The present paper adds to the growing body of literature that explores the effects on changing environmental drivers on marine phytoplankton.

I appreciate the effort that was taken in the experimental design, particularly the prolonged acclimation phases for each treatment and the gradient design to retrieve functional response curves to each driver. Obviously, the experimental design was flawless and all analysis have been conducted appropriately. Generally, I also appreciate the usefulness of culture experiments, despite their limitations, as I think a lot can be learned about physiological mechanisms that are relevant for the "bigger picture".

However, regarding the present paper, I am not quite sure what the main conclusions and the "new lessons learned" actually are, and what the paper should be highly cited for.
Certainly, the study provides some nice physiological information on two species, which might certainly be of some use e.g. to compare to other culture experiments.
The main message seems to be that the diatom *Pseudo-Nitzschia subcurvata* might be favored under global warming relative to *Phaeocystis antarctica*, due to its higher optimum temperature and thermal tolerance. However, the fact that different species and / or functional groups have different thermal response curves is not new. It is quite obvious that this might lead to some degree of reorganization of community structure or spatial shifts of species distributions. However, I don't see such new insights from the present study. I would have loved to read about possible physiological mechanisms behind the observed responses, for instance, the interactive effects of temperature and CO2 on *P. subcurvata*. There are several recent studies that discussed responses to multiple stressors in much more detail (e.g. Brennan & Collins 2015).
Furthermore, the authors state that CO2 effects on elemental composition were negligible, and mention several other studies that observed differential effects. However, no possible explanations for these contrasting results, e.g. based on physiology or differences in experimental setup, is provided.
One critical aspect that I'm missing in particular is the role of nutrient status. Responses to temperature and CO2 have been tested in semi-continuous cultures with permanent nutrient replete conditions in this study. The authors state that CO2 effects were negligible, which is indeed in agreement with earlier studies that were conducted under nutrient-saturated conditions.
However, a number of recent studies have demonstrated, that physiological responses to CO2, as well as temperature, tend to be much stronger under nutrient-limited conditions (e.g. *Sala et al., 2015*) or in the transition from exponential to stationary growth (e.g. *Taucher et al., 2015*). This is particularly true for elemental ratios, which are also prominently discussed in the present paper.
Therefore, I wonder how relevant and representative the results of the present study might be? What do the authors think, how might the response have looked like under more realistic nutrient conditions, e.g. a transition to nutrient depletion? Furthermore, how relevant is *P. subcurvata* in the study region at all(in terms of biomass)? And what about other important diatom species in the

study region? Without discussing such aspects, I find it hard to justify larger-scale extrapolations to phytoplankton community structure or even biogeochemical cycles, as done by the authors.
In fact, the tendency to extrapolate the findings from the culture experiments to large-scale biogeochemical cycling (e.g. export flux) seems rather far-fetched. What about possible food-web effects resulting from a transition from *Phaeocystis* to diatoms? Particularly with regard to predictions on future export, it seems odd that the discussion goes straight from physiological responses (under artificial constant exponential growth conditions) to predictions on future export, without mentioning possible shifts in food web structure. For instance, how might the grazer community respond to a shift from *Phaeocystis* to diatoms? And how might this in turn influence export patterns?

Besides, I generally agree with the other reviewers that the writing style of the paper is rather tiresome. The results section reads very generic and large parts of the discussion are somewhat repetitive as they just state the same as already said in the results. I think a more focused and in-depth discussion combined with a more appealing writing style would make the paper a lot better.

Some further comments:

- The 8°C treatment is a rather unrealistic scenario. Of course, it is desirable to observe an effect in such experiments, but I wonder about the environmental relevance of this treatment, as such temperature cannot be expected for the near future.
- Q10 values for growth of 2.11 and 3.17 seem rather high. Usually, values of 1-2 have been reported for autotrophic processes. It might we worthwhile to embed the presented findings with earlier studies on temperature responses.
- Elemental ratios at different temperatures might be difficult to interpret, as the cultures experienced differences in length of growth period (i.e. number of cell divisions) and nutrient uptake, with differences in left-over nutrients at the end of the incubations. Thus, they might not be in the same physiological state
- Competition: Why does P. subcurvata outcompete Phaeocystis at 0°C ? According to the thermal response curves, Phaecystis should have a higher growth rate. Was there any difference in experimental conditions compared to the thermal response experiments?
- Fig 5: It would be helpful if the scale of the y-axis would be identical in all panels.

References:

- *Brennan & Collins 2015: Growth responses of a green alga to multiple environmental drivers*
- *Sala et al., 2015: Contrasting effects of ocean acidification on the microbial food web under different trophic conditions*
- *Taucher et al., 2015: Combined effects of CO2 and temperature on carbon uptake and partitioning by the marine diatoms Thalassiosira weissflogii and Dactyliosolen fragilissimus*