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

Anonymous Referee #1
Received and published: 22 August 2012

General Comments:

Effort should be made to make the discussion clearer, and to flesh it out a bit more. I also found it difficult to read. Many sentences could benefit from removing qualifying clauses and working toward clearer sentence structure.
We have re-worded sentence structure and check the text to make the discussion clearer.

The discussion could be improved by including a figure or two that utilize ancillary data and/or other referenced data to illustrate their points.
Ancillary data (Nd-isotopic composition, macro-nutrients, O2, transmissometry) have now been added.

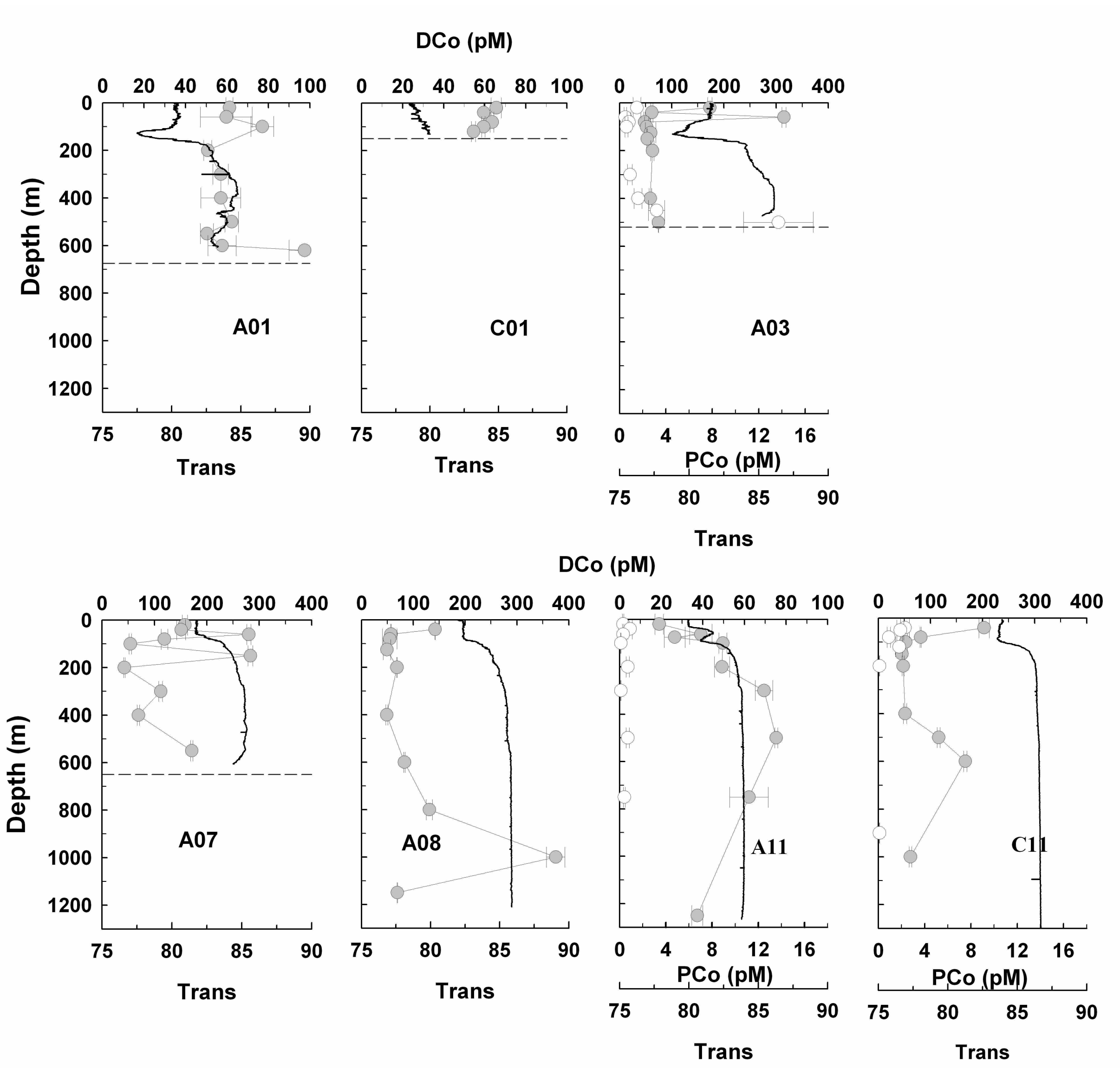
Only two sentences in the methods section are dedicated to describing the dates of the cruise and the primary objective. The Park et al. 2008 reference provides considerable context, but I think a contextual grounding for the study should be present in the manuscript itself and not require outside reading. For example, there is no mention of the Southern Ocean (where the Kerguelen Plateau is located), or the relevance of studying cobalt aside from its potential use as a tracer of lithogenic iron.

