Biogeosciences Discuss., 10, C4754–C4756, 2013
www.biogeosciences-discuss.net/10/C4754/2013/

Biogeosciences
Discussions
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Interactive comment on "Sedimentary and atmospheric sources of iron around South Georgia, Southern Ocean: a modelling perspective" *by* I. Borrione et al.

Anonymous Referee #2

Received and published: 5 September 2013

1. Brief summary

In this paper, the authors use outputs of regional hydrodynamic and biogeochemical models in order to assess the mechanisms of natural iron fertilization in this area. They investigate the possible sources and removal terms of dissolved iron (dFe) in the surface and deep waters with an emphasis on sedimentary and dust inputs. They conclude that lateral inputs of resuspended shallow sediments are the most important source of dissolved iron around South Georgia and that atmospheric dust inputs are almost negligible.

2. Major comments:

In my opinion, inputs like glacial inputs or upwelling are a bit neglected in this work. I feel that at this stage, you cannot claim the identification of the main sources of dissolved iron in the area as you only truly consider sedimentary and dust inputs. I would like to see more discussion on these other sources. Other concerns are:

1) Modelled dissolved iron concentrations are compared with surface dissolved iron data obtained in Austral summer 2008 by one of the co-authors. Although the comparison is quite good for these surface values, I am surprised that no comparison was made for the entire water column. Near-bottom concentrations are extremely important when considering sedimentary inputs. For example, modelled dFe concentrations close to the seafloor should hopefully be in agreement with concentrations measured by Nielsdóttir et al. (2012). I would like to see such a comparison in the manuscript.

2) I feel that upwelling and glacial inputs are not considered well enough in the discussion. Recent work around glaciers alongside the coast of Antarctica reveals that glacial inputs could provide substantial amounts of dissolved iron (e.g. Alderkamp et al., 2012). I would like to see more discussion focused on these, and in particular, how the distinction can be made between sedimentary and glacial inputs. High concentrations of dFe originating from basal meltwater could also be laterally advected. How can you make the distinction in your model?

3) There is in my opinion another drawback of the manuscript which resides in the discussion of atmospheric inputs. The authors quickly mention that solubility and iron content are variable and could influence the magnitude of this term. This should be developed further, in particular using the work of Baker and Croot (2010).

4) This paper needs editing by a native English speaker.

3. Minor comments:

P. 10812, lines 21-24: please rephrase

P. 10813, lines 14-16: a reference is needed here to support this statement

P. 10814, lines 1-10. Sources like glacial inputs or upwelling should be mentioned here. Local sources of dust should be mentioned too.

P. 10816, line 5: could you please define the size of these particles?

P. 10816, lines 22-23: could you justify your choice of iron content and solubility? If you look at Baker and Croot (2010, Fig.3) solubility can vary from $\sim$1 to 10% in the Southern Ocean. How would this influence your results?

P. 10817, line 1: justify your choice of 1μmol dFe m-2 d-1

P. 10817, line 5: can you justify as well the chosen ligand concentration?

P. 10817, line 22: Chelton et al.,1998 is missing from the reference list

Fig. 8: would be nice to see a similar comparison for 30-100m depth for example

4. Literature cited in this review:

Gerringa L.J.A, Alderkamp, A-C., P. Laan, C-E. Thuróczy, H.J.W. de Baar, M.M. Mills, G.L. van Dijken, H. van Haven, K.R. Arrigo (2012). Iron from melting glaciers fuels phytoplankton blooms in the Amundsen Sea (Southern Ocean): iron biogeochemistry, Deep-Sea Research II, 71-76, 16-31

Interactive comment on Biogeosciences Discuss., 10, 10811, 2013.