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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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Referee Overall Comments 'In this manuscript, Lancelot et al., use the SWAMCO ecosystem-biogeochemical model nested within the NEMO-LIM ocean circulation-sea ice model in a Southern Ocean-only configuration to examine the impact of different iron (Fe) sources on chlorophyll biomass. The authors find that continental shelf sediments are a key Fe source for phytoplankton chlorophyll and also draw attention to the potential importance of iceberg calving and the transport of dissolved Fe (dFe) by sea ice. The evaluation of the importance of different Southern Ocean Fe sources is an important current question for the ocean biogeochemistry community and as such the paper will be of interest to the Biogeosciences readership. However, the manuscript needs major revisions prior to being accepted to publication. The analysis of the model

is not, to my opinion, quantitative enough and remains somewhat superficial (in terms of both model results and the evaluation of model performance). This aspect must be improved prior to being accepted for publication. The simulations conducted are very short in terms of reaching 'quasi equilibrium', which is fine if their length can be justified, but obvious sensitivity experiments must also be examined. Finally, the results must be put into context of the literature and in particular a recent paper (which the authors of the present manuscript could not have been aware when submitting their discussion version), which addresses many of the same questions (although not all) in a much more quantitative fashion'. Reply: We thank A. Tagliabue for his constructive comments and suggestions. Of course we will refer when relevant to his freshly-release paper that does not however refer to our own modeling work in their conclusion recommending the consideration of sea ice iron in future models. This being said we agree that one weakness of our paper is the quantitative validation and will develop this aspect in the revised version.

Referee General Comment 'A recently accepted paper (Tagliabue et al., 2009) used a global ocean circulation and biogeochemistry model to address the quantitative impact of sedimentary and atmospheric Fe sources on Southern Ocean Fe concentrations and carbon export during simulations of 500 years. They found sediment Fe to be more important than dust derived Fe in governing overall Southern Ocean export production (depending on the dust model employed, but supporting the conclusions of Lancelot et al.) and further explored the quantitative importance of each source to dFe and carbon export on a regional basis (which at the moment cannot be compared with Lancelot et al.). Of course, Lancelot et al. cannot have been aware of this paper when submitting their discussion paper, but it is now accepted and published. Tagliabue et al. (2009) should be discussed in the introduction, as well as during the discussion sections. As such, we can learn about how different ecosystem-biogeochemistry models differ in their conclusions. The model of Tagliabue et al. (2009) is somewhat simpler in terms of the number of phytoplankton groups, but appears more complex in terms of how Fe is treated (dynamic Fe/C ratios, ligands, scavenging etc, but this needs clarification) than that used in the current manuscript, it has a global domain and was integrated for much longer (500 years versus 11 years). On the other hand, Tagliabue et al. (2009) do not consider iceberg or sea ice sources of Fe. As such, the appraisal of these sources is a major novelty of Lancelot et al. (but I feel some sensitivity tests are necessary). In addition there are a number of modeling and observational studies that have looked at the importance of sediment and (in more detail) dust derived Fe (e.g., Cassar et al., 2007; Aumont et al., 2008; Moore and Braucher, 2008; Wagener et al., 2008; Tagliabue et al., 2008; 2009). In light of the Tagliabue et al. (2009) paper (which covers very similar ground to Lancelot et al.), as well as (less Southern Ocean specific) other work, the final paragraph of the introduction needs to be completely revised'. Reply: We will revise the introduction and discussion based on these suggestions. We like to remark however that most of the suggested papers have been cited and used in our paper except obviously Tagliabue et al. (2009) that was not published when we submitted in early April.

Referee General Comment 'Another recent paper (Schwarz and Schodolk, 2009) has examined the impact of icebergs on chlorophyll in the Southern Ocean. They discuss other impacts of icebergs on phytoplankton that are not considered in this study (transport of cells, mechanical disruption of surface waters and release of freshwater) – these should also be discussed in the current manuscript to add depth to this section. How might these factors interact with the role of icebergs in supplying Fe?' Reply: We thank the Referee for informing us on this 'in press' paper that we will use in our revised paper as support of the positive influence of iceberg melting on phytoplankton growth. Obviously our model does not consider transport of cells and mechanical disturbance of surface waters but well the release of freshwater that contains DFe. In our model the spatial distribution of iceberg-derived iron supply is based on the results of an iceberg model (Gladstone et al., 2001), which computes the spatial distribution of the melt water injected into the ocean due to iceberg melting with an iron concentration of 20.4 μmol Fe m-3 (Löscher et al., 1997)

