We warmly thank the two referees for their comments. All comments have been addressed here below and in the manuscript when needed.

M. Saito (Referee)

In general I think the manuscript could do a better job of providing context, particularly in the context of ancillary datasets, additional data that cite (Fe, Nd, etc), and I am not entirely convinced of the interpretation of cobalt as a tracer for iron. I think these concerns could be addressed in moderately revised version to produce a nice and very useful study.
We have had ancillary data to further highlight the context of the natural Fe fertilization above the Kerguelen Plateau.
Differences in biological uptake, dissolution kinetics from basalts and solubility of Fe and Co above the Plateau, accounts for the observed strong positive gradient of DCo and a strong negative gradient of DFe in the surface waters of the plateau between Heard Island (station C01) and the core of the bloom (station A03).These concerns are now addressed in a moderately revised version.

One of my substantive concerns is the notion that Co serves as a tracer for Fe. This would be useful if true, but I am concerned that the authors point out that the shallowest station C01 is where the highest iron and other metals were observed, but here while elevated in Co related to the open ocean station, this is not the highest Co station, with A03, A07, and A08 all showing much higher concentrations. The authors explain this with a particulate Co measurement C01, which appears to only be found in the text rather than the figure, and invoke dissolution of lithogenic material. These observations suggest significantly different source functions for the fluxes of Co and Fe (with iron either fluxing from sediments or being dissolved/released from suspended sediments earlier), which makes the connection between elements more difficult. Moreover, because station C01 doesn't show the same dissolved maxima as found elsewhere it seems that there could be other sources closer to the "A" stations rather than the C01 station, which based on examination of the hydrography in Figure 1 seems plausible. Particularly, Co distributions are known to be related to manganese oxides, and perhaps there are exposed regions of the seafloor that could be leaching/dissolving some Mn and Co, is there Mn data for this sample set?
There are two potential lithogenic sources in the area of study, Kerguelen and Heard Islands which have similar geochemical characteristics. In addition to be impacted by Heard Island shelf, the northernmost A transect stations located above the shallow plateau could also be impacted by sporadic water intrusions through the Polar Front that could have been enriched by Kerguelen Island shelf (Zhang et al., 2008) which are more difficult to constrain and understand. Because of the resolution we applied and the complex water mass circulation pattern on the area of study, our dataset do not allow us to further discuss this hypothesis.

Several trace elements including REE, Fe (TDFe) and lithogenic source tracers such as Ra and Nd isotopes all indicated lithogenic inputs at C01 and A03. The lithogenic imprint was even more marked in C01 station (where iron displayed the highest concentration, Chever et al., 2008, Blain et al., 2008) as showed by Nd isotopic composition which was less negative ($-6 \leq \varepsilon_{Nd} \leq -4$) than at A03 station ($-8 \leq \varepsilon_{Nd} \leq -6$) and even much lower than εNd that characterize oceanic waters (εNd~-10, Jeandel et al., 2011).

Moreover, Kerguelen basalts could be characterized by Co concentrations (69.2±8.60 ppm; estimated Co abundance in dredged basalt; Weis et al., 2002) that could be 3 to 5 fold higher than in the continental crust (~15 ppm; Mc Lennan et al., 1995) and that lead us to think that basalt dissolution is the main source of DCo above the Kerguelen Plateau.

As there is no PCo data available at C01, we estimated it from PNd concentration as it has been described p 7298 in the Figure 3 caption. The calculated PCo concentration in the surface waters of A03 ($[PCo]_{estimated}$=1.6±.0 pM; using the same estimation made for PCo calculation at C01) reasonably agree with PCo measurement made there ([PCo]measured=0.85±0.23 pM). This has been added in the discussion.

DMn vertical profiles (Wake and Boye, unpublished) are only available for the A transect and unlike DCo, DMn distributions do not exhibit anomalous maxima in the 0-150 depth layer or in intermediate and deep waters. At A01 and A03, DMn ranged between 0.2 nM at 20 m depth to 0.9 nM near the bottom (500 m depth) and exhibit increasing concentrations with depth. At A07 and A08 stations, DMn vertical distribution did not reflect such increasing concentrations. Even at the bottom DCo and DMn did exhibit contrasted behavior, while DMn is increasing gradually with depth from 100m to the bottom at A01 and A03, DCo only exhibit a single depth higher concentration close to the bottom. DMn and DCo distributions at stations located above the slope also exhibit contrasted behavior; DCo shows anomalous maxima in the 0-150 m depth water layer and increasing concentration with depth while Mn slightly decrease with depth.

