

## ***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.***

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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.

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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 com-

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prehensive 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 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.

We thank the reviewer for his/her very positive comments.

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). Procedures for determining blanks for particulate iron were reported in the companion paper (Table A1 in van der Merwe et al., 2015) following methods previously documented in Bowie et al. (2010). Blanks were typically 2-3% and <1% of the pFe sample concentrations for the ISP deployments and P-trap deployments, respectively. The following sentence has been added to the manuscript (page 17872, lines 16-). “Blanks from replicate analysis of filters treated identically to the sample filters, but without large volumes of seawater

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passed through them, were typically 2-3% and <1% of the pFe sample concentrations for the ISP deployments and P-trap deployments, respectively.”

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.

As noted on page 17877 line 9, operational constraints (bad weather) prevented us from sampling deeper than 340 m at station A3-1. We agree that we only have 2 dFe datapoints below the mixed layer (123m) at A3-1 (Figure 3b, page 17919), but still contend that the profile shows an increasing trend towards the seafloor. Our statement is supported by the profile at A3-2 (5 datapoints below the mixed layer; Figure 3c), the KEOPS-1 dFe profiles at station A3 (Figure 4b, page 17920) and our knowledge of plateau shelf sedimentary supply process for dissolved iron in the ocean. We have changed the sentence to (page 17877, line 10-): “Such enrichments at depth were also observed in dissolved Mn and Co profiles (F. Qu  rou  , personal communication, 2014; data not shown) and dFe profiles from the occupations of station A3 during the KEOPS-1 study (Fig. 4b), indicative of plateau sedimentary supply.”

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 low phytoplankton abundance, the longer time period probably has a minimal effect upon interpretation of the data. We agree with the reviewer and did note on page 17889 line 16 of our original manuscript the following: “estimates based on 234Th at this time, reflecting the previous ~30 days of export”. We have added the sentence to

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note 4 of Table 1, which we think is the most appropriate place: “This method will reflect the integrated Fe export over the previous ~30 days, rather than an instantaneous flux at the time of sampling. Since this was a reference site, with 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. We agree with the reviewer that this is a data sparse region and estimates of winter stocks based on the assumption of low interannual variability may have inherent uncertainties. We earlier examined interannual variability in bloom development and progression in the study region (Mongin et al., 2008), but believe that extrapolating this analysis to dissolved Fe is problematic as concentrations are dependent on a number of factors not determinable via satellite. We have modified the sentence to include this possible caveat (page 17882, line 1): “If annual variability is low, which may not always be the case (Grenier et al., 2014), by late summer >90% of the winter stock had been used with only 4.7  $\mu\text{mol dFe m}^{-2}$  remaining in the surface mixed layer at A3 (KEOPS-1 data; Blain et al., 2007).”

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)? This concern is valid and

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was pointed out in a review of our companion paper (van der Merwe et al., 2015). In response to that reviewer, van der Merwe et al. modified their text to: "If we assume that all particulate phosphorus (pP) is of biogenic origin, we can calculate the biogenic Fe fraction of the total Fe concentration by normalising to pP and comparing with published elemental ratios of Southern Ocean diatoms (Planquette et al., 2013). For the calculations we used the upper limit of Fe:P (1.93 mmol mol<sup>-1</sup>) reported by Twinning et al. 1 (2004) for Southern Ocean diatom assemblages. Given that pP and POC are remineralised throughout the water column and are generated within the surface mixed layer, calculations of biogenic trace metals will only be valid within the surface mixed layer, as the concentration of pP and POC decreases strongly with depth. It should also be noted that Kerguelen Island basalts and upper continental crust can contribute particulate phosphorus concomitantly with pFe to the particulate pool. However the Fe:P ratio found within Kerguelen Island basalts and the continental crust is 12.8 and 25.8 (mol:mol) respectively (Gautier et al., 1990; Wedepohl, 1995). Thus, the factor of 1000 increase in pP within suspended particles compared to these rock sources indicates that this pP is likely produced insitu within the mixed layer from dissolved PO<sub>4</sub> rather than supplied from rock weathering. Furthermore, within the upper 200 m of the water column, biogenic Fe correlates significantly with both fluorescence (Spearmans RHO R = 0.518, P < 0.05, n = 30) and dissolved oxygen (Spearmans RHO R = 0.507, P < 0.05, n = 30) confirming the autotrophic composition of the particles identified as high in biogenic Fe." In this paper, we cite the modified van der Merwe et al. text and have added a sentence (page 17882 line 8): "These calculations follow methods reported in Planquette et al. (2013), and assume particulate P is not in part derived from local rock weathering. van der Merwe et al. (2015) have tested this assumption using Fe/P ratios in Kerguelen Island basalts and the upper continental crust, and note that the ~1000-fold increase in pP within suspended particles can only be explained by pP produced in situ within the mixed layer from dissolved PO<sub>4</sub>."

