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Interactive comment on “Bioavailability of organically bound Fe to model phytoplankton of the Southern Ocean” by C. S. Hassler and V. Schoemann

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Short Comment on 'Bioavailability of organically bound Fe to model phytoplankton of the Southern Ocean' by Hassler and Schoemann, BGD, 2009.

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Comments:

Improving our understanding of the bioavailability of Fe bound to different organic ligands is of key importance in understanding how Fe regulates current, past and future

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biogeochemistry in key Fe-limited regions and beyond. In this context, such experimental studies that address bioavailability as a function of organic ligand type and phytoplankton species are undoubtedly crucial.

There have been recent efforts to examine the consequences of variability in bioavailable Fe pools on ocean biogeochemistry, as well as investigation how different assumptions regarding the nature of Fe binding ligands can impact bioavailability using theoretical, regional and global models of ocean biogeochemistry (Tagliabue and Arigo, 2006; Tagliabue et al., in press). The authors could point out how their datasets can help in adding/improving the realism in the representation of Fe bioavailability in such models. I am happy to pass on a copy of our in press paper if requested.

To this end, I have a few brief questions/comments:

1. Growth of cultures.

I note that *Phaeocystis* and diatoms were grown at different irradiance levels (120 vs 60 μmol photons $\text{m}^{-2} \text{s}^{-1}$). Can the authors comment on these differences and on how they might impact the interpretation of their results? Particularly since our canonical understanding is that light levels will be a key determinant of the Fe demand (expressed as the Fe/C ratio). In addition, it would be useful to know the specific growth rates, Fv/Fm (if measured), mean biomass etc of the cultures.

2. The definition of Fe limitation

The authors use the Fe/C ratio and/or Fe uptake to suggest the presence/absence of Fe limitation. I am not sure that this makes sense. The Fe/C (uptake) ratio is a measure of the Fe demand to fix a given unit of C. Only when this is considered alongside the external Fe concentration can we suggest the presence/absence of limitation. For example, a high Fe/C ratio can be non-limiting if the external concentration is also high. A better way to look at Fe limitation is by examining the specific growth rates under each treatment relative to an Fe replete standard. For example, in Sec 4.1

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the authors suggest increased uptake of Fe suggests increased bioavailability. Could this not also simply reflect an upregulation of the cellular Fe demand? This could be an especially important factor for phytoplankton that are acclimated to low Fe conditions. Sunda and Huntsman (1997), amongst others, have shown cellular Fe uptake rates increase with increasing external Fe concentrations.

3. Extracellular Fe

The authors consider that extracellular Fe is 'associated with extracellular binding sites'. How is this measured? How can you eliminate the possibility that the Fe is simply physically adsorbed/scavenged on to the only particles present in the cultures (i.e., the phytoplankton cells themselves). Fe should be more efficiently scavenged by particles/cells with high SA:vol ratios- can authors comment on this?

4. Bioavailability of organically complexed Fe

This follows the discussion in section 4.4. The authors might be interested to know that global modeling suggests that only if organically complexed Fe is bioavailable can predictions of chlorophyll and macronutrients be reconciled with observations (Tagliabue et al., in press). This would further support the inferences of this experimental study. The postulated connection between the importance of organically complexed Fe and the cell surface area to volume ratio has also been suggested by observational (Blain et al., 2004) and modeling studies (Tagliabue and Arrigo, 2006; Tagliabue et al., in press). Recent work (e.g., Maldonado et al., 2006) has proposed that copper plays a key role in governing the bioavailability of organically complexed Fe. Do the authors have anything they can add to this debate?

I will end by reiterating my support for this important study. However, I feel this manuscript itself can be improved by considering some of the issues I have raised above and that its scope can be expanded by considering what lessons its has for our conceptual understanding of the processes controlling bioavailable Fe in the field and the representation of key processes in ocean biogeochemical models.

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