Reviewer 1:

This manuscript shows the concentrations of dissolved (<0.22 µm) Fe in the water column (up to 1300 m) of the Kerguelen Island (covering coast to offshore waters). The authors attempt to explain the high variability of Fe concentrations found in this part of the southern ocean. Although dataset is valuable I think that the authors do not provide enough insights to demonstrate the sources and reasons for the variations of Fe concentrations. Although most of the hypotheses presented could be perfectly valid, they are hardly demonstrable with the data presented. For example, the higher concentrations measured close to the seafloor are justified by resuspension of sediments and porewater release, however other potential sources such as hydrothermal vents existing in the area are not considered in this case. As indicated by the authors since particulate Fe concentrations were not measured it makes difficult to confirm this hypothesis. Although I agree that biological uptake was probably the main responsible of the temporal decreasing of Fe above the plateau, other aspects influencing the concentration of Fe such as the presence of krill and/or whales (eg. Tovar-Sanchez et al. GRL 34 L11601, 2007; Nicol et al. Fish and Fisheries 11, 2010) should be, if possible, considered or discarded. Authors include atmospheric inputs as additional source, however the study only includes backward trajectories air masses without providing any chemical aerosol measurement. In the conclusions section the authors state that atmospheric inputs were negligible during KEOPS2 cruise however this paper does not present any data that confirm this fact. In summary, I believe that the authors present a valuable data set that could provide important information about the biogeochemical cycle of Fe in this part of the southern ocean, however I think that additional data (some of them are presented or under evaluation in separately papers as part of the special Issue) are necessary to support the main findings presented here.

We thank reviewer 1 for his/her review.
This ms is not trying to present a full iron biological cycle for the KEOPS2 experiment; rather the dFe distributions and the reasons for the concentration variability. Dissolved Fe data presented in this paper together with particulate Fe data from a closely aligned companion study (van der Merwe et al., 2015) are combined by Bowie et al. (2014) in order to establish short-term Fe budgets at three sites (above the Plateau, in the recirculation area, and the HNLC area). The three papers should be read as an Fe collective whole.
We made a substantial effort to cite data from papers from the special issue, in particular, concerning sedimentary inputs and particulate Fe data close to the seafloor. For example: “This is corroborated by high pFe values at A3-1 and A3-2 (30 and 15 nmol L-1 respectively) and pFe:pAl ratios that resemble basalt over the Kerguelen Plateau (van der Merwe et al., 2015)“ or: “Moreover, close to the seafloor, van der Merwe et al. (2015) observed high values of pFe, pMn, and pAl, likely due to sediment resuspension.”
To support our hypothesis, lithogenic Si data are also highly cited throughout the manuscript (Lasbleiz et al., 2014).

In the following, we address the concerns of Reviewer 1 in terms of demonstrating the various sources:
Hydrothermal source: we agree that hydrothermal events are expected in our study area, more particularly in the vicinities of the Heard Island and the Leclaire Rise. Heard Island is

located further South of the studied area and dFe inputs from this source could be advected to the Plateau area. The following sentences were added to the text:
"Hydrothermal input may be an additional Fe source above the Kerguelen Plateau more particularly in the vicinity of the Heard Island. The Mn:Al ratio at this station is much lower than any of the other stations 0.007-0.009 (van der Merwe et al., 2015) and very similar to the Kerguelen Island Basalt mean of 0.004-0.010 (Gautier et al., 1990). This supports fresh weathering of basalt downstream of A3, which may be glacial/fluvial runoff or hydrothermal."
At station R-2, both hypotheses were considered in van der Merwe et al. (2015) for the high Mn:Al ratios. We considered that the enriched Mn at R-2 could be due to either MnO2 enrichment in the surface sediments during redox cycling of early diagenesis (Planquette et al., 2013), or supplied via a Mn enriched source such as hydrothermal venting near the Leclaire Rise. The extremely low carbon content of the sediment at station R-2, as evidenced by its near white colour, low diatom content (L. Armand, pers. obs., 2012) and low carbon export flux (Laurenceau et al., 2014; Planchon et al., 2014), suggests that MnO2 enrichment in the surface sediments during redox cycling is more likely at R-2.