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 C9375

manuscript is the sum of 1-53µm and >53µm 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. We have changes the text on page 17892 line 14 to acknowledge that some sinking particles may be included in that term: "We also note that pFe data is the sum of 1-53µm and >53µm size fractions collected by ISPs and thus may also include some sinking particles. This may affect the suspended Fe/C ratios."

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 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) We have changed the text on page 17892- of the article as follows. We have also changed the term Fe/C<sub>tot</sub> to Fe/C throughout the manuscript for consistency. "Suspended mixed layer Fe/C ratios (Table 1) were significantly higher than phytoplankton intracellular uptake ratios. This finding is likely the result of the contribution of lithogenic and detrital Fe to suspended material Fe/C ratios, and is consistent with the removal of C at a faster rate than that of Fe, and for Fe to be added through new sources after phytoplankton uptake. Differences may also arise because of luxury uptake, the

timescale of integration in deckboard experiments compared to Fe/C ratios in ocean suspended and sinking particles (which are broadly similar – see below), and/or that our system was not in steady-state. Also, since a Ti-citrate-EDTA wash was used to remove extracellular surface Fe during the incubation experiments, but not on particles collected in the ISPs and P-traps, our suspended and sinking pFe concentrations include Fe present within cells, adsorbed to cell walls, detrital Fe and lithogenic Fe. This would tend to increase Fe/C in suspended particles. Differences between intracellular and suspended mixed layer Fe/C ratios may also derive from the C term, since the ISP sampling includes detrital material as well as living cells. In addition to the ratio of “total” particulate (biogenic+lithogenic) Fe over POC (Fe/C) in suspended particles discussed above, we also calculated the ratio of biogenic Fe over POC (i.e.,  $Fe_{bio}/C$ ) following methods discussed in section 3.2.1. Profiles are shown in Figure 5.”

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. These are valuable comments. We have added a third budget in Figure 7 (panel a) for reference site R-2. We have also modified the figure (three separate panels) so that the lateral advection into plume station E does not appear to be a direct output term from station A3 (this was not our intention).

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Additional comments (with page and line numbers): Page 17868, Line 23: change “. . . were taken tracking. . .” to “. . . were taken as tracking. . .” Done

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

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. Thank you for pointing that out. Figure 3 plots correctly used  $>1\mu\text{m}$  pFe data, whereas the original text referred to the 1-53  $\mu\text{m}$  size fraction only. We have modified the text for A3 and E stations (station R-2 was okay).

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. . .”. Done

Page 17883, Line 23: Italicize “w” and include the “ek” subscript. Done

Page 17889, Line 7: Replace “and” with “at”. Done

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  $\text{pmol Fe/L/d}$ ). Sorry, incorrect text. Fixed

Page 17893, Lines 24, 25: Change “conversation” to “conversion” Done

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”. Done

Figure 4: Change “Note difference scale for. . .” to “Note difference in scale for. . .”. Done

â&ccaron Anonymous Referee #2 Received and published: 18 March 2015

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“ Iron budgets for three distinct biogeochemical sites around the Kerguelen archipelago (SO) during the natural fertilization experiment Keops-2” by Bowie et al. This is a very good manuscript, well written, clear and above all very interesting. It describes the multiple sources of Fe for phytoplankton on three different sites close to the Kerguelen Islands in the Southern Ocean. Budgets are made of sources and sinks in which fluxes are compared with the needs of phytoplankton at the three sites. These budgets differ per site. We thank the reviewer for his/her very positive comments, and for the very useful comments below which have improved our manuscript.

