

***Interactive comment on* “Spatial distribution of the iron supply to phytoplankton in the Southern Ocean: a model study” by C. Lancelot et al.**

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I found this to be a very interesting and exciting paper. It reports on the first model simulations to include all of the key, hypothesized sources and transport mechanisms for iron in the Southern Ocean. The results indicate the importance of the sedimentary iron source and highlight a potentially important role played by sea ice transport of iron. Below I provide some detailed suggestions for improving the manuscript before final publication in Biogeosciences. One key weakness in the current manuscript that needs to be addressed is to include a more thorough comparison of the simulated dFe distributions with the available observations. I also think the paper would be improved with a couple of additional sensitivity simulations, focusing on the sedimentary iron source and the sea ice transport mechanism.

In the introduction the authors cite two papers of mine from BGD, the Moore and Braucher, 2007a and 2007b papers (Braucher is mis-spelled). It would be better to cite the final version of this paper, which combined the two earlier discussion manuscripts into one paper, published in Biogeosciences as Moore and Braucher, 2008. The compiled observational database was the same in the final 2008 paper.

In general the methods provides a nice overview of the SWAMCO model and its treatment of iron cycling. One thing that is missing is a description of how particle scavenging of dFe is handled in the model. This is relevant for some apparent bias in the iron distributions, discussed below. Also, the Kfe for the diatoms is given, but what are the values for other organisms in the model?

At the bottom of page 4926, lines 22-26 discuss a “dormancy phase” entered when the solar flux is less than 5 W/m² and Chl a concentration is lower than 0.1 mg/m³. Does this mean that at that point biomass levels are frozen (all loss terms set = 0) until light starts to increase in the spring?

Section 2.1.3 outlines the parameterization of the sequestration of dissolved Fe in sea ice, and its subsequent release upon ice melt. I think the approach outlined is a reasonable first try at incorporating this process, given the limited data available. However, it is clear that the choices made here strongly impact the simulation results. Also, the formulation only allows for incorporation of dissolved iron into the sea ice. There is good evidence that sea ice forming in coastal regions may pick up substantial amounts of particulate iron (Grotti et al., 2005). This potential additional iron source should be discussed. This additional source could perhaps be included in an alternate sea ice iron formulation (higher dFe for ice in coastal regions).

Section 2.2.1 addresses iron sources in the simulations. An additional section should address iron sinks, in particular how scavenging and iron removal from the system are handled. In lines 16-19 of this section the authors note that their estimate of iron inputs from icebergs are an order of magnitude lower than estimated recently by Raiswell et

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al., 2008. Some additional discussion of this difference and the factors that drive it should be added.

At the end of section 2.2.1 the authors describe how the sedimentary source is handled in the model. A constant source of 0.43 $\mu\text{molFe}/\text{m}^2/\text{day}$ is applied for all grid cells shallower than 900m depth is employed, and it is noted that this value is ~ 3 times greater than that estimated by Moore and Braucher (2008) for this region. I would have preferred a source that decreased with depth, but this approach is probably okay to first order. Why was this particular value chosen? The main question I have is how sensitive are the results to this particular, constant value chosen for the sedimentary iron source? Are the major conclusions regarding the sedimentary iron source changed by increasing or decreasing this source term by say ,50%? A couple of additional simulations could address these questions for the final publication.

Section 3.2. reports on the simulated distributions of dissolved iron and offers a limited comparison with the observations. This comparison consists of two mean profiles computed from shallow areas (depths < 1200m) and deeper regions. This is unsatisfying and doesn't really let the reader evaluate the model results. For the final BG publication, a much more extensive comparison with the observations needs to be included. For example what does a scatterplot of observed vs. simulated iron look like? What is the correlation between the two? The authors note that summer surface dFe simulated values are "quite low". How low? How do they compare with the observations, most of which were made in summer. The winter surface concentrations shown in Figure 5a seem reasonable over most of the Southern Ocean 0.1-0.2 nM, but are very low (< 0.01 nM) over large areas in the Pacific and Atlantic sectors, which leads to low sea ice concentrations (Figure 5b), maybe in part because of uptake into the sea ice. This is an interesting pattern, and I agree with the authors highlights a deficiency in the way ice incorporation of dFe has been parameterized. In figure 6, there seems to be a strong bias in the top panel for upper ocean waters below the euphotic zone. The averaged observations are between 0.2-0.3 nM, while the model results are higher at

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~0.4nM. These subsurface iron concentrations are very sensitive to how particle scavenging is implemented. This is part of the reason I suggested details on the scavenging need to be added to the methods section. A more comprehensive comparison with the observations could shed light on whether scavenging and/or particular source terms (with regional variations) are driving this overestimation of dissolved iron concentrations. Lastly, in several places the authors cite the observational data as “..observed (Moore and Braucher, 2007a)”. I would suggest noting in the methods section that you compare “the model output with the observational dataset compiled by Moore and Braucher (2008).” Thereafter you could just refer to the “observations”, without citing our paper each time. After all, we just compiled measurements made by many others.

Section 3.3 discusses chlorophyll and bloom distributions in part in relation to the winter sea ice iron content shown in figure 5b. The authors note how the bloom distributions closely follow the sea ice iron content during winter. The blooms are also overestimated in many regions, suggesting that too much iron has been released from melting sea ice. Similarly, in the areas with very low winter sea ice iron content, where nearly all the dFe has been removed from the upper water column, the simulated chlorophyll concentrations are much lower than observed by SeaWiFS, suggesting too much iron has been removed during sea ice formation. These results highlight the potential importance of the sea ice iron transport mechanism, but also point to deficiencies in the way iron incorporation into sea ice has been implemented. It seems impossible for all the dFe in the upper ocean to end up in the sea ice. It would be great if an additional simulation could explore an alternate formulation, where the iron incorporated into the sea ice is proportional to the dFe concentrations in the water, perhaps with a lower maximum value. This way the upper ocean iron depletion in the low iron regions would not be so extreme, and the release in other areas that is driving phytoplankton blooms would be less intense, reducing these blooms towards what is observed from satellites. A higher maximum sea ice iron concentration for ice forming in coastal regions could also be explored.

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Line 5, page 4943, "...transporting sea ice from one region..." should be "... transporting iron from one region ...", right?

Figure 1. caption states that iceberg source is 0.22 pmolFe close to the continent and also 0.22 pmolFe farther offshore. Offshore should be lower, right?

Figure 3. caption should state that the black line shows the 15% ice cover location.

Figure 5b, caption should state what the arrows indicate in 5b.

Figure 8. What is displayed in this figure? Is it the change in surface concentrations? For what month(s) is this calculated?

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