RESPONSE TO REVIEWER S. BLAIN

The manuscript presents and comments on a new database of dissolved iron concentrations in the Southern Ocean. The data set is significantly improved compared the previous one and this allows to draw interesting new features on the spatial and temporal variability of DFe distributions in this ocean. In a general manner this is a very useful work that certainly deserves publication after a few issues have been addressed:

> We thank Stephane Blain for his review and general support for our manuscript. We present our response in bold and preceeded by '>' in case of formatting errors.

Section Methodology: To synthesize the data set the authors defined different regions in the Southern ocean. The frontiers between the different basins (longitude) are arbitrary but this is not critical. More important are the definitions of the different regions based on latitudes. In the manuscript the Southern ocean is defined as the region south of 40°S. This definition is based on the argument that 40°S is roughly the position of the Subtropical Front (STF). This imprecise definition contrasts with the attention paid to the definition of the frontier between the Antarctic and the subantarctic regions that is based on the position of the polar front which is highly variable as correctly mentioned in the manuscript. If one looks carefully at the position of the STF it is also highly variable (e.g. 41-42 at 40°E or at 5°E, but 45°S near Kerguelen and 45°2 at 140°E (SR3)). The data of SR3 are largely commented in the paper. Due to the wrong position of the STF in this region, data have been included in the subantarctic data set whereas they really belong to the subtropical region. This point should be corrected or at least commented in a revised version of the manuscript.

> The reviewer raises an important issue. While the STF does indeed show variability in its latitudinal position, it is not as clearly defined from sea surface height across the entire Southern Ocean region as is, for example, the Polar front. Nevertheless, the reviewer is correct that the STF is around 45degS at SR3, which is significantly different to our 40degS assumption. Thus while we have retained our general 'separation point' of 40degS for the Southern Ocean, we have modified this to 45degS on the SR3 section for our seasonal analysis.

The definition of the depth ranges is not soundly argued. What is the rationale for defining limits at 100, 500, 1000 and 2000m? (in the Antarctic region 200m (winter mixed layer depth)is may be a more interesting frontier than 100 and 500m).

>Our rationale for the depth ranges was based on the two-fold objective of separating depths of interest while maximising the number of observations in each depth bin in order that a representative profile of dFe could be drawn. Our first depth bin (0-100m) was chosen to represent the surface layer where one might imagine dFe depletion due to biological activity, we therefore wanted to retain the summer MLD within this bin while also retaining enough dFe data for our analyses. Therefore in that sense, the 100m horizon is a compromise such that it can be considered to be 'shallow enough' to reflect the region of dFe depletion and 'deep enough' to include enough data. The 100-500m depth bin was then chosen to represent the subsurface reservoir that would be isolated from upper levels during summer but potentially impacted by winter mixing. Of course this subsurface bin (100-500m) may exceed winter MLDs in the Antarctic zone, but MLDs are deeper than 200m in winter in the sub-Antarctic zone. Our choice again reflects a compromise that allows a consistent depth range for the entire Southern Ocean, but is of course somewhat arbitrary. We have amended the manuscript to include our

rationale.

In the manuscript the regional and temporal variability of DFe concentrations in the surface water is largely discussed in relation to the biomass of phytoplankton (derived for satellite images). In this context the depth of the mixed layer could be a better choice than 100m (see below). It is possible that the data base compiled by the authors does not allow to easily estimate the MLD. However, if it is the case it should be a recommendation of the paper that future iron data bases include a few other interesting parameters such as MLD and may be others: Concentrations of major nutrients...

> The reviewer is correct that MLD-averaged dFe might be more representative, but as he notes we did not have enough metadata associated with the historical dFe measurements (which then set the benchmark for what was included subsequently). We hope that with the intermediate GEOTRACES data product that a holistic dFe-metadata dataset will permit such investigations in the future. We have amended the manuscript to emphasise the importance of metadata.

Case study SR3: This is a very interesting section addressing for the first time the seasonal cycle of DFe in the Southern ocean. In addition to my comment above I have the following question/comments. Why using weekly chlorophyll to compare with monthly iron data?

> We chose to use weekly chlorophyll since it allowed for a more detailed representation of the seasonal cycle. The general patterns do not change if monthly chlorophyll is used.

From line 24 page 11501 until line 7 page 11502. This section should be move to discussion. In addition I am not very convinced by this discussion for two reasons: The first one is that the discussion is based on mean values of DFe and Chla in a surface layer, with constant depth 0-100m, and not in the mixed layer. The effect of deepening and shoaling of the mixed layer has a strong impact on the Chla and DFe concentrations. This should be mentioned and commented. The second reason is that the role of biology is not only to consume dissolved iron, but also to mediate the transformation of particulate iron into dissolved iron (see detailed comments in the review of P. Croot). Therefore I do not think that the conclusion "the major driver of DFe variability is not biological activity but rather exogenous input and or/ ocean circulation "is really supported by the analysis of the data set. At least alternate hypothesis should be presented.

> The reviewer is correct that dilution/concentration effects can be important and also is correct to draw attention to the role of biological activity in transforming dFe. Nevertheless, we do note the very deep ferriclines (down to 500m) in the Southern Ocean from recent IPY-GEOTRACES cruise (Chever et al., 2010; Klunder et al., 2011), which are decoupled from major nutrients and hydrography. Thus the impact of MLD variations on Fe could be very different to their impact on Chla. We of course, had only considered biological uptake of dFe in the original manuscript. Thus we will qualify our statements to include the potential importance of MLD variations (although these are alluded to as part of 'ocean circulation', they will be mentioned explicitly), as well as biological mediated iron transformations. It is therefore plausible that high dFe associated with high Chla could be due to intense dFe recycling as an alternate hypothesis. These issues have been included in the revised manuscript.