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Biogeosciences Discussions

Interactive comment on "A global compilation of over 13 000 dissolved iron measurements: focus on distributions and processes in the Southern Ocean" *by* A. Tagliabue et al.

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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: 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 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). 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 parameter such as MLD and may be others: concentrations of major nutrients... Section results: 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? 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 mentionned

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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.

Interactive comment on Biogeosciences Discuss., 8, 11489, 2011.