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Interactive comment on "Resilience to temperature and pH changes in a future climate change scenario in six strains of the polar diatom *Fragilariopsis cylindrus*" *by* M. Pančić et al.

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This paper measures the growth rates of six identical strains of Fragilariopsis cylindrus under a series of pH and temperature manipulations. The authors found a significant interaction between temperature and pH, where the lowest pH at the lowest temperatures expressed the lowest growth rates for all six strains. While there were differences in the absolute growth rates of all six strains, the response to the pH and temperature manipulations were the same, with all strains showing increased growth rates with increasing temperature. This study shows that the general response of F. cylindrus to the environmental manipulation was the same across all strains (increased growth

rates with increased temperature and lower growth rates at lowest pH). The variation in actual growth rates between strains demonstrates phenotypic plasticity within the species.

While the methodology and experimentation undertaken in this paper appear sound, to make the paper suitable for a high impact journal, these data need much more exploration to highlight the potential impact of the study. In its current form there is limited novelty in the study and I feel that it is better suited to a different journal. It is also my recommendation that the authors consider re-writing the study as a note paper, based on the fact that there is only one variable being measured, growth rates.

Detailed specific comments:

1. The title is not quite appropriate for the content. It should reflect more the phenotypic plasticity and intra-specific variability rather than resilience. Also, there were only 3 strains used across temperature manipulations not 6.

2. The authors have analysed their data using a two factor ANOVA, yet present their data both in the text and Figure 2 as 3-factorial; temperature, pH and strain. Three-way ANOVA would be more appropriate, as testing the significance of differences between the strains is fundamental to the hypothesis and therefore equally as important than the environmental factors (pH and temperature) being tested. Statistics need to reflect the hypothesis being tested, the data presentation and the discussion.

3. Data on strains D10A12, D4D11 and D3G1 is doubly represented in Fig 1 and 2, where the exact same data is re-plotted separated by temperature instead of strain and with the 3 additional strains added at 5 degrees only. One of the figures should be removed.

4. Also, I agree with reviewer Dr Ajani, that there is no rationale provided as to why the authors only analysed a subset of the 6 strains for 1 and 8 degrees. This needs to be clarified.

5. The authors have put lines connecting the growth rates across the pH measurements. This is misleading and incorrect, as the data is categorical. The lines need to be removed. The data would be better replotted as bar graphs, which I feel would be more fitting, similar to the data in Fig 2 of the supplementary. In general, I find the data presentation could be improved to assist the reader in interpreting the findings.

6. The supplementary figures more clearly and accurately represent the data. They also include the statistical significance information, which is missing from the main figures. However, again, the same data is plotted 4 times in the supplementary and also a table provided with the same values. This should be reduced.

7. Figure 3 is not data and is therefore non-essential. I recommend making it supplementary or removing it altogether.

8. With the removal of 2 figures, there is now only one figure and two small tables, therefore, I strongly encourage the authors to consider making this study a note paper, as I don't feel that there is sufficient significant data to warrant an entire discussion paper.

9. The authors discuss the data in both combined effects and individual effects. However, they detected a significant interaction, which means that the data only need be discussed in terms of the combined effect. This may of course change once a 3rd factor (strain) is included.

10. The results section needs refining and could be reduced substantially. In general, figures should be sufficient to describe the patterns to the reader. Therefore, the text should report on the statistically significant aspects of the data and main trends.

11. Please be specific in your subheadings. At 4.1.1 please change 'multiple' to 3 F. cylindrus strains.

12. Take care when using the term alkalinity instead of higher pH, as they aren't the same thing.

13. The authors should try to avoid ambiguous terms such as 'greatest impact' or 'slightly'.

14. The section in which the authors mention the natural pH range of Arctic phytoplankton (7.5-8.3) should be disclosed much earlier on, as it shows the inherent plasticity the strains would be expected to have, due to the seasonal or diel fluctuations that occur in their habitat. This provides a framework in which to discuss the results, particularly with respect to the minimal effect on pH changes from 7.4-8.0 in their experiments.

15. With respect to the data presentation; the growth data could be shown in a way as to be more informative when interpreting the data in terms of phenotypic plasticity, for example it could be a figure that shows the relative change in growth rates across strains, pH and temperature, or it could simply plot the range in growth rates (0.2-0.7 d-1) across the six strains. Alternatively, the authors could consider using the supplementary figure 2 (temperature and pH in 3 strains) and the supplementary table (with all the strains at 5 degrees) to represent their data set in the main manuscript. The only supplementary figures would then be Sup Fig 1, table on carbonate chemistry and the Figure 3 from the main paper.

Best regards,

Katherina Petrou

Interactive comment on Biogeosciences Discuss., 12, 4627, 2015.