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Interactive comment on "Modulation of ecdysal cyst and toxin dynamics of two *Alexandrium* (Dinophyceae) species under small-scale turbulence" *by* L. Bolli et al.

Anonymous Referee #1

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General comments

This is short paper detailing how growth rate, cyst production and toxin content in two species of dinoflagellates respond to growth in turbulence. While I applaud the attempt at brevity in the paper, there are sections of the paper that need serious expansion and contain critical omissions. I also found the data analysis somewhat subjective and incomplete.

Overall, the results are not unique or groundbreaking, as these types of turbulence effects in dinoflagellates (changes in growth rate, cyst formation, toxin content, cell size, swimming behavior, etc.) have been previously documented for these species and numerous others. However, some of the results do represent new information on species specific responses to turbulence and could be useful to the community.

In general, papers detailing laboratory based turbulence experiments on dinoflagellates are becoming fairly common in the literature, however a continuing issue within these papers is that most never critically address whether their experimental design is realistic or how their results might transition and/or be ecological meaningful in the "real world". For example, these authors examined the responses of dinoflagellates at two turbulence intensities, a "low turbulence" of 0.4 cm^2 s^{-3} and a "high turbulence" of 27 cm^2 s^{-3}. This is somewhat misleading to readers not familiar with oceanic turbulence as the authors "low turbulence" value is actually very high for natural ecosystems and the "high turbulence" value is extremely high (rarely measured) and might only be expected to occur directly under a large breaking wave, dissipating within seconds. The authors are aware of this and attempt to diffuse the use of non-realistic turbulence treatments in the experimental design by saying: "Although our experimental values of ϵ; are very high, both in intensity and persistence, these conditions may help to ascertain the underlying mechanisms of cell adaptations". I believe this statement is open to debate and should be critically questioned by reviewers, readers and editors, as results obtained by exposure to extremely high, continuous turbulence levels for many days (4 to 15 days in this case), may have little or no ecological significance. I challenge the authors to document an example of a turbulence dissipation rate of 27 cm^2 s^{-3} routinely persisting in normal ecosystems for more than a few minutes in the same spatial volume of the ocean. The obvious danger in results that can only be produced by many days of exposure to unrealistically high turbulence levels is that the conclusions drawn from the results may simply represent laboratory artifacts that never occur in nature. Readers should be made acutely aware of this caveat, no matter how it reflects on the research. The authors should include further discussions on the true nature of turbulence in the ocean and how their results can be interpreted

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in that context.

If we examine the overall results of the authors further, we find that under their "low turbulence" treatments (i.e. real world high turbulence), both species of dinoflagellates actually grew **faster** than the non-turbulent controls with **no effect** on cyst production or toxin content (a very interesting result in its own right, remember this for future comments below). In their "high turbulence" treatments (i.e. unrealistically high turbulence), after **4 days** of continuous exposure they found **no significant change** in growth rate or toxin content in either species (they did find a change in cyst production). **Only** in their much longer "high turbulence" treatments (> 10 days continuous exposure) did the authors find a significant change in toxin content and growth rate. However, it is these experiments that the authors chose to examine in detail (e.g. Figures 1a, 2a, 2b, 2c). This is an example of the subjective nature of the author's data analysis. The authors seem to have written this paper in the context of the older paradigm that "turbulence is detrimental to the growth of dinoflagellates" and have chosen to only highlight the results that showed negative effects of turbulence (i.e. lower growth rates), while essentially downplaying the results obtained at more realistic turbulence levels that show the opposite trend. While it is the authors choice to interpret their data to emphasize their point of view (and the editors ultimate decision on the appropriateness of this for any Journal), I would hope that the authors would recognize this apparent subjective bias and give more time and analysis to equally important results, even if the results are contradictory to their dogma. Examples of this subjective analysis are outlined in more detail in parts of the specific comments section below.

Specific comments

Abstract

Page 894, line 4: The authors report their epsilon values in cm^2 s^{-3}. Why not use the more common units of m^2 s^{-3} and/or its non SI equivalent Watts kg^{-1}? Most physical oceanographers and current literature reporting epsilon values use m^2 s^{-3}. At a minimum, you might provide the equivalent m^2 s^{-3} value.

Materials and Methods

Page 896, lines 14 - 25: Why did you use two different methods to generate turbulence, i.e. the oscillating grid for generating “low turbulence” and the orbital shaker for generating “high turbulence”? This is not explained or justified anywhere in the manuscript. Why didn’t you just use one method at different speeds to generate the needed turbulence levels? You state in your introduction that comparing other researcher’s experiments that use different turbulence generation methods is not possible (Page 895, line 5: "direct comparison among studies in not possible”), yet you have done this very thing without explanation. This needs to be fully explained as it appears to make the experiments unnecessarily complex and open to interpretation.