I have only minor comments. Three comments are relatively important: the influence of error estimates on the budget are not discussed. Information on certain calculations and assumptions is missing or difficult to find, since it is given in the discussion instead of methods (see below). The references used are not always correct. We have modified the text to make it easier for the reader to understand the budget calculations (and associated assumptions) and updated the references, as detailed in our response below. We have included estimates of the error bound where available in Table 1.

The title: In the title and whole manuscript the word experiment is used. This word implies manipulations whereas it is essential here that it is a natural fertilization. I suggest research or study. We have modified the text, and changed the word “experiment” to “study” in both the title and manuscript text.

Abstract line 7: introduce the term new here, so that it is clear below which supplies are considered new, and which are not. We have changed the sentence to: “Iron supply from ‘new’ sources (diffusion, upwelling, entrainment, lateral advection, atmospheric dust) to surface waters of the plume was double that above the plateau and 20 times greater than at the reference site, whilst iron demand (measured by cellular uptake) in the plume was similar to the plateau but 40 times greater than the reference.”

Introduction P 17864 Line 2 add “ marine” to carbon cycle. Done

Line 17: add de Baar et al., 2005 to the Boyd reference. Done

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Line 22-24: what about wind mixed layer depth and differences in estimated or assumed Fe/C ratios of the cells? We have modified the sentence to: “This is due to a number of factors, including rapid grazing of phytoplankton in surface waters, loss of the added Fe by its precipitation and scavenging onto sinking particles, differences in estimated or assumed Fe/C ratios of the cells, and changes in wind mixed layer depth.”

P17866, line 23: In Cullen et al a lot of text is on particulate Fe however, the subject of the paper is DFe, thus this reference is not correct here. We have removed the Cullen et al. reference.

P 17867 Line 15: abbreviations ISP and P-trap are not explained yet We have changed the sentence to: “Our observations also include particulate measurements in both suspended water column (in situ pump; ‘ISP’) and sinking export (free-floating sediment trap; ‘P-trap’) particles below the mixed layer, with linkage to food web processes via discussion of iron-to-carbon (Fe/C) ratios.”

Methods P17869 Line 19-20: as closely as possible: Has anything been treated differently? If so then specify. The data of KEOPS-1 and 2 are compared in this manuscript. Is this data obtained in the same way, with the same analytical methods? We have changes this paragraph to: “All trace metal sampling and analytical procedures followed recommended protocols in the cookbook2 published by the international program GEOTRACES (Bishop et al., 2012; Cutter and Bruland, 2012; Planquette and Sherrell, 2012). All methods have been successfully used previously by this team during the KEOPS-1 (Blain et al., 2008b) and SAZ-Sense projects (Bowie et al., 2009). Subtle differences in methods employed during the earlier KEOPS-1 and SAZ-Sense projects are described in those papers and/or later in this manuscript.”

P17872 line 18: please use capital P and D to indicate particulate and dissolved. PFe for particulate Fe cannot be confused with the negative logarithm of ‘free’ iron as pFe clearly can. The terms dFe (for dissolved iron) and pFe (for particulate iron) have been used by many groups and widely reported in the literature previously. We thus prefer

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to keep these terms in our paper. They are clearly defined on page 17864 line 10 (for dFe) and page 17871 line 8 (for pFe), so I do not think there will be confusion with the negative logarithm of 'free' iron (which isn't discussed at all in the manuscript). We have added this sentence to that start of section 2.3.2 (page 17872 line 11): "Particulate Fe (pFe) was determined as follows."

