Biogeosciences Discuss., 5, S900–S903, 2008
www.biogeosciences-discuss.net/5/S900/2008/

Biogeosciences Discussions

Interactive comment on "Sub meso scale phytoplankton distribution in the north east Atlantic surface waters determined with an automated flow cytometer" *by* M. Thyssen et al.

Anonymous Referee #3

Received and published: 24 June 2008

General comments:

The authors present an excellent dataset, providing one of the few currently available for pico- and nanoplankton at this high resolution. They analyse this in an attempt to explain the large amount of variability witnessed within it. At larger scales they relate variability to hydrographic properties. At smaller scales they see the variability as predominantly temporal in origin. My main concerns relate to the interpretation of the data at these two scales. My first concern with the manuscript is that the analysis appears to make a bold assertion regarding the source of small scale variability (with the consequence that the title and last but one sentence of the paper may be misleading). Most

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

people would take 'sub-mesoscale' to refer to physical length-scales, and the excellent dataset presented here is of a high enough spatial resolution to do this. However, the only analysis carried out on the small scale variability focuses on interpreting it as temporal variability. If the variability is predominantly temporal (and synchronised across regions as the correlation analysis implicitly assumes) then the implication is that there is little spatial variability at sub-mesoscales. However, it is difficult to see how such significant fluctuations in abundance could be explained by diel cycles of reproduction as each reproduction can only change the abundance by a factor of 2 at most. I suspect that both diel cycles in physiology and submesoscale spatial variability are present and that the authors face the difficult task of separating the two. This is exacerbated by the fact that I have concerns about the technique used to infer periods of temporal variability (please see below). My second concern is that the analysis at larger scales is purely qualitative: no attempt is made to establish the strength of the relationships between hydrography, nutrients and organisms abundance/properties quantitatively in the 5 sub-regions, even using correlation analysis. Furthermore, I have a concern regarding comparisons to mixed layer depth (please see below).

Specific comments:

1. I have reservations about the technique used to infer correlation times. First, Fig.8 should also show the data so that it can be seen how well the 'low span loess' fits the data. Second, more information should be given regarding the fitted polynomial - what order is used? How sensitive are estimates to the choice of order? Third, it is vital that estimates of the correlation scales are reported with associated errors as they may be large and have obviously major consequences for the interpretation of correlation scales. These are currently not calculated. One method of doing so would be via a bootstrap approach: choose 90% of the data at random (allowing multiple sampling of the same data point) and calculate the correlation length; repeat this several thousand times to build up a distribution of estimates for the correlation scale from which both mean and errors can be calculated.

BGD

5, S900–S903, 2008

2. The authors use a model to estimate mixed layer depth. Interpretation of this in conjunction with the observations clearly requires great care, particularly at the scales that the authors are addressing. Do they have any evidence that the model matches the provinces they define on the basis of their observations? What are the errors associated with the model predictions? How rapidly do the spatial distributions of mixed layer depth change - it can be as rapidly as a day at the sub-mesoscale.

3. What checks were done to ensure that the process of pumping water onto the boat did not damage the cells. On a related note, how far was the intake for the pump below the bottom of the vessel? Given the movement of sailing ships would the intake have always been in the water?

4. How did the authors avoid the temperature of the water changing from intake to analysis?

5. Was the conductivity cell pre or post cruise calibrated?

6. What relationship between the cycles for abundance, fluorescence and scatter would be expected on physiological grounds e.g. presumably the abundance cycle should lag the scatter cycle as the cell increases in size before splitting? Are these relationships seen in the data? If not, why not?

7. How did the definition of the clusters take into account the diel variability? Presumably the cluster 'boundaries' had to be moved?

8 p2483, lines 7-8 from bottom: the authors have presented no evidence for the influence of submesoscale physical processes. Mixed layer deepening is only a submesoscale process if such changes in depth are consistent over a scale of 1-10km, and there would still remain the question of what is causing the deepening.

9. p2488, line 10 from bottom: it is currently pure conjecture to state that the observations are 'reminiscent of some isolated eddy interior'. What characteristics would you expect from such an environment, and what evidence is there for such characteristics

BGD

5, S900–S903, 2008

here?

Technical corrections:

I have a number of technical corrections. However, it would seem sensible to only communicate these once the more significant issues with the manuscript have been addressed.

Interactive comment on Biogeosciences Discuss., 5, 2471, 2008.