

Interactive comment on “Phosphate availability and the ultimate control of new nitrogen input by nitrogen fixation in the tropical Pacific Ocean” by T. Moutin et al.

Anonymous Referee #2

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General Comments: In this paper the authors are trying to discuss controlling mechanisms of nitrogen fixation in the tropical Pacific by phosphate availability. The new data which are making the basis of their discussion were obtained during the BIOSOPE cruise to the ultra-oligotrophic South Eastern Pacific gyre. Those new data were combined with the authors' previous surveys in the North Pacific Gyre and at a station in the South West Pacific for discussion of the subject posted in the title of the paper. The authors' major findings from the BIOSOPE cruise are, to my understanding, high phosphate availability and low nitrogen fixation by organisms other than *Trichodesmium* in the South Eastern Pacific gyre, and low phosphate availability and high nitrogen fixation (again by other than *Trichodesmium*) at the both ends of the transect,

especially in the upwelling area near Chilean coast. Among the above two new findings in this paper, I have major concerns about the N₂ fixation rates. The authors measured the N₂ fixation by following the ¹⁵N method of Montoya et al. But there are two important differences in the method adopted: The authors used only 0.6 liter sample water, and the measurement were done with a low precision mass spec. It is very hard to get reliable isotopic abundance data from insufficient sample gas using a low precision mass spec. This data set is the first of the in situ nitrogen fixation measurement in the very uniquely ultra-oligotrophic region, hence I strongly encourage the authors to reinforce the credibility of the data. I read this paper with difficulty because the logic leading to a rather arm-waving discussion and a conclusion is tangled and not straightforward. Some sentences and paragraphs are not conclusive, and instead end with speculative statements. The most frustrating thing for me was that the authors do not separate the discussion on nitrogen fixation by *Trichodesmium* and that by other un-identified organisms. Especially, this paper is presenting the first-ever-measured in situ N₂ fixation data which is very unique in the respects; showing contrasting difference from the distribution predicted by Deutch et al. and that the fixation is not by *Trichodesmium*. I would suggest the authors to discuss first about the significance of the nitrogen fixation in the South East Pacific gyre, and then discuss the controlling mechanism including P availability etc.

Specific Comments:

p2422, section 4.1.4 The term "Availability" used here is different from that defined from turnover-time. It is no more than the DIP/DIN ratio. Care should be taken, however, that the trend suggested by Deutch et al (2007) may not directly be reflected in a snap shot observation.

Technical Corrections:

Fig 1. Better to put number of degree on the latitude scale.

Fig.2. NO₃ concentration could be n-mole, instead of micro-mole.

Fig.3. Description of summer and winter symbols are better moved into legends, since it appears in N-Gyre as well.

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