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Comment

# ***Interactive comment on “Iron budgets for three distinct biogeochemical sites around the Kerguelen archipelago (Southern Ocean) during the natural fertilisation experiment KEOPS-2” by A. R. Bowie et al.***

**Anonymous Referee #1**

Received and published: 20 January 2015

General comments:

I found this to be a well-written synthesis paper bringing together many aspects of the other manuscripts in this special issue to produce a set of comprehensive iron budgets from the data collected. The paper combines dissolved and particulate iron concentration measurements made during the KEOPS-2 cruise with export fluxes, iron uptake and regeneration rates and estimates of lateral, upwelling, vertical diffusive and entrainment processes measured during the same study to synthesize biogeochemical iron budgets at three contrasting sites within the Kerguelen archipelago region: over

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the plateau, downstream of the islands and at a reference site upstream of the Kerguelen plateau. There are very few examples of such budgets available in the literature and so this study represents an important addition to our understanding of the relative importance of various supply terms in the biogeochemical cycle of iron in the ocean. The inclusion of an estimate of the vertical entrainment of iron in this case suggests an overlooked but significant supply term in the surface ocean iron budget. Similarly, the relatively low contribution calculated to be supplied by bacterial regeneration and zooplankton grazing in this study compared to the other iron budgets available highlights the variability of this term. My recommendation is that this manuscript be published in Biogeosciences after a few minor points are addressed.

Specific comments:

There is no reference made to blanks measured during the description of particulate iron sampling (page 17872, section 2.3.2). I appreciate that this part of the study is described in more detail in an accompanying paper, but I think it would be worthwhile to include something to the effect that blanks were carried out and were low compared to sample measurements (which I assume was the case).

In comparing the two dFe profiles at stations A3-1 and A3-2 (page 17877, lines 7-8), the authors state that “below the mixed layer, similar profiles were observed during both visits to A3”. However, it appears that the profile below the mixed layer during A3-1 consists of two points. Whilst the both profiles do show an increase in dFe concentration with depth below the mixed layer, based upon interpolation between the data points available, I feel the fact that only two data points contribute to the A3-1 profile makes the statement a little misleading.

During the discussion of export measurements (page 17880, line 26 and Section 3.2.4) it should be mentioned that as the value calculated at station R-2 was derived from Th measurements it reflects integrated export over the previous ~30 days, rather than instantaneous vertical flux at the time of sampling. As this was the reference site, with

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low phytoplankton abundance, the longer time period probably has a minimal effect upon interpretation of the data.

The authors estimate the amount of the winter dFe stock that had been used by the time of sampling during KEOPS-1 based on the observed decrease in concentration during the current study and with the assumption that annual variability is low (Page 17881 Line 28 – Page 17882 Line 2). Obviously there is limited additional dFe data available to comment on the validity of this assumption, but is it possible to do so based on indirect methods (e.g. interannual variation of bloom progression from satellite imagery)? The assumption of low inter-annual variability is mentioned again at Page 17890 Line 10, but with the caveat included that this may not be so (citing Grenier et al., 2014) and I think that this caveat should also be included during the earlier comparison to KEOPS-1.

The estimates of biogenic iron are calculated from particulate phosphorus data with the assumption that all particulate phosphorus collected has a biogenic origin. Can this assumption be validated from elemental composition of the local rocks (i.e. that they do not contribute a significant amount to the particulate P pool)?

There are several references to Fe/C ratios of “suspended” particles on Pages 17892 and 17893, but the authors state on Page 17871 that particulate Fe data reported in this manuscript is the sum of 1-53um and >53um size fractions collected by in situ pumps. As such, the particulate material collected undoubtedly consists of both suspended and sinking pFe and POC. The references on pages 17892 and 17893 should be reworded, or an explanation included on page 17871 that the measured particles are included as a “suspended” term, while acknowledging that there is some sinking material included in that term.

In the comparison between mixed layer Fe/C ratios and those of phytoplankton intracellular uptake (page 17892, lines 1-13), I feel that the current paragraph structure may be interpreted as suggesting that the contribution of lithogenic and detrital Fe to

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the suspended material Fe/C is a relatively minor factor. I would argue that this would be the most important factor in explaining the much higher values for Fetot/C of suspended material and should be emphasized more - looking at the lithogenic and total pFe concentrations in Table 1, the lithogenic fraction alone contributes 15-66% of the total pFe. Also, the following paragraph (line 14) starts with the statement that Fetot/C was also calculated – is this not the same as described in the paragraph before? In explaining the surface-to-deep increases in Fetot/C, the authors state that scavenging drives the Fetot/C ratio (page 17892, line 22). Is it possible that a greater lithogenic contribution to particles is also a factor? This is later mentioned as a reason for the higher Fetot/C observed in sinking particles over the plateau (page 17893, line 29)

Although the text states “a comparison of Fe supply and demand at the three sites around the Kerguelen archipelago in spring was possible (Fig. 7)...” the figure only includes budgets for the plateau and plume sites. As there are a limited number of detailed biogeochemical iron budgets published and the reference site does after all represent a different variation of the upper ocean iron cycle (in terms of the magnitude of iron supply), I feel that it would be worthwhile including the third budget here. Also relating to Figure 7, the way it is set out makes it appear that the lateral advection input to the plume also represents an output term from the plateau station. Was this intentional? Although the plateau and plume certainly do seem to be linked by a lateral export of Fe from one to the other, the source of this plume Fe is discussed in the manuscript (Page 17886 Line 1) as being from the northern part of the plateau, rather than from the area of station A3. This being the case, the current spacing between Figure 7a and 7b could be misleading.

Additional comments (with page and line numbers):

Page 17868, Line 23: change “...were taken tracking...” to “...were taken as tracking...”

Page 17869, Line 7: replace “passes” with “pass”.

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Page 17879, Lines 22-24 and Figure 3d-f: The pFe concentrations given in the text in reference to the profiles of stations E-1, E-3 and E-5 appear to be lower than the concentrations shown in Figure 3d-f. Perhaps the numbers quoted refer to the 1-53  $\mu\text{m}$  size fraction only and the data in the figure is for material  $>1\mu\text{m}$ . As far as I could tell the values stated for the reference and plateau stations did match the figure.

Page 17881, Line 12: Suggest changing "...on plateau A3 compared to the plume E..." to "...on the plateau (station A3) compared to the plume station, E..."

Page 17883, Line 23: Italicize "w" and include the "ek" subscript.

Page 17889, Line 7: Replace "and" with "at".

Page 17890, Line 14 and Table 3: The range of mesoplankton Fe regeneration rates given in the text (0.04-0.08  $\text{pmol Fe/L/d}$ ) does not match that in the table that it refers to (0.02-0.08  $\text{pmol Fe/L/d}$ ).

Page 17893, Lines 24, 25: Change "conversation" to "conversion"

Figure 2: Polar Front is shown as a white dashed line and labeled in the first panel. The caption could be changed from "The polar front is shown as a black dashed line" to "The polar front is shown as a black dashed line in panels b and c".

Figure 4: Change "Note difference scale for..." to "Note difference in scale for..."

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Interactive comment on Biogeosciences Discuss., 11, 17861, 2014.

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