

## ***Interactive comment on “Enhanced pH up-regulation enables the cold-water coral *Lophelia pertusa* to sustain growth in aragonite undersaturated conditions” by M. Wall et al.***

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This paper is very interesting and provides a potentially valuable contribution to the literature as to impacts of ocean acidification on cold-water coral biomineralisation. However, at present some of the findings are unsupported by the data/methods, and need to be clarified.

A lot of nice work has been done on the samples, but there is a lack of quantitative and rigorous data on which the conclusions are based. In general, I think there are two major areas that need revision. Firstly, more quantitative data is needed for all the analysis based on the images. Secondly, the discussion should acknowledge the

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fact that the control is not a true one, and discuss their findings with this in mind. The authors do find some very interesting results, and I genuinely think that these will advance the field, but a revised manuscript has to place these in the correct context.

Specific points: It is vital to include all the method background needed to interpret the results. Presently, it is hard to see how many samples were used in each condition and thus how much confidence we can have in results. This is crucial since the authors state how variable the samples are.

Since the controls for this study are natural samples (i.e. not lab grown), it is not feasible to state that differences (or lack of) are due solely to ocean acidification. For a true control you would need samples grown under ambient conditions in the laboratory, as changes in biomineralisation could be a lab effect (i.e. possibly due to feeding regimes). The results that organic matrix layers are less distinct in high CO<sub>2</sub> treatments are very interesting, but the discussion should be wider to consider that factors other than CO<sub>2</sub> could cause this.

Results: You state that the skeleton growth was variable (from how many samples and how were they compared?) and that there was no change in strength or structure. However, you did not measure strength, and the structure analysis is based solely on pictures. A table of measurements and some analysis would greatly strengthen this section.

You state “the organic matrix layers of the fibre growth layers between natural conditions and the high CO<sub>2</sub> individual”. This implies that these results and subsequent discussion are only based on 1 sample but in other sections there appear to be multiple samples? Please clarify this in the methods.

Discussion: You state “Tomographic analyses clearly showed that the morphology of *Lophelia* skeletons are highly variable and does not change under high CO<sub>2</sub> even in undersaturated waters, i.e. there is no morphological indication of a stress response.”, and “the template of size and shape of the corallite, does not change between treat-

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ments". These conclusions are not really supported by the data. You do not have a proper control for comparison, and the growth under elevated CO<sub>2</sub> is not in newly grown corallites, but short extensions and thickenings of existing corallite(s). Since the extension just seems to be a corallite tip, it is unclear what you are basing this interpretation on. How many corallite tips were compared and how did you compare them? Did you measure any sizes? Since all the growth you recorded were extensions of existing corallites, would it be influenced by the form it was already taking (i.e. continuing to grow through established EMZs?). Provision of quantitative measurements of key corallite characteristics in a table, along with more clearly defined methods would greatly strengthen this section and the paper as a whole.

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