Specific comments: Figure 2, the profile figure is too small. Reorient in two rows. Include the data in a table and/or deposit to a data management office. Add temperature, salinity, transmissometry, and nutrient datasets to the study if available.

Scales have been modified on the profiles for stations where DCo was lower (A01, A11 and C01). A11 is now totally distinguishable as it is the only one nutrient-like DCo profiles in this area of study. Profiles have been also re organized in 2 rows (with station located on the plateau in the first row and stations located above the eastern slope on the second row, see Figure 2). In addition, we added O_2 profiles that shows lower oxygen in the UCDW (160 µmol L^{-1}, Figure 2) coincident with high dissolved cobalt at stations A03, A07, A08 and C11, A11 observed in intermediate and deep waters. We also added isotopic Nd datas (εNd) to the DCo profiles of C01 and A03 stations to make the hypothesis of a basaltic lithogenic source near Heard Island and the transport of this lithogenic material to the surface waters of the Plateau more explicit for the reader.

Figure 3 Co budget makes a number of assumptions, some caveats in the text would be appropriate. Using solubility, atm flux, and biological uptake from other studies and regions (and sample types for solubility) will likely introduce uncertainties. Discussion of pycnocline, but no salinity profiles are presented.

Some caveats have been added throughout the discussion. We do not present salinity profiles as the pycnocline is only discussed to divide the water column in 2 boxes for the fluxes calculations.

Additional recent references and discussion of them in this context might be quite useful to the interpretation here. Noble et al., L&O 2012 discusses Co and Fe fluxes from OMZ/coastal environments, and Shelley ... Lohan in GBC in press (if available) discuss solubility relative to iron, and Saito and Moffett 2002 GCA discusses continental shelf source of Co, Ellwood et al 2005 - Co speciation across the polar front, and Saito and Goepfert L&O 2008 for use of Co in polar phytoplankton.

The Noble et al., 2012; Shelley et al., 2012 and Saito and Moffett, 2002 have been added throughout the discussion.

A sediment trap Co flux is discussed, in a caption, but I didn't see mention of it elsewhere.

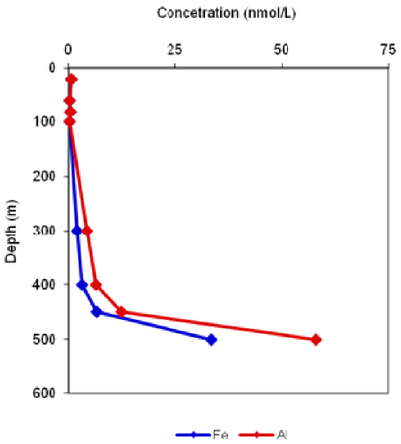
This was a mistake from previous version of this article and it has been corrected.

p7298: interpretation of lateral flux from C01 as the most likely source, despite having a lower dissolved concentration. Seems odd to invoke this when the A# stations are also close to the sedimentary/coastal environment (in fact closer to the islands), couldn't they have a distinct source instead of the advected particulate material?

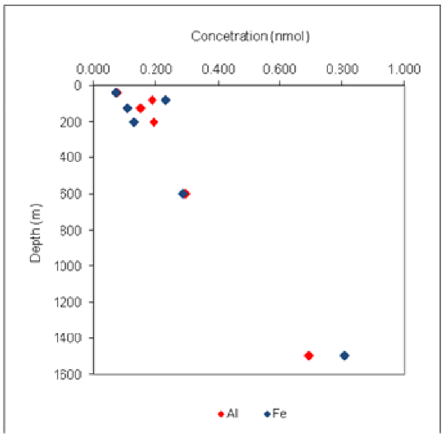
There are two potential lithogenic sources in the area of study, Kerguelen and Heard Islands which have similar geochemical characteristics. In addition to be impacted by Heard Island shelf, the northernmost A transect stations located above the shallow plateau could also be impacted by sporadic water intrusions through the Polar Front that could have been enriched by Kerguelen Island shelf (Zhang et al., 2008) which are more difficult to constrain and understand. Because of the resolution we applied and the complex water mass circulation pattern on the area of study, our dataset do not allow us to further discuss this hypothesis.

