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Biogeosciences Discussions



# *Interactive comment on* "Dissolved iron distribution in the tropical and sub tropical South Eastern Pacific" *by* S. Blain et al.

**Anonymous Referee #2**

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Review of Dissolved iron distribution in the tropical and subtropical South Eastern Pacific by S. Blain, S. Bonnet, and C. Guieu

The authors have provided high-quality dissolved iron data from a part of the ocean where no good data previously existed and that is at the lower limit of Fe supply by dust (but adjacent to low-O2 continental shelf waters). The data set is beautiful and is a significant advance in our knowledge.

The discussion is well written, thorough and balanced. For the most part, I concur with their interpretation of the data and don?t see the need for any revisions, save for one point raised at the end of the paper:

Quote: It has been hypothesised that DFe may also regulate the rate of nitrogen fixation

in such low nitrate environments. This seems not to be the case in the South Pacific gyre where nitrogen fixation is extremely reduced (Bonnet et al., 2007; Raimbault and Garcia, 2007) and not stimulated by iron additions. The low DFe supply rate probably did not satisfy the elevated cellular Fe quota of nitrogen fixing organisms (Kustka et al., 2003), explaining the lack of nitrogen fixing organisms commonly found during the same season in other oceanic gyres (i.e. the counter part North Pacific Gyre, Church et al., 2005).

I disagree with this statement. I think that the authors have overlooked an important characteristic of their data. The near-surface phosphorus concentrations are all about 0.2 micromolar in the oligotrophic gyre. I believe their data is accurate because the limited GEOSECS data from this region is the same. The surface nitrate is very low, and I believe the reason that nitrogen-fixing organisms don?t thrive in this high-light, high-phosphorus environment is that there isn?t enough iron for the nitrogen-fixing organisms. Yes, I see their citation of an experiment where Fe is added to the water in bottle experiments but doesn?t produce an increase in Fe fixation, but I argue that instead of indicating that Fe isn?t limiting N fixation, the experiment indicates that a lack of N-fixing organisms due to the low Fe rules out an increase of N fixation in short term experiments because the N-fixing populations are so depleted. I believe that if a longer-term experiment were to be done, the N fixers would increase in abundance and there would be a long-term increase in N fixation in this environment.

I would like to see the authors consider this interpretation in their revision.

Finally, I have read the other referee comment and feel that the concern for data quality is misplaced. Although in general it has been wise to be skeptical of Fe data, Blain has a good track record and I trust his data. However, the referee does make the good point that the data table contains excessive significant figures; I doubt that the precision of the data is better than 0.01 nM (and is probably not even that good). That also indicates that the authors should add a few sentences talking about detection limit and reproducibility at low levels.



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