

Interactive comment on “An iron budget during the natural iron fertilisation experiment KEOPS (Kerguelen Islands, Southern Ocean)” by F. Chever et al.

F. Chever et al.

Fanny.Chever@univ-brest.fr

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Thanks for your insightful comments, which greatly help to improve the manuscript.

General: This budget study at a site of natural iron fertilisation is a valuable contribution to this special issue. Although I have classed as subject to major revisions, the revisions required are moderate, not major or minor.

The authors should clarify better what the main aim of constructing the budget is. It appears that the main aim is to refine the C sequestered per unit iron ratio for KEOPS rather than to explore this interesting system dominated by PFe and by lateral supply and sediment resuspension.

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We agree with the reviewer. The aim of this budget was indeed not only to refine the C sequestered per unit iron ratio but also to better define the geochemical cycle of Fe (notably sources and sinks of DFe) during the KEOPS cruise. It was also calculated to explore the role of the TDFe as a tracer of lithogenic inputs coming from Heard Island, a mechanism shown for other tracers (REE, ^{228}Ra . . .) (van Beek, P., et al.: Radium isotopes to investigate the water mass pathways on the Kerguelen Plateau (Southern Ocean), *Deep Sea Res. II*, 55, 5-7, 622-637, 2008; Zhang, Y., et al.: Dissolved rare earth elements tracing lithogenic inputs over the Kerguelen Plateau (Southern Ocean), *Deep Sea Res. II*, 55, 5-7, 638-652, 2008) but not for Fe yet. To clarify the overall aim of constructing such a budget, we have added a sentence at the end of the subsection “4. Discussion” (p 11, line 189 -193):

“The main objectives of this budget were to better define the geochemical cycle of Fe during the KEOPS cruise, and notably to explore the role of the TDFe as a tracer of lithogenic inputs coming from Heard Island, a mechanisms shown for other tracers (REE, ^{228}Ra . . .) (van Beek et al., 2008; Zhang et al., 2008) but not for Fe yet. It was also used to refine the C sequestration efficiency”.

Given that a major budget term is that for lateral advection (reportedly from interaction of the waters with resuspended shelf sediments near Heard Island) Chever et al. need to provide better evidence of this rather than just the C1 profile and information from REE. They often use qualifiers when referring to this potential source (lines 15-20 on 6806. Do they have any current meter or ADCP data in the vicinity of C1 to help them come up with physical transports that could be used in conjunction with concentration gradients to make more robust estimates in their budget presented in Fig. 3b.

Our assumption of a lateral transport from Heard Island is supported by the physical results from Park et al. (2008a) (Park, Y.-H., Roquet, F., Durand, I., and Fuda, J. L.: Large-scale circulation over and around the Northern Kerguelen Plateau, *Deep Sea Res. II*, 55, 5-7, 566-581, 2008a). In this study, Park et al. summarized that the KEOPS area appears as a “cul de sac” formed by surrounding strong current systems.

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The flow over the shallow platform in the eastern side of the plateau is consistently northwestward, a direction perpendicular or opposed to the dominant westerlies of the region. This feature is strongly supported by depth-averaged (over the first 500m depth) time-mean currents directly measured by one-year-long current meter moorings at 2 stations (located above the plateau, at 50.2°S/72.3°E and 49.5°S/73.0°E), as well as by repeated LADCP measurements at A3 and C11. Two sentences were added p 5 (lines 74 - 80) to support our assumption of a transport of lithogenic iron coming from Heard Island over the plateau:

“Additionally, Park et al. (2008a) showed that the flow over the shallow platform in the eastern side of the plateau is consistently northwestward. This feature is strongly supported by depth-averaged (over the first 500m depth) time-mean currents directly measured by one-year-long current meter moorings at 2 stations (located above the plateau at 50.2°S/72.3°E and 49.5°S/73.0°E), as well as by repeated LADCP measurements at A3 and C11. A transport of iron from Heard Island to the plateau should thus be possible.”

By far the dominant budget terms are for sediment resuspension and lateral supply of particulate iron. So presumably very small changes in assumptions as to how much PFe sinks out, dissolves, is bioavailable etc could make very large changes to this budget. I think that this warrants some discussion in the text, as does some ranking of the degree of certainty/uncertainty associated with each term in the budget.

