I can follow this, pH was used as the variable to adjust conditions to even with changes in alkalinity (albeit not large). This seems to have resulted in somewhat variable CO_3 concentrations, particularly between the mid and high CO_2 treatments. The authors argue that the carbonate ion concentration is important to the larvae, however, the difference in this concentration is marginal (~14 umol/kg) between the high and mid treatments, whereas the difference the mid and ambient treatment is nearly 2x. It seems like there may be some worthwhile discussion here that the authors can more fully develop.