P17874, line 16: instead of estimated use assumed to be equal to Done

P17875, line 6. I am glad that the description is given, it is essential for understanding the discussion. OK

P17875 which temperature is used, is it corrected for pressure, is it conservative temperature (in  $\sigma_t$ )? For st R and E this is important with depths to 2500 and 2000 m. Thanks for the clarification. Potential temperature is used and has now been defined in the caption to Figure 3.

17877 line 26 Planquette et al 2011 Frew et al. 2006 : Are Planquette et al et al really discussing the size change for small to large particles, they did not discriminate between sizes didn't they, for that they referred to Lam et al 2006. No idea whether these cover the microbial aspect. Frew et al, 2006 do not, it is an extremely interesting paper but as far as I understood they describe that the lithogenic part of PFe increases with increasing particle size. They only mention microbial dissolution of biogenic particles. Lam, P. J., J. K. B. Bishop, C. C. Henning, M. A. Marcus, G. A. Waychunas, and I. Y. Fung (2006), Wintertime phytoplankton bloom in the subarctic Pacific supported by continental margin iron, *Global Biogeochem. Cycles*, 20, GB1006, doi:10.1029/2005GB002557. Noted, we have changed the text to: "This may have been due to either: (i) physical aggregation of the particles onto diatom aggregates; and/or (ii) microbially-driven conversion of small lithogenic Fe (1-53  $\mu\text{m}$ ) to bioavailable forms and incorporation into the large (>53  $\mu\text{m}$ ) diatoms as biogenic Fe, with potentially some fraction of these larger particles exported to depths below the mixed layer, as previously discussed by Lam et al. (2006), (Frew et al., (2006) and ;

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Planquette et al., (2011)."

17878 lines 1-10: The comparison between Keops 1 and 2 cannot be done so easy, seasonal difference occur as said in the text but can the biological uptake be the cause of the differences below 400 m? How do other stations compare in Fe between KEOPS 1 and 2? Can that support the comparison here? The only exact station that was visited on both KEOPS-1 and KEOPS-2 was A3, which we compare here. We had other reference stations (C11 or KEOPS-1 and R-2 on KEOPS-2) and stations east of Kergulen (A11 on KEOPS-1 and E stations on KEOPS-2), which we compared on page 17876 (lines 7-14) and page 17880 (lines 4-9), respectively. We agree that biological uptake cannot explain the differences between Fe concentrations in waters deeper than 400 m at station A3. We did note in our original text that biological uptake must be combined with seasonal changes in the strength of the supply mechanisms to A3 (discussed in van der Merwe et al., 2014) to explain the differences. Thus we have changed the text as follows: "The lower values during KEOPS-1 were likely the result of biological uptake in surface waters and export of Fe during the spring bloom prior to our arrival at the study site, combined with seasonal changes in the strength of the supply mechanisms to deeper waters at A3 (discussed in van der Merwe et al., 2014)."

Pages 17879 lines 25-30 and 17880 lines 25-28. Data treatment like using P and Al for biogenic and lithogenic fractions and Th fluxes on page 80 should be in a separate methods section, then it is not necessary to refer to information later in the text. We prefer to leave these descriptions of how the data was treated and the validity of such approaches in the Results and Discussions section (and with reference to other manuscripts who have adopted similar approaches) to minimise repetition and improve the flow of the text.

3.21 Iron Pools Line 26-29: Is it correct to assume that A3-1 is representative for winter stock, it might be prebloom, but both DFe and nitrate are lower than below the mixed layer (fig 3b), why not use the concentration just below the MLD for integration? Or perhaps mention that this winter stock is possibly (probably) an underestimation. This

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is a valid point. Unfortunately, there are no wintertime dFe profiles at A3 to use in our calculations, and for these reasons it is problematic to try to estimate the integrated winter pool. We still think this seasonal comparison is strong and therefore prefer to include a note of caution in our text. We have added this sentence: "We note this drawdown is probably a conservative estimate since the winter dFe stock was probably an underestimation (as evidenced by lower dFe in the mixed layer compared to deep waters at A3-1; Queroue et al., 2015).