Krill/Whale: It is true that Antarctic krill eat diatoms and recycle iron in surface waters when feeding. Baleen whales eat krill, and defecation by baleen whales could be a major mechanism for recycling iron, if whale faeces contain significant quantities of iron (Nicol et al., 2010, Southern Ocean iron fertilization by baleen whales and Antarctic krill. Fish and Fisheries, 11: 203–209. doi: 10.1111/j.1467-2979.2010.00356.x, Schmidt et al., 2011, Seabed foraging by Antarctic krill: Implications for stock assessment, bentho-pelagic coupling, and the vertical transfer of iron, Limnol. Oceanogr., 56 (4), 1411–1428). Nicol et al. (2010) demonstrated that krill can act as a long-term reservoir of iron in Antarctic surface waters, by storing the iron in their body tissue. Populations of whales and krill can then store larger quantities of iron and also recycle iron in surface waters, enhancing overall ocean productivity through a positive feedback loop. Schmidt et al. (2011) suggested that due to their large biomass, frequent benthic feeding, and acidic digestion of particulate iron, krill might facilitate an input of new iron to Southern Ocean surface waters. However, as we do not have any data concerning these processes, we did not consider them in our manuscript.

Atmospheric inputs: this was also addressed by Reviewer 2. Therefore, we have modified substantially the paragraph in pages 245-246 and modified the conclusion. Please refer to our answer 2 to Reviewer 2.

"As indicated by the authors since particulate Fe concentrations were not measured it makes difficult to confirm this hypothesis."
Particulate Fe was measured and reported in a companion paper. We cite the van der Merwe et al. (2015) article throughout the manuscript, so we do not understand the reviewer's comment.

Reviewer 2

- ***General Comments:***

1. The general presentation of the paper is good but it is sometimes hard for the reader to follow the discussion. This could be ameliorated by using a unique color code used on the different figures. On figure 1, a different color could be attributed to each cluster for the stations of KEOPS-II. This color code could then be used on figure 2, figure 3, figure 4, figure 6, figure 7 and figure 9. Within the different stations of a cluster, a different shade of color could be used for each station.

We thank reviewer 2 for his/her review.

We agree with the reviewer that consistent colour codes through the figures would be a useful improvement and we have changed the figures accordingly. Cluster 1 is yellow-orange, Cluster 2 is green, Cluster 3 is blue-violet, Cluster 4 is brown, and Cluster 5 is grey. Figures 1, 2, 3, 6, 7, and 8 follow the same colour code.

2. For the recirculation area (cluster 3), on page 244 and 245 of the manuscript, the higher surface concentrations at station TEW-4, E-4W-2 and E-3 could, according to the authors, be due to atmospheric deposition. I think this hypothesis is to hazardous to be mentioned in this manuscript: First of all, the surface maximum at station TEW4 is very relative : 0.17 _ 0.02(SD) at 40 m, 0.15 _ 0.01(SD) at 70 m and 0.20_ 0.01(SD) at 100m. Concerning E3 and E4W2, even if a small dFe increase is observed, the arguments given by the authors are not very convincing. Even if the air masses over the sampled stations have traveled over the Kerguelen Island on the day before dFe sampling (which is not true for the trajectory at 10m), there is no evidence that a significant amount of dust has been emitted in the atmosphere on Kerguelen this day. Moreover, even if the Kerguelen island could emit limited quantities of dust, it is certainly not enough to increase the dFe concentration to 0.38 nM at station E3. Finally the authors give the argument that no pAl increase has been observed. In consequence, I believe that figure 8 is not supporting the discussion (and could be removed from the manuscript) and that it is impossible to attribute the surface increase in dFe to dust deposition.

The reviewer makes a good point and concurs with Reviewer 1. It is true that we are lacking supporting data concerning atmospheric inputs and that the discussion was too speculative. We have therefore removed the dust deposition argument and modified our statement in these pages to: “The higher sea-surface dFe concentrations at stations TEW-4, E-4W-2, and E-3, may be indicative of atmospheric inputs. However, no particulate aluminium (pAl, a proxy for atmospheric inputs) surface enrichment in the recirculation area was observed during the study (van der Merwe et al., 2015), suggesting that air-masses were not carrying enough aerosols to enhance pAl surface concentrations. Moreover, Bowie et al. (2014) showed that atmospheric inputs are in the order of 50 nmol m^{-2} d^{-1} which is insignificant compared to the lateral supply of dFe in the same area 180-2400 nmol m^{-2} d^{-1}) . “

Figure 8 was deleted from the manuscript.