Does the particulate material stay constant in abundance (e.g. pAl/L content) while becoming depleted in Co (a sign of dissolution, with the Al presumably being slower to dissolve)?
We have PAl data at A11, C11 and A3 (see figure below). It is elevated at station A3 (especially in deep waters) compared to A11 and C11.

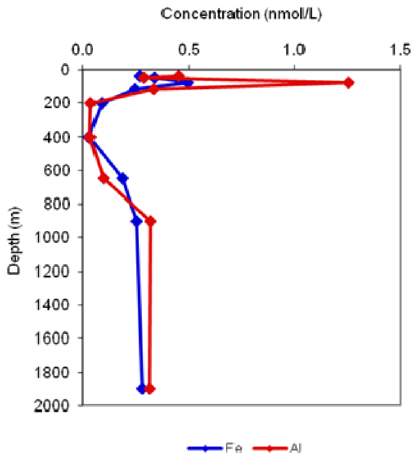
A03:



A11:



C11:



There is no Al data available (and no other trace metals except Co and Fe) at C01 station so it is impossible for us to check if the particulate material stays constant or not in abundance between C01 and A03. However, PNd (Jeandel C., pers. Comm.) and apparent particulate Fe (TDFe-DFe, Chever et al., 2010) indicate strong decrease between C01 and A03, which can suggest particles removal from the water column and/or dissolution of PFe into DFe.
For example at station C01, PFeapp ranges between 300 and 400 nmol l^{-1} and decreases to 1-2 nmol l^{-1} at A03. However DFe also exhibits decreasing concentrations between C01 and A03 in the 0-120 m depth layer (from ~0.8 nM to 0.1 nM) which cannot help to support the dissolution of lithogenic material between C01 and A03 because DFe is strongly removed by biological uptake in the euphotic layer of A03 (in the center of the diatoms bloom).

p7299: One of the arguments is that Co dissolution from lithogenic material occurs in the water column. Why wouldn’t this occur in the sediments as well? Are Co porewater concentrations really so high as to prevent this? Abiotic reduction in the sediment and diffusion of DCo from pore waters can occur and lead to increase DCo in bottom waters at stations A01 and A03 (Figure 2) but these inputs are not thought to reach the 0-150 depth layer over the Plateau.

p7301/7304: the conclusion that lateral advection is much higher than biological uptake seems a bit tenuous if uptake was not measured directly in this study.
DCo vertical distributions only exhibit a nutrient-like profile outside the plateau (A11 station) which contrasts with the profiles shapes described on the shallow plateau which were generally homogenous and impacted by anomalous concentrations maxima (Figure 2). This is not suggesting that DCo is strongly removed from the surface waters by biological uptake.

In general, some brief discussion of Co speciation might be appropriate, particularly if dissolution and biological uptake are discussed.
The organic speciation may enhance both the balsaltic dissolution and the stabilization of Co in the dissolved phase above the Plateau. The organic complexation of DCo would probably promote Co stabilization and its transport in the UCDW (O2~165µmol l^{-1}) above the eastern of the Plateau. These considerations have been added to the discussion.
In general, I think one of the challenges we have with water column datasets of increasing resolution and precision is that we can see features like in this study that are suggestive of sources. But I think we need to be somewhat careful of the interpretation, because water column data doesn’t prove the flux since they are not process studies (they can be consistent with them), as much as they are observations of distributions. While the quantitative efforts are a nice attempt in this study, they are more speculative than the analytical dataset due to the estimations and borrowed values from other regions/sample types as mentioned above.

We agree with M. Saito that the estimated fluxes have to be taken carefully. We believe that better knowledge on trace element exchanges between particulate and dissolved phase, especially dissolution kinetics of trace metals from lithogenic material in seawater, would further narrow the uncertainties of these estimates.

One of the exciting ideas about natural fertilization sources like the Kerguelen Islands is the potential for co-limitation to be avoided because the islands provide other micronutrients beyond Fe. This dataset is one of the first I know of to be able to discuss this with Co data (Martin's Galapagos paper being another perhaps). Including some discussion of this distinction for natural fertilization sources would be very useful.
This distinction has been addressed in the last section of the discussion.