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Biogeosciences Discussions

Interactive comment on "High variability of dissolved iron concentrations in the vicinity of Kerguelen Island (Southern Ocean)" *by* F. Quéroué et al.

Anonymous Referee #2

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This manuscript presents a dataset of oceanic dissolved iron concentration (dFe <0.22 μm) in the Southern Ocean (Indian sector, vicinity of Kerguelen Island). Samples have been collected during the KEOPS-II oceanographic cruise. This is a valuable dataset which clearly deserves publication in the KEOPS-II special issue. To discuss the dataset, the different vertical profiles are organized in five clusters based on T-S diagrams of the water column. This approach is valuable to discuss the results in terms of external and internal iron sources. However, the discussion of the results is too dense and sometimes hard to follow. To my opinion, the manuscript deserves publication after some substantial changes that are listed below.

General Comments:

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Interactive Discussion

Discussion Paper

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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.

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 $\pm$ 0.02(SD) at 40 m, 0.15 $\pm$ 0.01(SD) at 70 m and 0.20$\pm$ 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.

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.

Detailed Comments:

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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.

P234 L11: Please replace “held in late summer 2005” by “held in late austral summer 2005”.

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.

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 the two other Fe papers of the special issue.

P236 L1: 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”.

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

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.

P241 L12. The reference to (Fig. 2) is not correct. Figure 2 are T-S diagrams

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.

P263 Figure 2: In the figure 2 legend, station E2 is cited two times for cluster 3.

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.

References

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Wright, D., R. Pawlowicz, T. J. McDougall and R. Feistel, 2010: Progress Report for the SCOR/IAPSO Working Group 127 on "Thermodynamics and Equation of State of Seawater." CMOS Bulletin SCMO, 38, No. 3.

Spall, M. A., K. Heywood, W. Kessler, E. Kunze, P. MacCready, J. A. Smith, K. Speer,and M. E. Fernau, 2013: Editorial. Journal of Physical Oceanography, 43, 837.

Valladares, J., W. Fennel, E. G. Morozov, 2011: Anouncement: Replacement of EOS-80 with the International Thermodynamic Equation of Seawater-2010 (TEOS-10).

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Interactive Comment

Deep Sea Research, 58, 978.

Interactive comment on Biogeosciences Discuss., 12, 231, 2015.