Referee General Comment 'I remain concerned about what conclusions can be drawn regarding transport mechanisms of sediment Fe, dust Fe etc throughout the Southern Ocean basin during simulations of such a short duration (especially subsurface sources related to the continental margin and dust that does not dissolve at the sea surface). The authors need to carefully consider what impact this simulation duration has on their results. For example, it appears that the simulations began in 1989 and were initialized with their initial conditions (from a variety of different sources) at this point? From that point onwards the model is run prognostically (although there appears to be some restoring, but over what timescales?). I wonder what kind of 'shock' this provides to the system (i.e., is there a significant drift in the control)? Was the model not spun up under 1989 forcing prior to beginning the experiments? If that was the case, then I imagine the tracers were not at their 'climatological' value when the experiments began, since even under constant 1989 forcing there must be some drift due to the ecosystem/circulation model. More detail is needed on this. What is the timescale of the restoring terms and to what tracers does this apply? Have the authors conducted any tests to assess the performance of their model over such short timescales, i.e., what might be missed in such an experimental design? Certainly, the manuscript must be revised to place less weight on processes/mechanisms that might have been influenced by the very short simulation times (for such a large basin) unless there is evidence to the contrary.' Reply: The restoring is applied only on surface salinity, with a time scale of 2 months. We will specify that in the revised version because the submitted manuscript was not precise enough when mentioning 'the salinity of the ocean is restored towards the climatology of Levitus with a restoring timescale of two months'. This restoring is required because precipitation are not very precise in the Southern Ocean and without such a restoring surface salinity will drift quickly away from observed values. All the other variables (and the salinity at depth) are totally prognostic. In the revised version, we will also give more details about the spin up procedure and the experimental design. For the physical variables, the initial conditions are derived from a 85-year global simulation, using an interannually forcing, as in Timmermann et al. (2005). We consider that it is better than using the forcing of year 1989 only, because applying during such a long time the forcing of one particular year may induce biases in the simulated state. With the procedure applied here, using an interannually varying forcing, there is no particular shock on the initial state for the physical variables. Actually the shock is the same as should occur naturally from the interannual variations of the forcing. The Referee is right when he says that the conditions are not at equilibrium for the whole ocean in our short simulations, in particular for biogeochemical variables. This is the reason why, for instance, we do not discuss the balance of iron in our simulations between sources and sinks. The impact of the deep sources of iron on surface values is also very limited at this time scale. On the other hand, the sources of iron from the sediments on the continental shelves have a clear impact that could be estimated from our simulations. To take into account the spin up of the model, we have not discussed the first 8 years of the simulations. In the majority of the regions (at least the ones that are discussed in the paper), this is sufficient to reach a quasi-equilibrium in the first 500 meters of the water column. This has been checked for the control run by performing an additional simulation, starting in 1989 with the initial conditions obtained in 2000 in the run analyzed in the submitted manuscript (i.e. after 11 years of simulations). For both initial conditions, the results discussed here for the period 1997-2000 were very similar. In other words, the drift in 11 years was much smaller than the signal analyzed, which could correspond to a practical definition of quasi-equilibrium. We thus already ensure to discuss processes that are not significantly influenced by the short simulation times. We did not insist too much on those relatively technical issues in the first version of the manuscript but more details will be given in the revised version.

Referee General Comment 'A related issue is that of the northern boundary of the model domain. Tagliabue et al. (2009) show dFe anomalies related to sediment and dust Fe (Their Figure 1) than propagate far from Patagonian and Australasian sources. The SWAMCO model cuts out much of the dust deposition from Patagonia and appears to eliminate most of the deposition that occurs around Australia and south of Africa. Is it not possible that dust from these sources could propagate into the SWAMCO model

domain? As such, is it not possible that this study is underestimating the importance of dust? This issue needs to be discussed'. Reply: The frontier of the 3D- NEMO-LIM-SWAMCO domain is latitude 30°S and thus includes dust deposition from Patagonian and Australian sources (see text pp 4928 and 4929). We correctly choose to show and discuss results obtained in the Southern Ocean in the different figures to eliminate the influence of boundary conditions.