Flux calculations in our budget are not established from estimates of Fe dissolution or bioavailability, but on direct measurements of Fe concentrations. We thus think that the mathematical results of our budget allow us to discuss the Fe cycle as we do. However, the large standard deviations of some fluxes make the interpretation of this budget to be taken with caution. As also asked by reviewer # 1 (see above), the importance of each term of this model was thus tested by a Tukey test (SigmaPlot®). Results are discussed p 17, lines 324- 326 (subsection “4.1.3 Dissolved and particulate budgets”):

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“A Tukey test with different “n” values (from 3 to 50) shows that the lateral advection is always statistically different from the atmospheric and diapycnal fluxes ($p < 0.001$) for DFe and Fe_{app}.”

In Fig 3a, some labels in parentheses would be valuable to minimize jumping between text (in two different places (sections 4.1.1 and 4.1.2).

We agree that the reading of Figure 3 may not be easy. We put additional explanation in the caption of figure 3a:

““V” and “S” represent the box volume and the box surface. Letters “d” and “p” refer to the dissolved and the particulate fluxes. “A” represents the atmospheric inputs. “F_w” represents the water flux. “Plateau” represents the mean concentration above the plateau and “C” represents the mean concentration along transect C. “DM” represents the inflowing flux in the surface layer coming from the diapycnal mixing, “WS” refers to the winter stock. “E” corresponds to the net fluxes exchanged between dissolved and particulate fraction. “F” refers to the particle fluxes at the bottom of each box”.

As the authors point out, the very short residence times are indicative of a throughput system – in particular for PFe (1.7 days) which the authors suggest is due to rapid sinking. Surely this will have major implications for the scavenging of dissolved iron?

Indeed, such a dynamic system might have major implications for the scavenging of dissolved iron. This point has been raised with the insertion of a sentence p 19 (line 376-378):

“It should be noted here that such a dynamic system could have implication for the scavenging of dissolved iron which could thus be overestimated in the surface box of our budget”.

Also given such high concentrations of PFe (PFe reported to sink rapidly by the authors), it is possible that by using go-flos to sample PFe they will underestimate PFe concentrations (PFe sinking below the level of the spigots during recovery of the bottles,

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etc). See Gardner, W.D, M.J. Richardson, C.A. Carlson, D. Hansell, and A.V. Mishonov. 2003. Determining true particulate organic carbon: bottles, pumps and methodologies. *Deep Sea Research II*, 50(3-4), 655-674, doi:10.1016/S0967-0645(02)00589-1.

Considering that the length of a Go-Flo bottle is ~ 1.20 m, and that large particles ($20 \mu\text{m}$) settle at 18 m d^{-1} whereas fine particles ($1 \mu\text{m}$) settle at 0.05 m d^{-1} (Ridame, C. and Guieu, C., 2002. Saharan input of phosphorus to the oligotrophic water of the open western Mediterranean. *Limnol. Oceanogr.*, 47(3): 856-869), we estimated that the time needed to reach the bottom of the Go-Flo bottle is 1.6 h and 24 d respectively for large and fine particles. Taking into account the time needed to get back the Go Flo onboard once they are closed and to begin the sampling ($\sim 1-4$ h), an underestimation of Fe concentration of large particles can indeed occur. It is now mentioned in the text p 19 (lines 378-382):

“Additionally, such a short residence time could lead to an underestimation of the apparent particulate iron concentration. Indeed, during the time required for particulate sampling, large particles ($> 20 \mu\text{m}$) could have sunk below the level of the spigots on the Go-Flo bottles which would affect the apparent particulate iron concentration (Gardner et al., 2003).”

There are also a number of inconsistencies in the manuscript, the major one being that on line 25 on p 6820 they report the assumption that the bloom has ended (i.e. indicative of a system in non steady state), but surely if this is the case then they cannot develop the budget presented in Figure 3b which has to assume steady-state in order to solve the four equations presented in section 4.1.1. This is probably the major flaw in Chever et al..