3. Figure 6 presents vertical profiles of dFe concentrations on the left panel and the associated beam attenuation from the CTD on the right panel for stations of Cluster 1, Cluster 2 and station E-4W-2 of cluster 3. In its present form, this figure is difficult to read because the same orange color is used for different clusters. I would recommend to split the

figure in different panels for each cluster. The fluorescence data from the CTD could be added to discuss the decrease of dFe in cluster 2 which is linked to biogenic particles.
We have changed the colour code in fig 6 (see above), which makes the figure easier to read. We also add in figure 6 a third panel with Chl*a* concentrations, from Lasbleiz et al. (2014).

- ***Detailed Comments:***

p 233, L19-26 : In this introductory paragraph, an important number of references are cited to support some general and somehow trivial assumptions. The number of references cited could be reduced to cite only the most important work. For example, concerning atmospheric deposition, Jickells et al. 2005, Wagener et al. 2008 and Heimburger et al. 2013 are cited whereas citing Jickells et al. is enough to describe the importance of the atmospheric source at the global scale. The same is certainly true for the other sources cited.
In the introduction, we have shortened the number of references that describe the major sources of dissolved iron in the ocean. However, we decided to keep in all the references that referred to relevant studies in the Southern Ocean.

P234 L11: Please replace "held in late summer 2005" by "held in late austral summer 2005".
Change has been made as suggested.

P234 L22: Blain et al. 2007 is cited to present the KEOPS-2 cruise whereas this paper concerns the KEOPS-1 cruise. A introductory paper to KEOPS-2 would be more adapted here.
At present, there is no introductory paper for KEOPS2, so we have deleted this reference.

P234 L27-28: At the end of the introduction, a short section should be included in order to better explain how this article is articulated with the two other Fe papers of the special issue.
In order to better present the paper in the context of the special issue, the statement "The combined suite of KEOPS2 Fe results will be presented in two other papers in this special issue (van der Merwe et al., 2015; Bowie et al., 2014)" has been replaced by:
"Finally, dFe data presented in this paper together with particulate Fe (van der Merwe et al., 2015) are combined by Bowie et al. (2014) in order to establish short-term Fe budgets at three sites (above the Plateau, in the recirculation area, and the HNLC area). The combined suite of KEOPS2 Fe results will be presented in two other papers in this special issue (van der Merwe et al., 2015; Bowie et al., 2014)". The three papers should be read as an Fe collective whole.

For those who are not familiar with the TMR Model 1018 Rosette (which is my case), it would be helpful to have a short explanation on how the sampling depths are estimated. This is important because some dFe data are plotted against the "distance to the bottom".
The depths are defined before the TMR deployment and a pressure sensor allows the bottle to close at the right depths. Bottom depths are taken from the CTD data. This information was added to the Figure caption.

P 236 L8: What does a "Representative" ammonium acetate buffer means?

We were refereeing to “representative blanks” but we agree that it is not the right term and have removed this adjective.

P236 L24: The T-S diagrams of this manuscript are plotted with practical salinity (practical salinity scale) and potential temperature. The authors are certainly aware that since 2010, TEOS-10 was adopted by the Intergovernmental Oceanographic Commission to replace EOS-80 as the official description of seawater and ice properties in marine science (Wright et al. 2010, Spall et al. 2013, Valladares et al. 2011). In consequence in scientific publications, pratical salinity should be replaced by absolute salinity and conservative temperature should replace potential temperature. There is no doubt that these changes will not affect at all the conclusions of the present manuscript. I only recommend the authors to follow these new guidelines.
We take good note of these recommendations for future publications. However, since all the papers of the special issue are using potential temperature and practical salinity, and given the fact that the reviewer acknowledges that such a modification will not affect our conclusions, we decided to keep these units.

P241 L12. The reference to (Fig. 2) is not correct. Figure 2 are T-S diagrams
We agree and now only refer to Table 1.

P248 L22: The conclusion “The atmospheric inputs were negligible during KEOPS II”, which is certainly true, is not at all supported by the discussion in the manuscript and should be removed.
We have removed this sentence as we neither discuss the atmospheric inputs anymore nor show air mass back trajectories (original Fig. 8 has been deleted).

P263 Figure 2: In the figure 2 legend, station E2 is cited two times for cluster 3.
We thank the reviewer for noticing this and E-2 is now cited only once in the figure caption.

P265 Figure 4: I do not understand the reason to plot the median value with the interquartile range. I believe that this figure would better support the discussion if all profiles for a cluster were plotted.
At first, we plotted individual profiles for each cluster on the same figure. Given that these profiles are more or less similar, we decided to take the approach of plotting the median with the interquartile range to further support these similarities and make the figure easier to read. This type of plot also allows us to discuss water mass contents of dFe.