Biogeosciences Discuss., 6, C1061–C1070, 2009
www.biogeosciences-discuss.net/6/C1061/2009/

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: 7 July 2009

Review of Lancelot et al., Spatial distribution of the iron supply to phytoplankton in the Southern Ocean: a model study. Biogeosciences Discussion. 2009.

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1. 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 ice-

berg 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.

2. General Comments

2.1 Literature

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.

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 fresh water) – 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?

2.2 Experimental Design

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.

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 Fe 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.

2.3 Results and Model Evaluation

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 necessary 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 important to different functional groups.

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?

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 35°S) 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 influence 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.

2.4 Figures

It would be nice if the font size on the scale of the figures could be increased.

3. Specific Comments

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?

Last paragraph of introduction: see above discussion on new papers. This section needs to be heavily revised.

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?

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).

Page 4926, line 7: Sentence beginning: "For this application. . .." This should begin a new paragraph

Page 4926 line 14 – the Kμ 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?

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?

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 parts 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 stoichiometry? 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? In addition, what about scavenging/ligand complexation of dFe, is this included in the model? If so, how?

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?

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.

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.

Conclusion section: This section contains discussion. The conclusions should be much more concise.

4. 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 northern 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.

5. References

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