Biogeosciences Discuss., 6, C1393–C1400, 2009
www.biogeosciences-discuss.net/6/C1393/2009/

Biogeosciences Discussions

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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Received and published: 31 July 2009

Referee General Comment: '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 import 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'. Reply: We

thank Keith Moore for his enthusiastic comment on our paper and for his constructive suggestions. Even if the main objective of our paper is more 'qualitative' focusing on mechanisms rather than absolute iron numbers we fully agree that our conclusions will be stronger when a quantitative evaluation of the model performance and sensitivity tests on iron sources numbers are included. We are now preparing the revised version that will include a quantified comparison of modeled and observed DFe in the result section and a new 'sensitivity analysis of iron sources magnitude' section to be included in the discussion. Based on the large uncertainty surrounding the different iron source values and considering the scarcity of some of them we choose to run our sensitivity runs by multiplying and dividing by 2 the reference value of each source (this experimental design is suggested below by the Referee for the sedimentary iron source).

Referee Comment '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'. Reply: The reference will be updated in the revised version. Thanks.

Referee comment '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'. Reply: The Fe cycling in the actual version of SWAMCO is extremely simple including DFe and Fe in all particles without consideration of Fe scavenging (details in Lancelot et al., 2000). We assume that DFe is bioavailable and includes truly dissolved iron as well as Fe-ligands (<0.4 um). This allows consideration of the forms involved in the process of iron biological uptake and remineralization in the model equations. Particle Fe scavenging was not considered for mainly three reasons: DFe generally <0.6 nM (Jonhson et al., 1997; Boye et al., 2001), no explicit description of particle aggregation in the used SWAMCO version, no 'measured' but 'tuned' parameterization of Fe scavenging in existing models. We however agree that neglecting this process might overestimate DFe in the Fe enriched upper waters and will discuss this in the revised version.

Referee Comment 'Also, the Kfe for the diatoms is given, but what are the values for other organisms in the model'? Reply: In the model description we choose to detail those parameters that were changed with respect to the published versions of SWAMCO (Lancelot et al., 2000; Hannon et al., 2002; Pasquer et al., 2005). As this is also a request by the second Referee (A Tagliabue), KFe values of the three other functional groups (pico/nanophytoplankton: 0.03 nM; coccolithophorids: 0.03 nM; Phaeocystis colonies: 1.5 nM) will be added in the revised version.

Referee Comment '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'? Reply: Right. Dormancy was added as overwintering strategy in high latitude when darkness lasts for several months. The crude parameterization was adjusted based on late-fall/early-winter observations of phytoplankton and the ambient light.

Referee comment '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)'. Reply: We are aware that Fe associated to living cells and detrital particles has been measured

in sea ice by several authors including some of us. However, this accumulation results of pelagic (living and dead) particles trapping at the time of sea ice formation and further biogeochemical transformations up to the time of ice melting when these are released in the surface layer. Taking this properly into consideration needs an explicit sea ice biogeochemical model to be coupled to the SWAMCO model. This sea ice biogeochemical model is still under development.

Referee Comment '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'. Reply: Scavenging is not explicitly described. Only biological uptake and regeneration are considered. Iron sink also includes export that is bound to particle (mainly diatoms) sinking.

Referee Comment '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 al., 2008. Some additional discussion of this difference and the factors that drive it should be added'. Reply: Our estimate uses data on Fe content of glacial ice while the estimate by Raiswell et al., 2008 includes nanoparticulate Fe oxyhydroxides from ice-hosted sediment corrected for their bioavailability. We consider however that once release in the water column the major part of these nanoparticles will be quickly exported to the deep ocean, hence being not significant for the surface layer. This hypothesis will be added in the revised version of the paper. In addition the value chosen for the Fe content of glacial ice will be tested in the sensitivity runs.

Referee Comment '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 umolFe/m2/day is applied for all grid cells shallower than 900m depth is employed, and it is noted that this value is about 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'. Reply: The source of 0.43 umolFe/m2/day is calculated combining a Kz estimate of 1x10-4 m2s-1 as a mean value for the Southern Ocean (Law et al. 2003; Blain et al. 2007) with DFe gradients from a deep vertical profile of DFe measured in the Scotia Sea with input from the continental shelf of the Orkneys Islands (de Jong, Lannuzel and Schoemann, unpubl. data). This is an apparent flux from the continental shelf that takes into account the direct input of DFe from the sediments and also the input of DFe released from the dissolution of particulate lithogenic Fe. This estimate lies in the range of reported fluxes from the continental shelves, from 0.127 μmol/m2/day (Blain et al. 2007) estimated for the Kergelen plateau to 4.3 μmol/m2/day considered as a mean for the global shelf by Elrod et al; (2004). Sensitivity studies using reduced and increased sedimentary sources will be discussed in the revised version as proposed by the Referee.

Referee Comment '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 scatter plot of observed vs. simulated iron look like? What is the correlation between the two'? Reply: We fully agree with the Referee and will develop this in the results section. Actually we constructed a database containing 1275 observations (based on Moore and Braucher 2008 but completed with unpublished data) for the shelf and oceanic domain. The two profiles of Fig.6a and b are averages made on model simulations and the collected observations for the shelf and oceanic domain respectively. For this comparison observations were grouped according to the grid of the 3D model. These average profiles will be maintained in the revised paper but SD values for both the observations and the simu-

lations will be added. The detailed compared profiles will be assembled and submitted to BGS as supplementary material. Finally we will provide a scatter plot comparing model DFe to observations and compute the statistics (RMS, Bias). These will the basis for evaluating the sensitivity tests.

Referee comment '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 Fig.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 about 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'. Reply: We agree and we will take this point when discussing the scatter plot of modeled DFe vs observations.

Referee Comment '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'. Reply: Yet you did it and this is very useful for the scientific community. However we will do like you wish.

Referee Comment 'Section 3.3 discusses chlorophyll and bloom distributions in part in relation to the winter sea ice iron content shown in Fig.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'. Reply: We agree. While our present knowledge is insufficient to modify the way DFe sea ice trapping is described in the model, the only possibility to address this question is to modify the maximum concentration reached in sea ice. Planned sensitivity tests are modifying the reference value by a factor 2 and 0.5.

Referee Comment 'Line 5, page 4943, ". . .transporting sea ice from one region. . ." should be ". . . trans- porting iron from one region . . ..", right'? Reply: Both

Referee Comment 'Figure 1. caption states that iceberg source is 0.22 pmolFe close to the continent and also 0.22 pmolFe far ther offshore. Offshore should be lower, right'? Reply: Yes the offshore values is two order of magnitude lower i.e. 0.22x10-2 pmolFe

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

OK

Referee Comment 'Figure 5b, caption should state what the arrows indicate in 5b'. Reply: Arrows show ice velocities. This will be added in the legend

Referee Comment 'Figure 8. What is displayed in this figure? Is it the change in surface concentrations? For what month(s) is this calculated'? Reply: Fig.8 shows the contribution of each source obtained by subtracting surface DFe results from the FULL experiment and the corresponding sensitivity experiment. It is calculated in winter (September) when biological activity is minimum. Values are averaged over the 1997-2000 period.

Interactive comment on Biogeosciences Discuss., 6, 4919, 2009.