

Interactive
Comment

Interactive comment on “Nutrients limitation of primary productivity in the Southeast Pacific (BIOSOPE cruise)” by S. Bonnet et al.

Anonymous Referee #2

Received and published: 17 September 2007

Review of Nutrient limitation of primary productivity in the Southeast Pacific (BIOSOPE cruise) by Bonnet et al.

This manuscript describes novel experimental observations of phytoplankton composition and productivity in a remote and little explored region of the Pacific Ocean. In particular, measurements conducted in the ultraoligotrophic waters of the center of the South Pacific central gyre are presented. This extremely oligotrophic environment poses several challenges, not only to existing experimental methods but also to current paradigms of nutrient limitation of marine productivity. The manuscript is succinct, well written and, although not conclusive, will hopefully spark further research in the area. Following are some comments which intend to improve the clarity of the text and provide additional views to interpret the results. I recommend publication of this

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manuscript in Biogeosciences, provided the authors respond to the main comments listed below. In particular, the description of the methods and experimental setup used should be improved. Also, the apparent contradiction between the lack of N₂ fixation reported here and recent results from other workers should be discussed.

General comments

Methods

The Methods section is rather uneven in terms of the amount of detail given. While some techniques are explained in detail, others are poorly described. For instance, more details are needed on the primary production method (type of bottle, amount of C-14 added, light simulation, duration of incubation, filtration, etc) and also on nutrient analysis (e.g., were samples analysed fresh? What was the detection level for each nutrient?)

The experimental setup could be described more clearly: all nutrient combinations (eg all treatments) should be described and identified in this section, so that one does not have to wait until seeing Fig. 2 in the Results section. The sentence on lines 13-15 (page 2738) is confusing (at least I couldnt guess its meaning). The starting time and the duration of the addition experiments are uncertain. It is also unclear if the parallel incubations were run immediately after the nutrient addition took place, or later. The way light quality and quantity was simulated should also be explained (eg beyond saying that it was 50% ambient light level).

Only 0.6 litres were used for 15N₂-uptake experiments. Volumes of up to 2-3 litres are frequently used by other researchers. Is it possible that the relatively small volume used played a role in the lack of N₂-fixation signal in the samples?

What was the sample volume used for nifH analysis?

Results

The authors claim that at station EGY N-Fe colimitation was taking place, based on

the observation that the FeN treatment showed a higher response than the Fe and N treatments. However, a statistical test should be done to prove that the FeN treatment was in fact different from both the Fe and the N treatments. If this is the case, this could be made clear in the figure by placing different letters above the bars for treatments which showed significantly different responses (as in Mills et al 2004).

Initial Fe concentrations are reported, but were there Fe measurements done on the incubated samples? It would be important to verify that after sample handling and incubation the Fe concentrations remained unchanged (in the controls) or increased only by the expected amount (in the Fe treatments). In other words, it is critical to demonstrate that the lack of response to Fe in the GYR station was not due to the fact that during incubation *all* treatments contained increased Fe levels (eg as a result of contamination during the preparation of the experiment).

Discussion

Page 2746: Interpretation of Fv/Fm: this variable is not always tightly related to C fixation, as it depends on the energetic coupling between photosynthetic electron transport and Calvin Cycle processes. In fact the experiment at GYR stations shows that N was limiting for C fixation, but Fv/Fm was high and remained unchanged in all treatments. What all this means is that one has to be cautious when interpreting Fv/Fm data in terms of carbon fixation rates.

There seems to be a contradiction between the results presented here and those of Raimbault et al, who found that N₂ fixation was significant in the area. The authors should clarify this apparent inconsistency: is it related to the fact that the present study does not include information on vertical variability of N₂ fixation? However, if N₂ fixation is in fact taking place and "may nonetheless represent the main source of new nitrogen for the system" (page 2748, lines 18-19), then surely something must be wrong with the nifH abundance data on Table 2, which suggest a nearly total absence of diazotrophs?

Also recent geochemical evidence suggests that N₂ fixation is important in the Pacific

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(Deutsch et al 2007) This paper is included in the References list but does not seem to be actually cited in the text. The authors should comment on the discrepancy between the conclusions of Deutsch et al and the results of their experiments during Biosope.

Finally, the authors should acknowledge the limitation that few experiments have been done and that no vertical resolution is available. This is important, since diazotrophs are characterised by a quite heterogeneous distribution, both temporally and spatially.

Minor comments

Title should read Nutrient limitation of...

I doubt there is anything like a "minimum threshold concentration". The internal quota for a given nutrient, below which growth limitation occurs, depends on the chemical composition of phytoplankton and their growth rate.

Page 2736: The title and full list of authors for Claustre et al should be given.

Page 2738 line 5: or in combination. NH_4^+ , 2 $\mu\text{mol l}^{-1}$ NO_3^-

Page 2744, line 13. Fe, N and P

Fig. 2: Presumably carbon fixation rates given are mgC mgChl^{-1} not $\text{mg}^{14}\text{C mgChl}^{-1}$. That is, DPMs have been converted in order to estimate rates of ^{12}C fixation by photosynthesis. Incidentally, reported rates are relatively high for such an oligotrophic environment (equivalent to 3-4 $\text{mgC mgChl}^{-1} \text{h}^{-1}$ if we assume 10-12 hours of light per day), considering incubations took place at 50% E_0 .

Legend to Table 2: nifH not nif . Explain superscript a. Also explain asterisks in Table 3.

Interactive comment on Biogeosciences Discuss., 4, 2733, 2007.

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