Page 17882: lines 15-24, what is the range, what is the influence on the budget of the choices made here? We believe that text on page 17882 lines 8-24 adequately explains the rationale for our chosen Fe/Al ratio (of 0.36) and how this value is representative of basaltic rocks in the region. We prefer not to use a range of Fe/Al ratios as we will then have to do the same for our Fe/P ratios (to estimate biogenic Fe), and this will over-complicate the budget and make it extremely difficult for the reader to compare biogenic vs lithogenic particulate iron stocks. As we note, there is some plasticity in the chosen Fe/Al and Fe/P ratios (which we discuss in the text). Nevertheless, our estimates of biogenic and lithogenic Fe provide a perspective on the relative contributions to the total pFe pool (page 17882, lines 22-24)".

3.2.2 Lines 9 and 11 give original references of Shih and Osborn, 1980 Osborn, T.R., 1980. Estimates of the local rate of vertical diffusion from dissipation measurements. *Journal of Physical Oceanography* 10, 83–89. Shih, L. H., J. R. Koseff, G. N. Ivey, and J. H. Ferziger, 2005: Parameterization of turbulent fluxes and scales using homogeneous sheared stably stratified turbulence simulations. *Journal of Fluid Mechanics*, 525, 193–214, doi:10.1017/S0022112004002587. Done

P 17884: line 5: Indeed used by de Baar but from Gordon et al. 1977 Gordon, A.L., Taylor, H.W., Georgi, D.T., 1977. Antarctic oceanography zonation. In: *Proceedings of SCOR/SCAR Polar Oceans Conference*, Montreal, Canada, May 5–11, 1974., Dunbar, M.J.(Ed.), Arctic Institution of North America. McGill University, Montreal. We have changed the sentence to: "...and a conservative value of  $w_{ek} = 0.13 \text{ m d}^{-1}$  for

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the open Southern Ocean (used by de Baar et al. (1995); originally reported in Gordon et al., 1977) was chosen for our reference station."

Line 19: here WW values are used, why indeed not do the same in A3-1 to calculate winter stock (see above). We have addressed this point above for station A3-1.

Lines 15-18: interesting, however, it is not clear how this was derived, can you explain in a few words? I realize that I probably ask too much here. However, reading on line 22: this value, causes the reader to wonder which value?? The method for calculating vertical supply due to entrainment at station A3 and E is described in full in the Supplement, so we do not wish to describe it again in the main body of the paper. For the 'detrainment' calculations for station R-2 (line 22), we have revised the text to: "Detrainment' at R-2 was accounted for by multiplying this new entrainment flux by the summer-to-winter MLD ratio."

Lines 18-20: how does the simplification applied to A and E compares with the detrainment-derived value? We performed the same 'simplified' calculations (used to determine entrainment at R-2) for stations A3 and E. This resulted in an entrainment of 240 and 222  $\text{nmol m}^{-2} \text{ d}^{-1}$  for A3-1 and A3-2, and 25 and 15  $\text{nmol m}^{-2} \text{ d}^{-1}$  for E-3 and E-5. Thus the simplified method gives lower entrainment values than the more robust calculations based on mixed layer depth excursions and their frequency (Supplement), although the relative magnitude of the fluxes between A3 and E stations were the same.

Page 17885: from line 3 onwards: the lateral fluxes: at which depth(s) are the lateral flux calculations applied to? For the plateau, lateral Fe supply (difference between inflowing and outflowing lateral fluxes of dissolved iron) was estimated in the 0-150 surface depth band (refer to Fig 3b in Chever et al., 2010 for details). We have changed the text as follows: "On plateau Fe supply at station A3 was taken from the steady-state box model of Chever et al. (2010) which used the horizontal dFe gradient and current velocities from Park et al. (2008a) to calculate the lateral flux of 180  $\text{nmol m}^{-2} \text{ d}^{-1}$  in

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the 0-150 m depth band above the plateau; noting this model used KEOPS-1 data.” For the plume, we also used a depth band of 0–150 m, considered as the winter mixed layer in the plume over the season. This text has been added to the manuscript.

