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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.***

## **Anonymous Referee #2**

Received and published: 18 March 2015

“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.

I have only minor comments. Three comments are relatively important: the influence

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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.

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. Abstract line 7: introduce the term new here, so that it is clear below which supplies are considered new, and which are not. Introduction P 17864 Line 2 add “ marine” to carbon cycle. Line 17: add de Baar et al., 2005 to the Boyd reference. Line 22-24: what about wind mixed layer depth and differences in estimated or assumed Fe/C ratios of the cells? 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. P 17867 Line 15: abbreviations ISP and P-trap are not explained yet 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? 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. P17874, line 16: instead of estimated use assumed to be equal to P17875, line 6. I am glad that the description is given, it is essential for understanding the discussion. P17875 which temperature is used, is it corrected for pressure, is it conservative temperature ( $\Theta$  in  $\text{E}\check{\text{Z}}\text{C}$ )? For st R and E this is important with depths to 2500 and 2000 m. 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

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the subarctic Pacific supported by continental margin iron, *Global Biogeochem. Cycles*, 20, GB1006, doi:10.1029/2005GB002557.

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?

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

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.

Page 17882: lines 15-24, what is the range, what is the influence on the budget of the choices made here?

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.

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.

Line 19: here WW values are used, why indeed not do the same in A3-1 to calculate

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winter stock (see above). 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?? Lines 18-20: how does the simplification applied to A and E compares with the detrainment-derived value? Page 17885: from line 3 onwards: the lateral fluxes: at which depth(s) are the lateral flux calculations applied to? 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. Line 18 is it allowed to use the Keops 1 data, since this paper shows quite some differences between the two studies?

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

P 17895: any idea why regeneration differs so much between the KEOPS studies?

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.

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 Thuróczy 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. Thuróczy, 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 col-

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loidal iron(III)(hydr)oxides. Mar.Chem. 93, 179–193. Page 17898, lines 16-18: It is not clear what the meaning is of this sentence.

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