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Biogeosciences
Discussions

Interactive comment on "Responses of the diatom *Asterionellopsis glacialis* to increasing sea water CO_2 concentrations and the effect of turbulence" *by* Francesca Gallo et al.

Anonymous Referee #2

Received and published: 5 December 2016

This manuscript addresses an interesting and important question- 'how do synergistic changes in pCO2 and turbulence impact diatom growth?'. While this is an important question that deserves enhanced attention in the literature, I have some concerns about the execution of these experiments and their interpretation, as described below:

1) How frequently were the cultures diluted and by what factor? How was this dilution rate selected (which measurements?), and when were the growth rates deemed to be in steady state? For instance, one criteria for determining this is to assume growth rates to be in steady state when they did not change by more than 10% between dilutions for some set number of generations or to use a statistical test to determine that the growth rates are not changing over some set period of semicontinuous manipu-

lation. See, for example, Fu et al 2007 J. Phycol 10.1111/j.1529-8817.2007.00355.x This is incompletely described in this manuscript and thus leaves the reader with some difficulty interpreting the results. Other important experimental details are also missing, such as growth temperature.

2) There is not enough detail provided to determine whether the carbonate system manipulations were effective and properly controlled. The bicarbonate and strong aid manipulations are only appropriate in closed system settings, eg: www.biogeosciences.net/6/2121/2009/ and it is not clear that this was maintained. In fact, the data in Table 1 suggest it was not.

3) It's also not clear whether the system was monitored frequently enough. It seems that these measurements were made simply at the beginning and "end" of the experiment. It's not only not clear how this is defined- how many generations did it take the semi-continuous cultures to reach steady state growth rates (end?)?, suggesting that these parameters "during" the experiment were simply and average of beginning and end values in not defensible (Table 1).

4) Cell size is discussed in the text as a way to understand when turbulence might be favourable or unfavourable, but not data is reported on cell size. From the cell quotas, we can make inferences, but measuring cell size would allow the authors to more specifically address their own question and test the models and mechanisms for turbulence impacts that they describe here (line 104). Hopefully the authors have retained images from their culture monitoring that could be used to address this. It would strengthen the interpretation involving chain length changes as well.

5) Table 1: In addition to the problems identified above, the authors should also indicate measured vs calculated values more clearly here.

6) Figure 1: the fits of these lines are not good, and not described. This should be excluded or explained and justified in much more detail.

7) I agree with anon. reviewer 1 that the turbulence imposed here needs to be put in much better context in order to justify extending the results to expectations in a future stormier ocean.

8) There are many spelling and grammar mistakes in the manuscript. If it is to move forward, this should be much more carefully addressed by the authors. In particular, the Abstract has many problematic sentences.

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