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Biogeosciences Discussions

Interactive comment on "Side effects and accounting aspects of hypothetical large-scale Southern Ocean iron fertilization" *by* A. Oschlies et al.

Anonymous Referee #1

Received and published: 7 July 2010

General

The authors present an interesting manuscript describing the potential regional and global consequences of large scale iron fertilisation in the Southern Ocean within a coupled ocean-atmosphere-terrestrial biogeochemical GCM. Although there have been a number of studies describing the predicted response to large scale iron fertilisation, to my knowledge, the study of Oschlies et al. is the first to present results from a fully coupled carbon-climate earth system model, rather than purely an ocean model. The approach highlights important new side effects and allows interesting discussions of how subsequent carbon sequestration should be accounted. In general I

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Interactive Discussion

Discussion Paper

thus support publication of the manuscript in Biogeosciences. There were a number of minor issues I would like to see addressed.

Specific comments

I generally accept the authors' pragmatic approach to simulating the influence of iron fertilisation by simply increasing the maximum growth rate and agree with their suggestion that given the current level of data/understanding, complex models including iron chemistry are likely to be poorly constrained at present. However I would like to have seen some further consideration of any potential caveats arising from such an approach, alongside further discussion of similarities/differences between the current results and those from models which have used a more explicit parameterisation of the iron cycle (e.g. Aumont and Bopp 2006). For example, it is possible that iron fertilisation would have a greater physiological effect on the light dependence of growth rather than the maximum rate. Would this have significantly influenced the results?

The discussion of remote aspects/feedbacks was a key strength of the paper. In particular the feedbacks with the terrestrial carbon cycle could clearly not have been explored outside of such a fully coupled carbon/climate model. I am also not aware of other studies suggesting that the anoxic volume of the oceans would actually be expected to decrease due to the reduction in mode water nutrient concentrations. With regard to the latter, the authors might wish to mention that fertilisation north and south of the boundary region separating portions of the Southern Ocean responsible for the ventilation of Antarctic bottom water and sub-Antarctic mode water/Antarctic intermediate waters (e.g. see Marinov et al. 2006) could have different impacts. E.g. if hypothetical fertilisation were limited to the southern portion of the Southern Ocean, presumably the low latitude feedbacks/downstream processes would be minimised?

Section 3.1.1 and Figure 2. Some indication of the magnitude of the seasonal cycle (and it's latitudinal dependence) within the model might have helped here.

Page 2952, Lines 10-25. I didn't find the discussion of artificial upwelling directly rele-

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vant.

Page 2967, Line 15. Some references might have been useful here.

Minor comments

The manuscript was generally very well written. Below I have highlighted a few remaining minor grammatical errors.

Page 2950, Line 21. at (rather than ‘on’) the expense of

Page 2951, Line 11. as a last resort

Page 2955, Line 5. Suggest: ‘Because of the difficulties inherent in explicitly modelling complex iron chemistry. . .’

Page 2963, Line 11. Lower than a very few uM?

Interactive comment on Biogeosciences Discuss., 7, 2949, 2010.