Referee General Comment 'The analyses presented in the current manuscript are interesting, but suffer from a distinct lack of quantitative treatment. In addition, the SWAMCO model simulates a number of keystone Southern Ocean phytoplankton groups – yet these are never discussed. While it might be justified to run shorter simulations with a more complex model (such as SWAMCO), it is disappointing when the complexity of the model is never fully utilized. One wonders whether it is necessar y to have used this model when only chlorophyll and Fe output is presented? How well does SWAMCO represent our current understanding of Southern Ocean biogeography? How does the phytoplankton species composition change when different Fe sources are switched on and off? The potential of the SWAMCO model must be better utilized. It would be interesting to examine which Fe sources are impor tant to different functional groups.' Reply: The question of the stimulation of one or the other functional group by the different iron sources is interesting and might be the subject of a following paper. In this first manuscript we are interested in understanding the dynamics of the bulk phytoplankton with respect to the light regime and the different iron sources. That's the reason why we choose to compute Chl a and to compare our results with Seawifs data which provide synoptic views of the Chl a field at the grid of our model and for the simulated years. Such a needed high spatio-temporal resolution is unfortunately not available for phytoplankton functional groups.

Referee General Comment 'Aside from the key experimental tests, there are no additional sensitivity tests to explore how reasonable some of the model conclusions are. For example, the authors come to a provocative conclusion regarding the role for sea ice as an Fe 'loss' term for the highly productive southwestern Ross Sea (for example, due to advective transport of sea ice Fe out of this sector). Yet this result seems implicit in their parameterization of the ocean to ice Fe flux (i.e. Fe is taken up until all is exhausted, or the sea ice reaches a concentration of 16.5 nM). The authors acknowledge that sea ice Fe in the Ross Sea has a lower concentration (2-6 nM dFe) than that of the Weddell Sea (16.5 nM, which forms the basis for the modeled parameterization). It would be interesting to know whether sea ice continues to have as large a negative effect on phytoplankton chlorophyll during a sensitivity test with a lower value for the 'target' sea ice dFe concentration. The fidelity of the conclusions must be evaluated to some degree. Does including sea ice Fe losses improve the modeled chlorophyll a and dFe? If not, what does this tell us about the role this source is playing? It would also be interesting to know how the results change under different assumptions regarding dust deposition (as examined in Tagliabue et al., 2009), sediment, and iceberg Fe fluxes. Since the model only appears to be spun up/run for 11 years, I imagine that some additional sensitivity tests along these lines are not computationally prohibitive and would enable us to better understand the sensitivity of biogeochemistry to possible variability in (especially) the poorly constrained source terms. In fact, the authors even provide alternative numbers for a number of their source terms throughout the manuscript – can these not be assessed? ' Reply: Based on preliminary trials testing the progressive addition of different DFe sources, the role of DFe ice sequestration and replenishment by sedimentary iron, and release at the time of ice melting was found crucial to describe the observed asymmetry of blooms at the retreating ice-edge. For this iron source as for the three others, we have used the best guess based on existing literature. We fully agree that our conclusions will be stronger by including in the manuscript different sensitivity run with different iron source numbers. Considering the large uncertainty surrounding the different iron source values and 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.

Referee General Comment 'One metric that could be used to evaluate the importance

of a given source and perhaps also give an idea as to the magnitude of its source term is to use statistics. For example, how well (in a quantitative sense) does SWAMCO reproduce the dFe observations compiled by Moore and Braucher (2008) – there are many observations in the Southern Ocean (south of 35S) that could be used - as well as chlorophyll concentrations from SeaWiFS when compared at identical locations? This is a much more quantitative means of evaluating model performance than qualitative appraisals of figures 'by eye' (although I agree that the figures are needed to put the statistics into context). The authors could then examine how the correlation (for dFe and chlorophyll a) changes during their experiments, as well as during the additional sensitivity tests that I propose above. The iinfluence of changes in a given source on the fidelity of the modeled dFe and chlorophyll distributions (relative to observations) might assist in understanding how probable the presence and/or magnitude of a given source term (dust, sediments, icebergs and sea ice) is.' Reply: We agree with the referee that a quantitative comparison between simulations and observations will make our conclusions stronger and will add such analysis in the results section. For DFe we constructed a database containing 1275 observations (based on Moore and Braucher 2008 but completed with unpublished data) for the shelf and oceanic domain. Actually 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 simulations 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. A similar attempt will be made for surface Chl a

