

Interactive comment on “Natural iron enrichment around the Antarctic Peninsula in the Southern Ocean” by M. V. Ardelan et al.

Anonymous Referee #2

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The manuscript by Ardelan et al. presents new Fe data in the South Shetland Island-Antarctic Peninsula region. Dissolved Fe (filtered samples) and TaLFe (unfiltered samples) were analysed using a method coupling a chelex-100 resin and a HR-ICP/MS. DFe and TaLFe concentrations showed an enrichment at the inshore stations and in the Bransfield Strait. The lowest concentrations were observed in the Drake Passage. The authors discuss these variations with regard to the hydrography and surface transports in their studied area. They highlight natural iron enrichments, as well as the importance of the mixing between the Fe enriched-water from the Weddell Sea with the Fe-poor waters from the Drake Passage. The relation between DFe and TaLFe is not linear suggesting a chemical control of DFe, including adsorption/desorption onto particles. The TaLFe:DFe ratios suggest that TaLFe settles out of the euphotic zone faster than DFe. The authors estimate the iron supply needed to sustain the new primary produc-

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tion (PP) of the Drake Passage and Bransfield Strait. Compared to an estimation of the lateral DFe supply using Fe to 228Ra ratios, their estimation of new PP could be largely sustained by this lateral supply. This work is essential since it provides new data in a key area: the Antarctic Peninsula in the Southern Ocean. This region is of high importance since it can supply Fe along the Antarctic Circumpolar Current as far as in the Indian Ocean. However, one of my main concerns is about the calibration of the method. The authors calibrate their method with the NASS-5 standard. Fe concentrations in this standard are much higher than the ones in the open ocean (3.7 nM vs. < 0.1 nM). The SAFe standard, for example, now allows calibration of analytical methods with low Fe concentration seawater (surface sample: 0.097 ± 0.043 and deep sample: 0.91 ± 0.17 nmol L⁻¹, Johnson et al., 2007). I think the authors should analyse these standards with their method. Moreover, the article would benefit from more concision. This manuscript should be suitable for publication in Biogeosciences after revisions.

Specific comments:

Abstract: p 7482, line 4: when Fe concentrations are measured on acidified unfiltered samples, this fraction is more generally called “Total Dissolvable Fe (TDFe)” instead of Total acid-leachable Fe (TaLFe) (e.g. see Löscher et al., 1997).

1. Introduction: p 7482, line 23: for the high Chl a concentrations above the Kerguelen Plateau, the authors can also refer to Blain et al. (2007). They can also mention the Crozet plateau (Pollard et al., 2009).

p 7483 line 1-2: “Conversely, elevated chl-a concentrations in coastal and plateau areas are thought to be the result of re-suspension of iron-rich sediments and subsequent upwelling...”. Advection of water masses is also an important process to transport over long distance iron, which can be available for phytoplankton (e.g. Lam and Bishop, 2008).

2. Materiel and method p 7484, line 11: replace 2.1.2. by 2.5.

p 7486, line 3: replace “throught” by “through”. Line 4, replace “filteation” by “filtration”.

p 7489, line 9: replace “tbe” by “the”

p 7489, lines 17-20: “When the averages for temperature, chl-a, nitrate, phosphate, DFe, and TaLFe in the UML at the 16 stations are plotted against salinity (Fig. 5), the data for individual stations within each group (Table 2) are fairly close with each group distinct from the other groups.” I do not find this sentence very clear.

p 7490, lines 15-16: “Concentrations of TaLFe, which belongs to in the particulate fraction increased more rapidly than concentrations of DFe with increasing density”. Rewrite this sentence

p 7490, lines 16-20: these results have already been described earlier.

p 7490, lines 18-20: “The ratio of TaLFe/DFe decreased from ~ 60 in shallow waters (90 m) close to shore to ~ 9.4 in deep water (1200 m) 50km from the shore (Fig. 8).” This is not what is shown on fig. 8.

p 7495: the authors here estimate the Fe:C ratio using two methods with nitrate but only one with phosphate. Why was the method $i\text{Fe}:i\text{DNO}_3$ not used with phosphate?

p 7495 line 23: change “obtained” by “obtain”

Fig. 4: without the ln scale, it would be easier to check if the DFe and TaLFe profiles are oceanographically consistent or not.

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