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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.

S. Blain et al.

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Response to the referee #1.

The referee #1mainly questions the reality of the data set (detailed comments 1 -5). As underlined by the reviewer #2 it is a wise attitude for a scientist to be sceptical. But it is also important for a scientist to not be prejudiced against data or concepts (we exclude the hypothesis that some scientists might be prejudiced against teams). The referee seems to think that in the ocean iron concentrations should increase smoothly from the surface to the bottom or that spatial or temporal variations do not exist. During the last two decades there were two major advances in the field of iron measurements in the sea. The improvement in the analytical procedures including the recent production of the international standard with low concentrations (e.g. SAFE) AND the acceptation by most of the teams involved in this field that whole data sets should be published without

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removing results that are suspect because they cannot fully be explained. That was exactly what we did with our work in the South Pacific. The reader is then free to believe (as referee #2) or not (as the referee #1) the data. But we would like to argue that if the data set was inconsistent it would not have been possible to draw some important conclusions as for example: The difference between the northern and the southern hemisphere, The identification of the source of iron based on the construction of Fe*. The referee #2 seems to share our view.

We agree with comment #6. The number of digits has been reduced in table 1 to take into account the precision and the limit of detection that is reported in the paper

Question 7 and 8 are certainly interesting but it was not the objective of the paper to discuss the status of the different nutrients (micro and macro) in the control of biological production. Other papers in the special issue are dedicated to this important question (see for example Bonnet et al.)

Response to the referee #2

The number of digits was corrected in table 1. The limit of detection is mentioned in section 2.1 of the paper. We have modified the last paragraph of the paper to take into account the comment of the referee. The new version is.

Our data show that DFe was low in the entire South Pacific gyre, but NO3- was also extremely low (Raimbault et al. this issue). The limitation of primary production by Fe on the edges of the gyre was demonstrated by deck incubation experiments (Bonnet et al. this issue). By contrast, evidence for severe nitrogen limitation of primary production was observed in the centre of the gyre (Bonnet et al., this issue). It has been hypothesised that DFe may also regulate the rate of nitrogen fixation in such low nitrate environments. During incubation experiments carried out in the centre of the South Pacific gyre, iron addition did not stimulated nitrogen fixation (Bonnet et al., this issue; Raimbault et al., this issue). This result seems to be at odds with the possible limitation of the nitrogen fixation by iron availability. However the lack of response in

BGD

4, S2131–S2133, 2007

this short term experiment could also be due to the quasi absence of N-fixing populations. Temporally more detailed information is required to fully understand the role of Fe on primary producers in these oligotrophic waters.

Interactive comment on Biogeosciences Discuss., 4, 2845, 2007.