Referee general Comment 'It would be nice if the font size on the scale of the figures could be increased'. OK

Referee Specific Comment ' Page 4921, line 6 – 'Boyd and van den Berg, 2000' is 'Boye and van den Berg' in the reference list. Regardless of that, this appears to be a strange reference to justify a statement regarding mesoscale Fe enrichment experiments?' Reply: The Referee is right. The reference is wrong and not that relevant. It will be deleted.

Referee Specific Comment 'Last paragraph of introduction: see above discussion on new papers. This section needs to be heavily revised'. Reply: We already replied positively to this strong recommendation.

Referee Specific Comment 'Page 4925, line 5 – how does a calibration in 1D relate to its 3D application here? We should at least be told where the 1D calibration occurred – how relevant is that location to basin-scale Southern Ocean biogeochemistry?' Reply: Our established strategy to develop complex biogeochemical models involves a first calibration step of the new parameterization in a simplified but realist physical frame. The location is chosen according the phytoplankton community, i.e. the observed presence of the tested functional group. This has been done at the location of KERFIX for testing the coccolithophorid module and in the Ross Sea for the Phaeocystis module (B. Pasquer PhD 2005; Pasquer et al., 2005). Once done the updated biogeochemical module is transferred to the 3D model with some adaptation needed when passing from 1 vertical D to 3D. Temperature and light adaptation are considered by the parameterization and thus apply to the global ocean (see Pasquer et al., 2005)

Referee Specific Comment 'Page 4925, line 24 – is there no grazing loss term for Phaeocystis colonies? If so, then this should be stated explicitly. It is consistent with the conclusions of a previous modeling study looking at phytoplankton – zooplankton coupling in Phaeocystis dominated Ross Sea (Tagliabue and Arrigo, 2003).' Reply: Phaeocystis colonies are assumed resistant to grazing (see Pasquer et al., 2005). Reference to Tagliabue and Arrigo (2003) is here out of scope as the paper don't discuss dominance of functional groups.

Referee Specific Comment 'Page 4926, line 7: Sentence beginning: "For this application. . .." This should begin a new paragraph Page 4926 line 14 – the K_μ for dFe (0.6nM) appears quite high? What are the implications of this given the seasonal variability in dFe? What was its value for the other phytoplankton groups?' Reply: The half-saturation constant for Fe uptake for diatoms was based on lab experiments by Timmermans et al. (2004) and chosen according to the size of reported dominant diatoms. Actually no seasonal change in KFe is considered along the season but might be taken in a future version of SWAMCO to mimic the change in diatom dominance towards smaller species better adapted to low Fe concentrations. KFe values of the three other functional groups are based on literature review and own experimentation (pico/nanophytoplankton: 0.03 nM; coccolithophorids: 0.03 nM; Phaeocystis colonies: 1.5 nM). The detailed information will be added in the revised version

Referee Specific Comment ' Page 4926 – How are chlorophyll to carbon ratios calculated? Does Fe influence them? If so, this should be considered in the discussion, do the simulated changes in chlorophyll relate to changes in biomass? ' Reply: As described in the original version of the model (Lancelot et al., 1991; 2000), each phytoplankton group is described by 3 state variables –functional F, reserve R, monomers S – chosen to consider the observed decoupling between photosynthesis and growth. Based on this, nutrient N, P, Si, Fe are only involved in F (N for proteins, P for ATP,DNA, Fe in Chla . . .) and have a fixed nutrient-specific stoichiometry while R and S only contain C. The chosen values are based on elemental biochemistry. With this description our model is able to describing the well-known daily variations and nutrient-stressed changes of the cell stoichiometry i.e. decrease of Fe:C cell ratio under high light or Fe stress. Discussing this is not relevant here, considering the scope of the paper.

