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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 #1

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Coello-Camba and Agusti describe the results of three experiments, testing the effect of temperature and pCO2 on diatom cell volume and valve thickness. I do not believe that they have tested the actual effect on diatom silicification as stated in the title for reasons that I explain further down in this review. The topic of how climate change related environmental drivers affect phytoplankton physiology and thereby possibly their ability to protect themselves against grazers is a very important one. However, I feel that this manuscript lacks important information, especially in the methods section and I am not convinced that the experimental set up and some of the methods used and especially the amount of data analysed are appropriate to allow the authors to draw the conclusions they did. I therefore do not recommend publication of this manuscript

unless the authors can clarify all the issues addressed in detail below.

Methods: Page 2, Line 32: You state that in the 2010 experiment you had two pCO2 treatments at 380 and 1000 ppm. However, in the figure 2 there are three pCO2 treatments at 217.7 (which would be pre-industrial), 780.0 and 1652 ppm which would both be future scenarios. In figure 3 you only show 780.8 and 1652 ppm. Which one is true? If you did not have and ambient control treatment at 380 ppm please explain why.

Page 2, Line 35: How many 20l bottles were incubated? Only one per treatment without replication?

Page 2, Line 38-Page 3 Line 5: Were the bottles constantly bubbled during incubation or was the target pCO2 just adjusted in the beginning? If yes, it will have changed during incubation, was pH monitored?

Page 3, line 8: 200 μmol photons constant light seems very high. Can you show environmental data to show that these are average light intensities phytoplankton is exposed to in Arctic summer considering the deep mixing?

Page 3 line 10 following: My major concern about the cell size and valve thickness measurements is that I don't know how you can be sure that you measured individual frustules from the same species. Following the cleaning procedure, the cells will be empty and broken. Were you able to identify the species or at least genus? If yes please report them. If not how can you be sure that you did not measure cells from different species in each treatment? Also how many cells did you measure from each treatment? In figure 1 it seems like you only took one measurement per treatment as there are no error bars. If this is true, I do not believe that your data show any temperature trend but simply the natural variability in cell size that you find in any diatom species. In line 27 and 28 you state that you determined cell volume from the closest geometric figure. Did you measure the frustule height for each measured cell or did you just use the same estimated height for all measurements throughout

all treatments? If you do not have the exact cell height for each cell you measured your cell volume estimations will be very inaccurate. Cell height in diatoms is much more variable than frustule diameter even within the same species. At least you would need to know which species you have measured in each treatment and use an average literature value for its cell height.

Page 3 line 30 following: If I understand this correctly you used the PDMPO uptake over 24 hours I sub-incubations to determine the silicification in your treatments. But you do not describe any biomass measurements to relate to PDMPO uptake, do you just assume that growth rates and grazing were exactly the same in all treatments? Total PDMPO uptake does not say anything about silicification if you don't know the diatom biomass. The lower Si uptake at high temperatures could also mean that less diatoms grew in total, or more of less silicified diatom species but it does not necessarily mean that each diatom species changed its silicification.

Page 4, Line 23 following: I don't really understand why you are using this model if the assumption that temperature and pCO2 have additive effects is clearly wrong and does not fit your data. I don't see how this is helpful.

Page 5, Line 1 following: The estimation of sinking rates does only apply for dead cells as you assume constant cytoplasm density for all species and throughout all treatments. This should be made clear here and later in the results and discussion.

Page 5, Line 18-20: This should go in the results section

Page 5, Line 27,28: I hope the pH values did not vary between 8.2 and 7.5 which would be massive but that these are indeed the pH values for the three individual pCO2 treatments. Please state how and when pH was measured and if these are the start values or average over the course of the incubation.

Tables 1 and 2: I think these two tables should be combined. Also in table 1 you state that the temperatures for the first two experiments were 1.5 – 10.5 and from 1 – 10 in

experiment 3 but in table 2 temperature goes from 1.6 to 10.5 and from 1.2 to 10 in the first two experiments and from 1.8 to 10.3 in exp. 3. Also pCO2 in table 1 is once again described as 380 and 1000ppm. Please make sure you report the correct values throughout.

Table 3: I cannot believe that all the cells you measured had the exact same size (e.g. 21.4 μm in exp.1). There is always a natural variance in cell size in every species and in this study it is absolutely crucial to know what the natural variability is in order to be able to estimate changes in cell size caused by the different treatments. So please report the number of individual cells you measured in each treatment and the actual cell size of each.

Figure 1: From this figure I am not convinced that you were able to perform the measurements of valve thickness and cell diameter with the precision that you report. In table 3 you have calculated a change in valve thickness of less than 1 nM per °C. As the temperature range you have tested is 10°C this would mean that you have actually measured a 10 nM difference in valve thickness between these two treatments using light microscopy! I find this hard to believe.

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