Lines 10-20: I do not understand how these calculations are done, and since it is interesting it would be good to either explain more extensively or add an explanation in the supplement. The calculations are explained fully in the KEOPS-2 companion paper, to which the reader is referred to (d’Ovidio et al., 2014).

Line 18 is it allowed to use the Keops 1 data, since this paper shows quite some differences between the two studies? We have added this caveat to the text: “On plateau Fe supply at station A3 was taken from the steady-state box model of Chever et al. (2010) which used the horizontal dFe gradient and current velocities from Park et al. (2008a) to calculate the lateral flux of 180 nmol m<sup>-2</sup> d<sup>-1</sup> in the 0-150 m depth band above the plateau; noting this model used KEOPS-1 data.”

Page 17891: line 5: Refer to Brussaard et al 2008. Brussaard, C.P.D., Timmermans, K.R., Uitz, J., Veldhuis, M.J.W., 2008. Virioplankton dynamics and virally induced phytoplankton lysis versus microzooplankton grazing southeast of the Kerguelen (Southern Ocean). *Deep-Sea Research II* 55, 752–765 Done

P 17895: any idea why regeneration differs so much between the KEOPS studies? We believe this is largely due the stage of phytoplankton bloom development, in other words seasonality. Our estimates of iron regeneration rates are based on biomass and activity of heterotrophic bacteria and mesozooplankton. Biomass and activity of heterotrophic bacteria and mesozooplankton are both a response to the phytoplankton bloom and the associated DOC and POC production. This response is more pronounced during the peak-decline of the bloom as compared to the early stage of the bloom. We note this on page 17891 lines 9-11 and thereafter: “Importantly, Fe regeneration was much lower during the early compared to late bloom stage and was dominated by bacterial regeneration in spring (60-90% of total Fe regeneration).” Other

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possible reasons are discussed on page 17895.

End of page 17895 and start 17896: One can only conclude that a flux is missing if the other fluxes and calculations are assumed to be correct within the estimated errors. One sentence is needed is here to make the assumption or discuss the errors. We have changed the text to: “Assuming all flux calculations to be correct within the estimated error bounds in Table 1, this implies there is a missing flux term in the budget at A3 and this is likely lithogenic pFe from the Kerguelen plateau and/or Heard Island (and this may be converted to biogenic Fe).”

Line 11: Shaked and Lis have written an excellent paper on Fe availability, however the dissolution of PFe by organic ligands is only briefly mentioned by them, add Thuroczy et al 2012 as reference; they measured complexation in Antarctic waters and discussed the role of ligands in transporting and dissolving PFe into DFe and refer to the more theoretical work of Borer. Thuroczy, C-E, Alderkamp, A.-C. Laan, P, Gerringa, L.J.A., de Baar H.J.W., Arrigo, K.R., 2012. Key role of organic complexation of iron in sustaining phytoplankton blooms in the Pine Island and Amundsen Polynyas (Southern Ocean). *DSR II*, 71-76, 49-60. Borer, P.M., Sulzberger, B., Reichard, P., and Kraemer, S.M. (2005). Effect of siderophores on the light-induced dissolution of colloidal iron(III)(hydr)oxides. *Mar. Chem.* 93, 179–193. Thanks for the extra references. We have changed the text to: “This will vary largely with the mineralogy (Schroth et al., 2009), provenance of the particles, and seawater characteristics (e.g., organic complexation; Shaked and Liset et al., 2012). Indeed, Thuroczy et al. (2012) previously measured complexation in Antarctic waters and discussed the role of ligands in transporting and dissolving pFe into dFe, using theoretical data provided by Borer et al. (2005).”

Page 17898, lines 16-18: It is not clear what the meaning is of this sentence. We have changed the text to: “This coupling has important implications for geoengineering schemes that propose to increase the supply of Fe to surface waters by pumping waters from below.”

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Please also note the supplement to this comment:  
<http://www.biogeosciences-discuss.net/11/C9371/2015/bgd-11-C9371-2015-supplement.pdf>

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

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