Referee Specific Comment 'Page 4926, line 18: Fe/C ratios. Are the Fe/C ratios the same for all phytoplankton groups? What about between diatoms and Phaeocystis? Tagliabue and Arrigo (2005) found it necessary to ascribe different Fe/C ratios between each species to accurately reproduce the macronutrient and phytoplankton dynamics of the central and western par ts of the southwestern Ross Sea (dominated by Phaeocystis and diatoms, respectively). In addition, is there no effect of light or Fe on Fe/C ratios? Sunda and Hunstmann (1997) have (amongst others) demonstrated that Fe/C ratios decline with decreasing external Fe and increasing light. If this processes is not included, then it must be stated. For example, it is possible that SWAMCO is over estimating the biological consequences of removing certain Fe sources, since in SWAMCO the Fe demand remains constant. In reality, one would expect the Fe demand to be down regulated as Fe becomes scarce. What about the C/N/P stoichiometr y? Can NO3 and PO4 observations be used to evaluate the model and, again, can the changes in these tracers during the different experiments help discern the likelihood of the presence/magnitude of a given source?' Reply: Again our phytoplankton module is based on biochemical principles and by considering fixed elemental composition of functional compounds only allows to describing the observed variations in total cell stoichiometry (i.e. (F)-Fe/(F+R+S)-C). Discussing all this is too far from the purpose of our paper but might be interesting in a more specific paper.

Referee Specific Comment 'In addition, what about scavenging/ligand complexation of dFe, is this included in the model? If so, how?' Reply: DFe includes truly dissolved Fe but also Fe bound to small ligands (<0.4um) and is considered as bioavailable. Scavenging in the upper Southern Ocean was neglected based on Johnson et al. 1997 and Boye et al., 2001.

Referee Specific Comment 'Page 4932, line 5 – 'quasi equilibrium' – how is this defined? Does your model really reach equilibrium after 8 years? What are the timescales for your restoring terms?' Reply : See the response above (General Comment).

Referee Specific Comment 'Section 3.2. dFe should be compared statistically with the database of Moore and Braucher (2008). In addition, I am sure the authors would prefer that the final version of their paper be cited, rather than the discussion paper. Section 3.3. How does a statistical comparison of SeaWiFS with modeled chlorophyll a compare? In addition the modeled primary productivity for the Southern Ocean can

be compared to Arrigo et al. (2008) and for the Ross Sea it could be compared to a regional model (Tagliabue and Arrigo, 2005). As mentioned above, does the modeled phytoplankton biogeography reflect observed evidence (for which there is enough to at least draw some general trends). A more quantitative treatment of the modeled dFe, chlorophyll a and primary production would provide a solid framework within which to draw meaningful conclusions from the sensitivity tests.' Reply: We already described above our plan concerning the quantitative evaluation of our simulations. This added to the sensitivity analysis conducted on the iron source values will constitute a substantial addition to the manuscript length. Although we could calculate primary production we don't see this as a 'plus' for our paper that focus on iron source and phytoplankton blooms.

Referee Specific Comment 'Page 4939, lines 5 to 9 – This is a model so the changes in chlorophyll (and ideally carbon as well) can be related to the change in Fe much more quantitatively. Page 4939, lines 10-16 – This is one of those sections where the use of a quantitative statistical (or other) metric would make the conclusions much more robust. As it stands the strong statements are supported by rather anecdotal evidence.' We already replied to this above.

Referee Specific Comment 'Conclusion section: This section contains discussion. The conclusions should be much more concise.' Reply: We feel happy with our conclusion that also includes directions for future research.

Referee General Comment 'I would also like to see a section that deals with the drawbacks to the experimental strategy and how this would impact the conclusions of the study. Topics might include: length of simulation, treatment of Fe sources that propagate across the nor thern boundary, variability in modeled Fe fluxes (given the range in numbers cited by the authors during the paper) etc etc. The authors do discuss a plausible range for some of their fluxes in the conclusion section, it would be nice if they had performed sensitivity tests to assess what degree of impact a (for example) one order of magnitude change in iceberg fluxes has – ideally also using some quantitative metric to assess its impact. I do not think such tests are beyond the scope of this paper and would greatly increase the importance and impact of this work.' Reply: We plan to include in the 'result' section of our revised manuscript text and figures on the quantitative evaluation of our results and, in the 'discussion' part a new section summarizing the results obtained by the different sensitivity tests on the iron sources values.

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