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Biogeosciences Discussions

Interactive comment on "Nutrient control of N_2 fixation in the oligotrophic Mediterranean Sea and the impact of Saharan dust events" *by* C. Ridame et al.

Anonymous Referee #2

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Review: Nutrient control of N2 fixation in the oligotrophic Mediterranean Sea and the impact of Saharan dust events. Ridame et al.

The study of Ridame et al. report on a series of nutrient perturbation bottle experiments from 3 stations in the Mediterranean Sea during the BOUM cruise. The study occurred during strong stratification and extreme dissolved inorganic P (DIP) limitation was present in the upper water column. They report a variable response in measured N2 fixation rates between stations when bulk water was amended with DIP, dissolved iron (Fe), a combination, or Saharan dust. They conclude that the community was in part responsible for the variable response, and in the case of the dust addition, an un-

known chemical element (trace metal) could also have been added which influenced activity. The report is complete, concise, and given the paucity of N2 fixation rate measurements for the Mediterranean Sea, it represents a valuable data set.

Although the expedition is complete, a few other measures or references to potential parallel studies would be helpful in the discussion of the results. For example, a brief description is provided about the community composition. Is there information or data on the actual community composition, i.e. molecular genetic assays, or microscopy cell counts? The gene expression related to the metabolic activity, i.e. nif and pho genes, could also be explained. Is the chemical characterization of the dust amendment known? If dust deposition is in fact a major influence on N2 fixing activities and community structure it would be valuable to identify if the rates increase or decrease over the seasons by these amendments? And finally, it would have been interesting to include or report on 13C/14C label(s) experiments (assuming 13C uptake was also measured) as carbon fixation is an important component of N2 fixation.

Questions and comments.

Page 2632, line 4. Which diazotrophic cyanobacterial picoplankton are the authors referring to? Are these the unicellular groups (i.e. A, B, C)?

For the non-expert on time series stations and research programs in the Mediterranean Sea, DYFAMED (pg 2632, line 15) and BOUM (pg 2634, line 3) will not be recognized.

Page 2632, line 22. After which phytoplankton bloom? Is this in reference to a seasonal bloom or are a potential scenario? In the results and in the methods section, references to anti-cyclonic eddies are made; maybe the authors should provide a brief description on these features and their potential importance/influence/relevance to their study on N2 fixation?

Page 2636, line 6. In section 2.1, N2 fixation rates were described for 0-24 and 24-48 hr incubations, here it is described at 0 and 24 hr?

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Page 2637. Is the initial N2 fixation rate, the 0-24 hr incubation or time 0? If it is in reference to the bottles sacrificed at the start of the experiment, or time 0, is this really possible to measure given the new evidence of underestimation due to dissolution of N2 bubble? Mohr W, Grosskopf T, Wallace DRW, LaRoche J. (2010). Methodological underestimation of oceanic nitrogen fixation rates. PLoS One 9: 1–7.

Page 2639, line 14. Note the reference to Needoba et al. 2007 study and maximum abundance of small diazotrophs within 22-24 °C temperature is to a study off the coast of N. California with SST and temperatures within the mixed layer which were below 20° C.

Sections 4.2-4.3. These sections might be combined into one as it appears repetitive. An additional reference that might be useful for comparison of rates and responses to P and dust amendments is an earlier study in the Red Sea: Foster et al. 2009. Seasonality of N2 fixation and nifH gene diversity in the Gulf of Aqaba (Red Sea) Limnol. Oceangr. 219-233.

Sections 4.3. Could the dust also contain some contaminants, trace metals, chemicals, which could be toxic to cells if these were in high concentrations in the dust? Was there any evidence, i.e. decrease in biomass, which could suggest a negative effect to dust amendments?

Section 4.4. It is not clear if the various phylotypes referenced as present were from this study or a parallel study. A bit more introduction to where these results come from. For example, the percent similarity of the phylotypes (Bradyrhizobium, UCYN-A), abundance by qPCR or qRT-PCR? It is not clear why some groups would be excluded over others by volume filtered? How is it concluded that 50-100% of the activity is attributed to the smaller size fraction? This section could be improved by including more of information or connectivity between the present study and the phylogenetic diversity studies which are referenced.

Suggestion. Is it possible to take the rates and information reported here in a more

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global context? For example, how do the rates reported here compare with other rates? Or, given the measurements of N2 fixation rates, it should be possible to estimate the amount of new nitrogen added to the euphotic zone by N2 fixation, i.e. what percent of new production is from N2 fixation measured during the given study?

Interactive comment on Biogeosciences Discuss., 8, 2629, 2011.