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Interactive comment on "Acidification counteracts negative effects of warming on diatom silicification" *by* Alexandra Coello-Camba and Susana Agustí

Anonymous Referee #3

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General comments: This manuscript presents data on the effects of temperature and CO2 on cell size, valve thickness, sinking rate and silica incorporation rate of in situ diatom communities. It's interesting to see that increased CO2 mitigates the negative effects of warming on silicification. However, I found several serious problems in the study: 1) my main concern is the replicates in the experiment, no detailed information can be found in the manuscript. From figure 1, there is only one data point for one temperature treatment. 2) The carbonate system parameters are missing to further constrain carbonate chemistry. 3) I think it's not proper to classify species according to cell size. Cell size can vary a lot even for the same species. The dominate species information should be provided. 4) Why the silica incorporation rate normalized to

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volume rather than biomass? If the biomass in different treatments were distinct, the rates can say nothing.

Specific comments: Page 1 line 1: I don't think this title is appropriate for this paper. In two of three experiments, the authors only focus on the effects of temperature. Moreover, the authors discuss a lot on effects of temperature, rather than interactions of OA and temperature.

Page 2 line 20: "stressors"? Increased CO2 mitigates the negative effects of increased temperature. So can you call CO2 "stressor"?

Page 2 line 26: The information of dominate species in these communities should be added.

Page 2 line 31: Two pCO2 treatments? In Figure 2, you showed three pCO2 levels. Moreover, the pCO2 values are self-contradictory in method and results parts.

Page 2 line 35: How many replicates in the experiment? In fig. 1, only one data point for per treatment. Does this mean that there is only one bottle for per treatment?

Page 3 line 6: Were the bottles aerated throughout the experiment or stopped when target pH was achieved?

Page 3 line 8: The light tubes on the top or side of bottles? Did the author measure light in bottles?

Page 3 line 9: The carbonate system parameters are missing to further constrain carbonate chemistry.

Page 3 line 12: The information of filtration pressure should be added.

Page 3 line 14-21: It's better to add some references for this method.

Page 3 line 27: How many samples measured for one treatment? Again, how many replicates for per treatment?

Page 3 line 38: When did the author measure the rate of incorporation of silica? At the end of experiments? Samples were incubated under light or darkness?

Page 4 line 1: The information of filtration pressure should be added.

Page 4 line 22: From my perspective, this model is useless for the discussion. You can analyze the interaction of these two factors from fig. 3A and C.

Page 5 line 9: Median values of density of the cytoplasm and cell wall density were used for calculation the sinking rate. However, I think these parameters may be species-specific and influenced by treatment, such as temperature.

Page 5 line 23: More detailed data analysis information should be provided.

Page 5 line 29: These values were mean of each pCO2 treatment? Please add the standard deviation. In the method, you said there were two pCO2 levels.

Page 6 line 2: Can you tell whether the test cells belonged to one species or one genus according to their valves?

Page 6 line 3-5: I think it's not proper to classify species according to cell size. Cell size can vary a lot even for the same species. The dominate species information should be provided.

Page 7 line18-21: These sentences are repetition of the method section.

Page 9 line 8: Cautions should be taken to draw this conclusion: you only test the interaction of pCO2 and temperature for the third experiment. What will happen for the second one? The in situ temperature for the second experiment is 6.2 âĎČ. Will the increased pCO2 counteracts negative effects of warming when temperature increases by 4 âĎČ or more for diatoms in these waters? Base on the third experiment (at 10.3 âĎČ, increased pCO2 acted synergistically with temperature), the answer may be "no". The author should add some discussion about this.

Page 9 line 12: I suggest to change "stressor" to "factor".

Page 16, table 3: Can the microscopic method test the minimal variation of valve thickness ($\sim$ 7 nm for temperature increasing 10 âĎČ)?

Page 17 figure 1: Why only one data point for one temperature treatment? How many replicates in the experiment?

Page 17 figure 2: For panel A, what's the pCO2 treatment for every temperature column? Mean value of three pCO2 treatments. Same for panel B, what's the temperature treatment for every pCO2 column? Why the rate normalized to volume rather than biomass? If the biomass in different treatments were distinct, the rates can say nothing.

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