Page 896, lines 16-19: You need to provide much more detail about your methods here. You provide a formula for calculating epsilon in your orbital shaker flasks and only justify this formula with an unreferenced statement saying: “This equation was derived from data acquired with acoustic Doppler velocimetry technology”. This is too simplistic and inadequate as a methods description. How was this formula derived? What is the uncertainty of the estimate? How did you take the velocity measurements in the flasks? What type of ADV did you use? How was it set up? If you can record actual velocity spectra in your flasks with a velocimeter, why are you using this formula? Why not show us the actual velocimeter data and the spectral calculations instead of this formula? Your paper is about the effects of quantified turbulence (i.e. epsilon) on dinoflagellate physiology, yet you provide no real justification, reference or

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supporting data for the methods you used to quantify turbulence in your apparatus. Other researchers can't critically evaluate (or duplicate) your methods of turbulence quantification, as you have provided no documentation. Please expand this section.

Results

Page 898, line 14 (and other places): You use the term "final biomass yield" here and a few other places in the manuscript. This is not the correct phrase to use. You actually mean "final cell concentrations" or "final cell numbers". Biomass yield is a misleading term as it obviously changes, for instance, as cell size changes. Simple example: if culture A had one half the final cell concentration of culture B, but culture A was twice the size of culture B, the two cultures could have equivalent "biomass". As you did not measure cell size or any other actual biomass index (e.g. carbon) of your culture treatments, you should change "final biomass yield" to "final cell concentration" or "cell numbers".

Page 898, lines 21: Why is this data "not shown"? It needs to be included. Other researchers should be able to see the reliability and standard errors in all of the data and analysis that you report.

Page 898, lines 22-24: It appears that you are selectively reporting your data in this section. Why is there no mention of the significance of the growth rate results vs. the controls for the high turbulence "exponential" treatment of A. minutum? You should not subjectively report your results. Please include the tests of significance and results for this (and all) treatments.

Page 899, lines 1-3: Again, it appears that you are selectively reporting data. What about the percentage change in the final “biomass” of the “exponential” treatments? Once again you are ignoring these results and reporting only on the “always” treatments. This is not acceptable; please include these data in the results text as well.

Page 899, lines 4-11: This section has your strongest results and is probably one of the most important aspects of you paper. Again though, you end the section with a data “not shown”. All of this data should be shown so the reader can critically examine it. You really need to provide a second data table that statistically summarizes all of your cyst and toxin results for every treatment (similar to how Table 1 summarizes the growth rate data).

Page 899, lines 12-18: This is the third instance that you cite data that is “not shown”. See my comment above about including a second data table to correct this issue.

Table 1:

a) You show 3 decimal places for your growth rate estimates. Do you believe you have quantified growth rates that are reliable to 3 significant figures? I’m guessing this is not the case. You should change these growth rate estimates to the correct number of significant figures (i.e. 2).

b) You report all these table results in terms of “orbital shaker” or “grids” with no mention of the turbulence value actually created by each of these methods. You need to report the results in terms of the turbulence value and add the generation method in parentheses or in the table header if desired.

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Page 900, lines 2-3: You found that dinoflagellates grow **faster** in what would be high levels of natural turbulence. This is an incredibly important result, yet you do not even speculate as to the ecological significance of this finding. Further, this result was obtained in the more realistic of your two turbulence treatments. This result suggests that at more natural levels of turbulence, dinoflagellate populations may actually grow faster than those in quiescent waters. Thus, even high levels of natural turbulence may in fact promote the growth of harmful algal blooms and be a critical **enhancer** of natural population growth. Your data clearly suggests this, but you do not discuss it or devote a figure to it. In fact, you pretty much spend the rest of this paragraph speculating about how "high biomass" results (i.e. increased growth rates) could be artifacts. For example, you state "This last observation suggested that turbulence facilitated gas exchange in the experimental vessels where high biomass developed" and "These differences in population development could be related to differences in shape or light wavelength transmission of the experimental vessels". In contrast, somewhat disappointingly, in the only figures presented in the paper, you decide to show examples of growth rate being **suppressed** by unrealistically high levels of turbulence exposure. I personally find this presentation style to be biased and potentially misleading to those not expert in the discipline. You need to reexamine all aspects of the data in this paper and give it equal weight in the results and discussion. This will be a very nice, informative paper after revision. Do not be afraid to take your data analysis wherever it might lead, even if it challenges your preconceptions.

Technical corrections

Page 894, line 24: You should probably omit "somehow".

Page 900, line 2: “shaken” should be changed to “stirred” or “mixed”.

Interactive comment on Biogeosciences Discuss., 4, 893, 2007.