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Interactive comment on "Distributions and stoichiometry of dissolved nitrogen and phosphorus in the iron fertilized region near Kerguelen (Southern Ocean)" *by* S. Blain et al.

Anonymous Referee #1

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Blain et al. present a subset of the results from the recent KEOPS2 study of the region surrounding the Kerguelen plateau in the Southern Ocean. Analysing both inorganic and organic dissolved nutrients they largely confirm previous observations in the Southern Ocean whereby marked blooms, particularly those dominated by diatoms, tend to be characterised by relatively low N:P uptake ratios. Although the overall result is not especially novel, the study provides a confirmation that the low N:P ratios are also characteristic of the KEOPS2 bloom and the authors provide a useful and interesting extension of previous work to include organic nutrient measurements. The results are well presented and the manuscript is well written. Overall, the manuscript thus represents a useful contribution to the literature, particularly when considered alongside

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the body of other information collected during the KEOPS2 study. I have a few minor comments/suggestions which the reviewers might wish to consider in revising their manuscript.

Specific points:

Overall the demonstration and discussion of the relatively low N:P drawdown ratios are clear. However, given the extensive data set which is available, as evidenced by the other KEOPS2 papers in the special issue, I was left wondering whether a bit of further investigation/demonstration of the causes couldn't have been carried out. For example, the authors argue that bloom dominance by diatoms was likely the cause of the relatively low N:P removal ratios, but no data on community structure is presented, at least directly within the current manuscript. Similarly, (e.g. Page 9962), could you use total nutrient or even DIC drawdown as an index of bloom duration to demonstrate this point? Effectively this is apparent in Figure 9, i.e. the high values of N* occur as both N and P are progressively depleted in the surface waters, but the authors could perhaps have been more quantitative.

Minor points:

Page 9950, Line 18: I believe you mean '. . .the occurrence of a subsurface minimum of N*. . .'

Page 9952, first paragraph was a bit awkward, rephrase?

Page 9952, Sampling section: before describing how the samples were collected from the bottles, it would be useful to describe how the samples were collected from the water column. Additionally, information on the collection of a second set of samples which didn't end up being analysed would seem to be a bit redundant?

Page 9953: Queroue et al. 2014 didn't appear in the reference list.

Figure 1: There was a lot of information on this figure and it was potentially difficult to distinguish where the stations were. It would be useful if clarity could be improved

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further.

Figure 9 caption: I don't think you mean that different values of rN:P were used in the calculation here. You assume rN:P = 16 throughout.

Figure 10: It would have been useful to have seen the corresponding profiles of N and P (not just N*).

Interactive comment on Biogeosciences Discuss., 11, 9949, 2014.

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