The construction of such a budget is indeed established on the assumption that the system is at steady-state (physical and biological balances). Results of our budget allow us to verify this assumption a posteriori: the E1 flux (corresponding to the biological uptake) is negligible compared to the lateral supply and the biogenic pool only

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represents $\sim 3\%$ of the apparent particulate iron stock, which indicate that the bloom had a small impact on the Fe cycle and that the assumption of a steady-state system is valid. A sentence has been added, p 12, lines 201-203 to explain our assumption of a system at steady state and another p 17, lines 333- 334 to confirm that this assumption is verified:

p 12: “A steady state was assumed to allow the construction of this budget. Results of the calculated fluxes will give information on the relevance of this assumption.”

p 17: “Whatever the state of the bloom, this result confirms a posteriori our assumption of a steady state.”

Finally, in section 4.3 the authors return to the question of C sequestration efficiencies. Given that their value is still 18 fold higher than that during CROZEX can they explore (over and above what they state on lines 5-10 on 6821) whether the iron biogeochemistry from CROZEX might differ fundamentally from their system (see recent paper by Planquette, Statham and others in *Mar. Chem.*).

The careful examination of the methods used to estimate the efficiency published by Blain et al. (668,000) and by Pollard et al. (8,640) shows that the difference resulted from a 10-fold higher export of carbon and a 8-fold lower flux of iron for KEOPS compared to CROZEX. With our new estimate the difference between the iron flux during KEOPS and CROZEX is only a factor 2. Therefore most of the difference results from differences in the excess of carbon export. Base on the data set available during both experiments it is impossible to say if this difference is real or if it results from the different approaches used. In fact when the excesses of C export at 100 m are compared, the factor of difference between KEOPS and CROZEX is only 5. The extrapolation at 200m produces an additional factor 2. This discussion points out the need for a more accurate determination of both terms of the ratio. Very likely, even if not straight forward, progress could be made on the carbon excess determination, e.g. by using similar approaches (direct fluxes measurements by sediment traps, or seasonal C budget).

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The determination of the excess of iron input is a more complicated issue because the fraction of iron that is really relevant for this calculation is the "bioavailable iron". How DFe, TDFe or another operationally fraction compared with the bioavailable fraction is unknown and this might depend on the environment and on the composition of the planktonic community (see lines 477 -483): "Taking into account the lateral supply, the supply of iron was only 2-fold lower during KEOPS than during CROZEX. Therefore most of the difference results from differences in the excess of carbon export. Base on the data set available during both experiments it is impossible to say if this difference is real or if it results from the different approaches used. In fact when the excess of carbon export at 100 m are compared the factor of difference between KEOPS and CROZEX is only 5. The extrapolation at 200m produces an additional factor 2")

Finally in this section, they suggest that the reason why there ratio of 154000 differs from that of 500 (reported for SERIES) is that a large amount of the DFe added to the seawater in such purposeful iron enrichment is rapidly lost from the system. There estimate is > 300 fold greater than that in SERIES suggesting that only 0.3 % of the iron added in SERIES was retained in the upper ocean. Wong et al. (DSR II, 53, 2075-2094 [2002]). They report that on day 6 of the experiment the DFe present represented < 10% of the initial iron addition, but that > 50% of the calculated initial addition was present when all forms of iron were considered (dissolved, colloidal, labile particulate, total dissolved) within the SF6 labelled patch. For this I don't accept their reasoning as to the discrepancy between the C sequestration efficiencies.

We think that our interpretation is correct, because both ratios (154,000 and 500) are calculated using DFe concentrations. During purposeful iron enrichment experiment, iron is added in a dissolved form but it is rapidly transformed in other physical forms, in that sense most of the DFe is lost. It is true that a significant fraction of these forms are present within the patch for a long time. But if we want to take into account this observation for calculating the efficiency ratio we need to know, in both cases (artificial and natural fertilisation), which fractions of iron are bioavailable. In the present status of

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our knowledge this question has unfortunately no answer. (see also comment above).

Clearly all the calculations and discussions on the efficiency ratio are based on the assumption that DFe represents the bioavailable form of iron. We agree with the reviewer in the sense that the numbers and the conclusions might dramatically change if we discover in the future that this hypothesis is falsified (see lines 507 -512:

"This discussion points out the need for a more accurate determination of both terms of the Fe/C ratio. All the calculations and discussion on the efficiency ratio are based on the assumption that DFe represents the bioavailable form of iron, and clearly this assumption impacts heavily on the Fe/C data and calculations of carbon sequestration efficiencies. The numbers and the conclusions might dramatically change if we discover in the future that this hypothesis is falsified".

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