The description of the study area is now more detailed, especially with the description of the seasonal bloom together with the circulation around and above the Kerguelen Plateau. The relevance of studying Co as a potential tracer of lithogenic iron is now further described in the introduction.

Some mention of why the cycling of cobalt and iron might be coupled (i.e. they are both affected by redox processes and biological uptake) would also be useful in the introduction, and would connect the first and second paragraphs since the second paragraph focuses solely on iron until the last sentence and the transition between the two is choppy.
We have added a paragraph in the introduction to further highlight the similarities and differences between the Co and Fe cycles.

I think the authors would benefit from adding in ancillary data that might be available from the cruise (i.e. a transmissometry profile showing low transmittance coincident with high cobalt concentrations or oxygen profile showing low oxygen coincident with high cobalt, or low macronutrients in the presence of high cobalt to highlight the excess of cobalt relative to the biological demand), and potentially plotting some of the published Fe, REE, Nd isotope, or 228Ra data that is referenced with the cobalt data. In many cases, other papers from the same study are referenced, but a figure would help illustrate this even better. There did appear to be CTD data in the Park et al. 2008 reference from the same cruise, so perhaps O2 and transmissometry data are available?
As explained above, some of those data have now been added in the figures. For instance the transmissometry shows lowest values at C01 compared to the other stations (see Figure below). This result together with other tracers that indicate a lithogenic origin of the particles at C01 (εNd, REE, Zhang et al., 2008; Ra isotopes, van Beek et al., 2008), and the absence of macro-nutrient depletion at C01 (low biological activity) suggest high density of lithogenic particles at C01. In contrast higher transmissometry at A03 located in the center of the bloom suggest a biogenic origin of relatively lower amount of particles. Low transmissometry signal generally match with DCo inputs that occurred in the 0-200m surface layer (A01, A07, A08)

but at these stations it is difficult to determine whether the particles are from biogenic or lithogenic origin. At C11 in the 0-100m depth layer, low transmissometry is concomitant with higher PCo and Ra activities indicates that a part of the particles could be from lithogenic origin. Transmissometry data are shown in Table 1 and are used throughout the discussion.
In addition, we added O_2 profiles to highlight the lower oxygen content in the UCDW coincident with high dissolved cobalt at stations A03, A07, A08 and C11, A11 observed in intermediate and deep waters. We also added isotopic Nd data to the DCo profiles of C01 and A03 stations to further support the hypothesis of a basaltic source near Heard Island and the transport of this lithogenic material into the surface waters of the Plateau.



The authors derive a cobalt budget, but there is no discussion of this budget. What are the implications or importance of their findings? Do the authors envision that cobalt will always be a good tracer of lithogenic input of iron? Is the system around the Kerguelen Plateau similar to any other systems that could drive future research in this area or be a model study for another area?
We did not dedicate a section to specifically discuss the budget, as fluxes are already used to strengthen the arguments described along the discussion. These fluxes have to be taken

carefully due to the estimations and borrowed values from other regions and different sample types.

In the case of the Kerguelen Plateau we are able to use cobalt to quantify iron input from the plateau because the biological uptake of Co by diatoms (which is potentially the main removal term for Co) is estimated to be much lower than the main source term (lateral advection of Co enriched surface waters). The annual recurrent bloom is dominated by diatoms (Armand et al., 2008) which have been shown to require much more Fe (and Zn) than Co, and that is why iron remain low in the surface waters of the shallow plateau (Chever et al., 2008, Blain et al., 2008). If similar configuration (high sedimentary inputs of both Fe and Co into a diatom bloom) occurs in oceanic and/or coastal waters, then it is well conceivable to use DCo as a tracer of DFe source. To further constrain the Co budget it will also help to further study the solubility and kinetics of Co from different sediments, as the chemical dynamics between the dissolved and particulate pools.

Specific questions/comments:

In section 3.1, the authors initially describe the range of profile distributions observed. Some of the profiles have 1 data point maxima and I wonder if the complexity of the profiles could be supported by transmissometry data or dissolved oxygen data, especially if the source is attributed to dissolution. As mentioned earlier, a figure showing overlap of similarly strong signals in other species would go far to support the sedimentary source argument.
This is now added in the figures and discussion.

On pg. 7296, there is reference to an oxygen minimum in Upper Circumpolar Deepwater - how low does it get? The UCDW is characterized by a 160-170 µmol L^{-1} O_2 core. This characteristic has been added in the text, and O2 vertical distributions have been added on Figure 2.

The Heggie and Lewis reference later in the manuscript is careful to describe that the mechanism by which cobalt can be dissolved from manganese oxides is dependent upon low O2. While a sedimentary source appears to be a reasonable claim, does the O2 get low enough for reductive dissolution to be a reasonable mechanism?
We agree with the referee, and we have now added this discussion in the revised manuscript.

