

Review BGD – “Microbial iron uptake in the naturally fertilized waters in the vicinity of Kerguelen Islands: phytoplankton–bacteria interactions”

This paper describes an extensive field effort of Fe uptake by size fractionated microorganisms in the vicinity of Kerguelen Islands. The scientific level is high and the methods are suitable and well conducted. The writing, however, should be improved. Terminology can be simplified and shortened. Some more data should be shown and discussion can be expanded. I recommend publication once these corrections are made.

Abstract:

Terminology is awkward (had to read the sentence 4 times):

“Bacterial Fe uptake rates normalized to carbon biomass were highest when bacteria were incubated in the absence of both micro- and pico-nanoplankton. The absence of microplankton resulted in a decrease in bacterial Fe uptake rates by up to 20-fold, while in incubations with the whole microbial community bacterial uptake rates were reduced by 2- to 8-fold”.

What about

“Bacterial Fe uptake rates normalized to carbon biomass were highest in incubations with bacteria only, and dropped in incubations containing other components of the microbial community. Substantial decrease in bacterial Fe uptake rate (up to 20 fold) was found in incubations containing pico-nanoplankton...”

Trying to re-write these sentences, I see that I do not get it. How come that pico-nanoplankton only, resulted in lower Fe uptake rates (i.e stronger competition) than those with whole water (which contains pico-nanoplankton +microplankton – in which case similar or stronger competition is expected?)

In general (throughout the text) I think the choice of words – in the absence of, rather than excluding or bacterial cells only, is awkward.

Similarly the choice of symbols for that purpose ($\rho\text{Fe}_{\text{bact}}^{<25\mu\text{m}}$ etc) is not good. Why not - $\rho\text{Fe}_{\text{bact}}$ for bacteria only, $\rho\text{Fe}_{\text{bact}}^{<25\mu\text{m}}$ for bacteria in the presence of nano-plankton only and $\rho\text{Fe}_{\text{bact}}^{\text{whole}}$ for bacterial uptake with the whole community.

I think that adding the carbon biomasses of phytoplankton although calculated from another study, to a table such as Table 2, is very useful. The calculated bacterial biomass is really a must, while cell numbers will also be great. We want to judge for ourselves if indeed we have very little bacteria that take Fe at “normal” rates, or for example there are more bacteria in one place, but since they are not Fe limited they acquire Fe at slow rates etc...

Table 2 uptake by 0.8-25 is calculated – it is noted in the methods but not in the table.

I liked the discussion part on the DOC limitation and I'd like to emphasize a point that may be missing from the discussion. The rates of Fe uptake we measure when we add Fe represent the pre-conditioning of the organism, and not necessarily its competence. It means that if the bacteria are Fe replete they'll show slow uptake rate per cell compared to Fe deplete cells. Bacteria can be Fe replete due to high Fe supply (e.g above the plateau), or due to slow growth as a result of limiting DOC. Slow growth will slow their Fe bio-dilution and hence inhibit the expression of transport molecules. This explanation indeed goes well with the observed link between primary production and bacterial Fe uptake rate. It has however some implications on the interpretation of the competitive ability of bacteria against phytoplankton. It probably only implies that pico-nanoplankton were more Fe limited than bacteria and hence upregulated more transport systems. So that when supplied with ^{55}Fe they were able to outcompete bacteria for this source.

The discussion mostly compares the new data to other studies, but hardly deals with the uptake itself and the differences between stations (not only for bacteria).