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Biogeosciences Discussions

Interactive comment on "N-limited or N and P co-limited indications in the surface waters of three Mediterranean basins" *by* T. Tanaka et al.

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The authors have conducted four-day bioassay experiments at three different basins in the Mediterranean to evaluate nitrogen and phosphorus as possible limiting factors for autotrophic (pro- and eukaryotes) and heterotrophic prokaryotic (Hprok) plankton in the euphotic zone. The experiments are not extensive in terms of numbers, but indeed in terms of the number of chemical and biological variables recorded. The main contribution of the paper is to demonstrate that 1) assessment of limiting factors is not trivial for the osmotrophic community, and 2) that systems may temporarily and spatially switch between different limiting factors. The first was emphasized by Heckey & Kilham (1988, L&O), and they particularly focused on the limited use of various approached in studies of marine systems and thus little robustness in conclusions. Although published

more than 20 years ago, the comment is still valid. The present study is, with its broad range of response variable, a contribution to fill the gap. The second contribution may be highly relevant for many marine areas. Although historically considered to be a N-limited medium for phototrophs, full seawater is a fairly balanced growth medium for phytoplankton with its atomic N:P ratio of 16. As a consequence one may hypothesize that marine systems easily may switch between different limiting factors spatially or temporarily. Both the data presented and comparisons with previous studies give support to such a hypothesis.

Experimental design and methods used are generally very good and state of the art. There is some confusion on the use of terms regarding the tracer experiments with radioactive P. It is claimed that uptake rate is measured, but this is strictly not true (pp. 8151-8152). The inverse of the turnover time (T) is the uptake rate constant, whereas the uptake rate would be PO4 concentration multiplied with the uptake rate constant (or divided by T). This also has implications for the definition of specific affinity on p. 8146 in the Introduction. In general the results are presented and discussed in a balanced way. However, the calculated DIN:PO4 ratios should be deleted from Results (p. 8154), and the discussion of these ratios should be kept to a minimum (i.e. close to zero; pp. 8157-8158). As several of the measurements in Table 1 are below the detection limit, some strange arithmetic's is the results. E.g. for station A the DIN:PO4 ratio is calculated as: (34 + <20) : <10 = 6.5:1. Thus the discussion on pp. 8157-8 is partly without data. This should be revised, even though it does not affect conclusions. I would have like some more discussion of the fact that PO4 concentrations increase considerably with time in the two treatments not receiving PO4 for Stn B and C (Fig. 1), and at the same time turnover time of PO4 decrease considerably (Fig. 2, notice log scale). Rough by eye calculations suggest that the uptake of PO4 may have increased a factor 10 to 20. This emphasize that it is the flux and not the concentration that best describe the demand for a nutrient. Moreover, why did PO4 and not NH4 increase with time, and which consequences does this decoupling between N and P regeneration have for limiting factors of auto-and heterotrophs?

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Interactive Comment

The data are well presented in one table and several graphs, and the description and statistical analysis is (with the exception mentioned above) balanced and appropriate.

Minor comments: p. 8159, l. 14-16: Hard to understand. Sterner & Elser is missing in the References. I have not checked the rest. Table 1: This is not a list of Parameters (as stated), but Variables.

Interactive comment on Biogeosciences Discuss., 7, 8143, 2010.