On page 7296, there is mention of previous works that have suggested sedimentary inputs of cobalt. A recent study by Noble et al. (2012 Limnol. Oceanogr. 57(4) 989- 1010) may be a useful reference in support of this shallow shelf sedimentary source of cobalt and iron. This reference has been added throughout the discussion.

I wonder if biogenic cobalt or iron has been considered. There is a description of basaltic weathering and lithogenic sources, but I wonder if the biogenic particles have been taken into account? Is this included in their estimates? This might be tricky given the productive region here that is naturally iron fertilized. How much of the production could be coming from recycled iron with resuspension and remineralization of sinking biogenic particles? Could this be an important factor given how shallow some of the waters are along this plateau?
In the present study only the gross Co uptake have been estimated, Co net uptake rates can be estimated from Co net seasonal depletion but the latter cannot be calculated here because Winter Waters are impacted by several DCo anomalous relative maxima. It is thus not possible to take into account biogenic particle dissolution.

In the plots in Figure 2, while the difference between the open ocean station and the plateau station are referenced as significant (pg 7298 lines 6-9), it is difficult to see this in the profiles. Perhaps the data could be plotted with a break in the x-axis? In its current form, the profiles from C1, A1, and A11 look more or less indistinguishable except for the deepest sample, though A1 and C1 are over the plateau.
We modify the scales for stations where DCo was lower (A01, A11 and C01). A11 is now totally distinguishable as it is the only one with a nutrient-like DCo profile 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).

On pg 7298, there is a discussion of particulate cobalt, however - only as estimated by making assumptions and using other species. Have the authors tested their estimation of particulate cobalt against the particulate profiles that they had measured to see if the assumptions were sound?
We thank the referee for this idea. We have now calculated PCo concentration in the surface waters of A03 ($[PCo]_{estimated}$=1.6±1.0 pM; using the same estimation made for PCo calculation at C01), which reasonably agrees with PCo measured at this station ([PCo]measured=0.85±0.23 pM). This has been added in the discussion.

It seems to me that lability of the cobalt in the weathering process may be of concern in determining what may or may not be released in dissolved form to the water column. I was also confused as to whether or not remineralization of biogenic material that may have settled would be accounted for. Presumably this would not have the same Nd signature as weathering basalt. I am unclear about what then constitutes a sedimentary source.
Several tracers (Ra and Nd isotopes and REE; TDFe,DFe) evidenced a lithogenic source near Heard Island (station C01). This lithogenic material is probably made of basalts as C01 sediments are composed of coarse grained basalts (in contrast, the sediments are mainly composed of diatom frustules above the Plateau). The basaltic nature of the lithogenic material is supported by Nd isotopic composition at C01 which is more negative than Kerguelen basalts (εNd~-2, Weis et al., 2002; Doucet et al., 2005) and ranges from -6 to -4, compared to -8 to -6 at A03 and -10 in oceanic waters. As C01 is shallow (150 m depth), basalts dissolution can impact the whole water column and the anticyclonic circulation above the plateau allows the lateral transport of this enriched water layer from C01 to the surface waters at A03. The lithogenic source considered in the present study is thus basalts of The Heard Island shelf.

On pg 7298, line 8 - what is meant by "whole water"?
The sentence is now corrected.

On page 7299 line 13, an estimated loss of PCo of 995+/-905 nmolM^{-2} is reported. I wonder how useful this estimate is.
This flux it is supporting the idea of PCo “removal” between C01 and A03 stations. PCo dissolution into DCo can be a first mechanism that explains the decreasing of PCo concentrations between A03 and C01 while particle aggregation and sedimentation could also be another sink for PCo between these two locations.

On page 7299, last paragraph, the authors claim that two stations have "comparable biological activity". Are there references that support this that could be mentioned, like PP rates or chlorophyll?
Computed daily primary production rates integrated within the euphotic zone for the total phytoplankton community are in the same range at these stations (between 0.60 and 0.80 g C m^{-2} j^{-1}; Uitz et al., 2008). This has been added in the discussion.

In section 3.1.2 (lines 5-9), there is discussion of the release of dissolved cobalt from particulate matter due to the reduction of manganese oxides and release of dissolved metals at the sediment-water interface. As mentioned earlier, the Heggie and Lewis paper is careful to articulate that low oxygen concentrations are crucial in determining whether metals are being deposited/oxidized or reduced and released into the water column. This should be made clear and/or taken into account in the discussion here. See answer above.

Typographical comments:
pg 7295 line 25: depending on stations location - should be "station" corrected as suggested
pg 7297 line 21: DCo inputs are occurring in the intermediate and deep waters that flows along - should be "flow" corrected
pg 7298 line 16: Hence the lateral advection from C01 is the most likely processus explaining - should be "process" corrected
pg 7299 line 4: is possible and consistent to physical observations - "to" should be "with" corrected
pg 7299 line 16: particles aggregation - should be "particle" corrected
pg 7299 line18: between C01 and A03 is supporting the dissolution hypothesis. - reads more clearly as "A03 supports the dissolution" corrected