

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

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Received and published: 1 October 2009

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.

Given that a major budget term is that for lateral advection (reportedly from interaction C2302

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.

By far the dominant budget terms are for sediment resuspension and laterals 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.

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).

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? 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, 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.

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..

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.).

Finally in this section, they suggest that the reason why their 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. Their 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.

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Interactive comment on Biogeosciences Discuss., 6, 6803, 2009.