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Biogeosciences Discussions

Interactive comment on "Dynamics of microphytoplankton abundance and diversity in NW Mediterranean Sea during late summer condition (DYNAPROC 2 cruise; September–October 2004)" *by* S. Lasternas et al.

Anonymous Referee #3

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General comments

The manuscript (MS) contains an impressive taxonomic work. In my opinion the intrinsic value of this effort and information might be suitable for publication. However, after reading several times the MS, my impression is that the data treatment and discussion of the results does not correspond with the big effort undertaken to obtain the taxonomic information. Furthermore, the title and abstract generate high expectation in the reader interested in global change, biogeochemical cycles and trophic interactions, but results and discussion derives in a charged taxonomic description and lacks

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in physical-biological coupling. This might be due, to the fact that the authors did not found the expected change from stratification to mixing conditions they were looking for to test the IDH hypothesis. However, if the test of the IDH hypothesis was the aim of the research carried out, it is surprising that the authors mention difficulties to determine and measure disturbance. I think that a stratification index, turbulence measurements or even nutrient input might serve as estimation of a disturbance that affect plankton composition.

The authors mention that the DYNAPROC2 is a multidisciplinary program and that the results could easily be analysed in the view of physical-chemical and biological parameters (Page, 5185, line 13). But no physical and chemical data were shown in the MS. Without figure 1 and 2 of the Discussion Paper (Raybaud et al. 2008, same issue), the reader is virtually lost in terms of spatial sampling and hydrological data. There is no need to repeat the same figures, the indication of the depth of the thermocline (bold line) and the date of wind events and intrusion of less saline water (arrows) in figures 1, 2, 3 and 4 would help the reader to go through the MS. This also would give more support to the statements of the vertical position of dinoflagellates and diatoms in respect to the thermocline. Concerning the association of Scrippsiella with low salinity a TS diagram could be included to show the association of different species to distinct water masses. The authors mention taxonomic singularities and link them to global changes, but for these statements long-time observations are necessary. However, as the authors indicate taxonomic approaches are rear and without the implementation of regular monitor programs the detection of a rear species at a certain date does not mean that the species just arrived because of global warming, but that finally the taxonomist encountered the species.

Most of the results and conclusion are not new and trivial. Of course, as diatoms, also the presence of silicioflagellates influence the carbon and silicate cycle, and the presence of nitrogen fixing organisms sustain primary production. But something more should be said about the amount of carbon, silicate or nitrogen in the species and, more

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important, about the canalisation of these compounds through the pelagic ecosystem. For biogeochemical cycles it does not matter the name of the particle(cell) but the composition and the possible faith of the particle.

Specific comments

The title, mention microphytoplankton, and the reader get the impression that a two month sampling was carried out. However, the real sampling includes autotrophic and heterotrophic organisms and the two sampling methods (hydrographic bottles and net sampling) do not coincide with the recognised limits of microplankton (20-200 micrometer). In the first method the authors analyse also part of nanoplankton, while the net sampling only is representative for plankton bigger than 53 mm The sampling does not correspond to a nearly daily sampling from October-September as inferred in the abstract, but to four 5-days sampling intervals in one month (between 17 of September to 17 of October). This should be clarified from the very beginning.

Following the structure of the sampling strategy, the authors refer always to Leg 1 and Leg 2, but it would be more precise to refer always to the sampling cycle (1, 2, 3, and 4). Accordingly, in spite of describing slight decreases and increases of the calculated indexes (diversity, species richness, regularity) during a disrupted sampling, it could be of help to verify if their exist a statistical difference among the four sampling cycles. It does not need a lot of figures, description and discussion, to check if the perturbation during the cruise affects the taxonomic composition of the planktonic community. But it seems that there are no statistical differences and also no significant perturbations, and the manuscript focus on a spatio-temporal description, resumed in the Correspondence Analysis.

In the absence of clear temporal patterns, and a persistence of stratified conditions it might be more interesting look for vertical changes in the diversity indices, than looking for temporal changes giving high importance to the last point of almost 18 sampling days (figure 4); especially if during the last sampling cycle only two samples of net

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sampling could be taken. Considering the elevate sampling resolution in the vertical (sampling in 10 metre interval between surface and 90m depth, page 5167, line 10) there is a lot of information suitable for publication. By the way, I could not find to which depth correspond the diversity indexes corresponding to the bottle sampling shown in figure 3.

Overall, results and discussion is to long. In spite of long descriptions, some correlations could be calculated and commented in order to avoid large and confusing comments (for example, page 5174, line 20: This pattern is partly related to regularity (I think the authors mean evenness), which varied little during the cycles 1 and 2 both for dinoflagellates. Regularity became more variable during the leg2 with a final decrease that followed a trend to increase). After reading this sentence, the reader still do not know if there is relation between the diversity and regularity of dinoflagellates.

Looking for all possible links between taxonomy and the encountered salinity minimum, the authors makes contradictions. At one hand, the low salinity is due to advection of coastal water as indicated by Scripsiella (and the associated community, page 5181, line 2), on the other hand they use the same event to explain a change in the taxonomic composition due to competition, suggesting that the low salinity is a stress factor for diatoms which are substituted by silicioflagellates.

Some technical corrections

Concerning the diversity indexes I would change the term of regularity for evenness, in fact this is the term used by Raybaud et al (same issue) for the same indices.

Page 5171 Line 23: I suggest to substitute “salinity isoclines” by halocline.

If there is more than one fluorescence, chlorophyll or diatom peak, I suggest a numeration from surface to depth.

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Interactive Comment

Line16: Practical Salinity Units (PSU), is not in use Line 20: I suppose that the second peak of diatoms correspond to the 50m peak of fluorescence mentioned in the last line of page 5171.

Page 5166 The Lines 1 -15 should be included in material and method.

Interactive comment on Biogeosciences Discuss., 5, 5163, 2008.