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Interactive comment on “Fractionation of iron species and iron isotopes in the Baltic Sea euphotic zone” *by* J. Gelting et al.

Anonymous Referee #1

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Review

Title: Fractionation of iron species and iron isotopes in the Baltic Sea euphotic zone Authors: J. Gelting, E. Breitbarth, B. Stolpe, M. Hassellöv, and J. Ingri In: Biogeosciences Discuss., 6, 6491–6537, 2009

General comments:

This paper presents a study of the spatial and temporal distribution of iron species in coastal brackish water systems. The topic is interesting. By using a multiple speciation technique the authors have been able to examine different iron species in the water column, based on physical and chemical properties. I think that the results have the potential to make a scientifically important paper. I recommend some minor changes to

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improve the MS. It would also be informative and strengthen this paper greatly if some comparison with previous studies of iron speciation in coastal waters was made.

Specific comments: : 1- Is it really necessary to use the stations codes such as C3, BY31 BY15? The name of the studied zones or their acronyms are good enough to follow the study sites.

2- While the water is waiting for cross filter filtration (5 to 24 hr, did you expect any changes in Fe speciation?

According to Chin et al., (1998) gels re-form from soluble and colloidal precursors after previous gels are removed by filtration, upon filtration new colloids form as a consequence of re-equilibration in the system. It means that in the filtrated samples, in time, colloid aggregation will start and samples will contain particulates again. The authors should discuss this point in the discussion section.

3- The description of the method in the MS is insufficient in some points to understand the method performed. I would recommend a slightly more detailed description of the sample preparations. For example: How the authors determined TFe, and DFe? Was total Fe determined from un-filtrated samples? If so can ICP-MS technique 'determine all Fe in the un-filtrated samples, or only the acid soluble portion of it? When the TFe and DFe samples acidified? Readers need to know the cleanliness of the acid used during extractions of DGT and to acidify the TFe and DFe samples. Either acid blanks or preferably method blanks should be presented in the MS. Did the authors perform any "accuracy and precision" tests by using certified samples or spikes prepared in the lab? These points should be clear for the reader of an iron study.

4- The authors assumed that the diffusive boundary of DGT unit are 0.23 mm (page 8 line 6) Is there other supporting data for this assumptions regarding the water movement/turbulence in the deployment sites, besides citation of Warnken et al., (2006)?

5-Did authors observe any biofouling on the active windows of DGT units, especially

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due to long deployment time (14 days to two months) (page 8, line 1). Biofouling may occur in all waters., and organisms can cover the active diffusive windows of DGT. This can cause two detrimental effects: a) changes in the diffusive properties due to clogging b) organisms may adsorb/uptake the studied element (Webb and Keough, 2002). Therefore, DGT technique with long deployment may underestimate the real DGT labile fraction. The likelihood of biofouling on the DGT windows should be considered while deciding the deployment time, and the authors should discuss this point when they interpret the DGT data.

Page 10 line 20: “Jasco FP-777 spectrophotometer” you mean spectrofluorometer?

Page 14 line 5: “Probably, Fe-oxyhydroxides can act as P scavengers, and remove P from the euphotic zone” Reference is needed here.

Page 14 line 15-20: I am not sure that the estimated amount of sedimented Fe from May to August (ca 50 $\mu\text{mol d}^{-1} \text{m}^{-2}$) is correct. My quick calculation gives about 80 $\mu\text{mol d}^{-1} \text{m}^{-2}$. Please check the estimation.

Page 15, lines 13-15: The authors wrote that “. . .the intercept of the regression line on the Fe/Ti axis shows a value of 17, clearly above average crust”. While in the Fig 6b, the intercept of the regression line on the Fe/Ti axis show a value about 11. Please check the intercept.

Page 16, lines 2-3: Who concluded? Stolpe and Hassellöv or Gustaffsson et al., (2000), please check this lines.

Page 20 lines 2-4: Something is missing in that sentence. Please re-formulate it.

Page 26, Lines-1-5: Is it correct to compare the Fe uptake with Fe concentrations? Uptake can be compared with rate of changes in Fe concentration. Additionally , could the authors cite the calculated Fe uptake rates for Baltic phytoplankton? Why did you consider only SFe as a bioavailable fraction? There are reports that shows colloidal Fe can be bioavailable too.

In figure 11 A and C “ $\delta^{56}\text{Fe}$ ” appears as “d56Fe”, please correct them

There are several places where the text is either difficult to read or small corrections are required. In conclusion I consider that the paper is acceptable for publication with minor revisions.

References:

Chin, W.-C., Orellana, M. V. & Verdugo, P. Nature 391, 568–572 (1998)

Webb J.A. and Keough, M.J Mar. Pollut. Bull. 44, (2002).

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6, C2052–C2055, 2009

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