1. General Comments

Death et al., investigate impacts of glacial iron sources on Southern Ocean (SO) primary productivity (PP) using the MIT global ocean model. They demonstrate that incorporating glacial iron sources into the model elevates simulated SO PP largely compared with an experiment only considering dust iron source, and conclude that the glacial sources are important for iron cycle in SO. The subject fits well within the scope of Biogeosciences. The method is novel; to my knowledge, this is the first study incorporating subglacial meltwater iron flux into numerical models. Their conclusions, however, are not sufficiently supported by their results, and there are several important concerns. A substantial revision is needed to make this manuscript suitable for publication.

2. Specific Comments

2.1 Sedimentary iron

Number of studies (Moore and Braucher, 2008; Tagliabue et al., 2009; Lancelot et al., 2009; Boyd et al., 2012) have pointed out that sedimentary iron is the most important iron source in SO, especially for near-coastal zone where the authors emphasize importance of glacial iron sources. Lancelot et al. (2009), for example, estimated that roughly 90% of iron is supplied from sediment for the Southern Ocean South of 60S. Ignoring sedimentary iron source lowers simulated dissolved iron concentrations and strengthens iron limitation in SO, leading to an overestimation of influence of glacial iron sources. I therefore guess that the estimated impact of glacial iron sources is overemphasized. I recommend to consider sedimentary iron in the control case. There are several ways introducing sedimentary iron into the model; putting a constant iron flux in model grids shallower than a threshold depth (e.g., Moore et al., 2004; Parekh et al., 2008; Lancelot et al., 2009) will be enough for estimating its first-order impact. It is technically very easy.

2.2 Model-data comparison

There are little comparison between the simulated results and observed data. Figures 1b and 1c compare simulated dissolved iron concentrations with observed data; but they are just visual comparison. Making scatter plots and calculating statistics (Correlation coefficient and RMSE …; e.g., Lancelot et al., 2009; Misumi et al., 2013) will enable us to evaluate relative fidelity among simulated cases quantitatively. Arrigo et al. (2008) presented spatial pattern and seasonality of SO PP based on satellite data. How do the simulated PPs compared with their estimate?

2.3 Regional impact

I guess that influence of the glacial iron sources is much smaller than that of sedimentary iron source. However, glacial sources may be important regionally because of their heterogeneous distribution and characteristic transport process (Fig. 1a). Analyzing spatial patterns of anomalies (both for dFe and PP) from the control case (dust + sediment) will reveal which regions are more susceptible for the glacial iron inputs.

2.4 Organic ligands

Iron complexation with organic ligands prolongs residence time of iron in seawater and increases potential of transport. For hydrothermal and fluvial iron sources, several studies have suggested that concurrent release of iron and organic ligands may enhance the transport potential (Bennett et al., 2008; Toner et al., 2009; Nishimura et al., 2012). If there is such mechanism for subglacial meltwaters, discussing it will strengthen the importance of glacial iron sources.

3. Technical Corrections

1. Abstruct

   I think the estimated value of 0.07-1.0 Tg yr^{-1} in abstract shoud be 0.07-0.2 Tg yr^{-1}. In my understanding, the values 0.07-1.0 Tg yr^{-1} come from Table 1, sum of iron fluxes of subglacial meltwater (0.009-0.90 Tg yr^{-1}) and icebergs (0.065 Tg yr^{-1}). In p. 12554 L. 8, the authors mentioned that iron export in subglacial meltwater is ~ 0.009-0.09 Tg yr^{-1} (the upper value is an order smaller than that in Table 1). The value in body of the text will be correct because a range of the estimated flux comes from SGM-Fe concentrations = 3 and 30 µM.

2. P. 12555 L. 12-13

   Explanations of iron cycle in section 3.1 in Supplement should be added in body of the text.

3. Supplement Section 3.1

   In Parekh's model, the total ligand concentration ($[L_T] = [FeL] + [L']$) and conditional stability constant (K_{FeL}) are constant values. The same would be true for the model used in this study. Description

   "… determined by the total number of ligands available (where the maximum ligand concentration = 1 nM) and the stability …"

   seems wrong because the total ligand concentration is always 1 nM. It should be

   "… determined by the total ligand concentration ($[L_T]$ = 1 nM) and the stability …".

4. P. 1255 L. 18-19
   The authors mentioned that “We assume that the Fe input via subglacial meltwater is Fe(II)”, but there is no distinction between Fe(II) and Fe(III) in the model.

5. Fig. 1
   They are really hard to see. For Fig. 1a, I recommend to add a bar chart showing regionally integrated values of subglacial meltwater fluxes. This will enable us to infer which regions are more likely affected by subglacial meltwater fluxes (see also 2.3 above). Characters representing longitudes in Figs. 1b and 1c should be put outside of the panels; I mistook these characters for circles representing observed data.

6. Figs 2 and 3
   I recommend to present anomaly (difference from the control case, dust + sediment) fields for cases with the glacial iron sources instead of showing the absolute values.

7. Supplementary Figure 3
   The dust only case looks more skillful than the other cases. If the authors argue that glacial iron source provides one plausible explanation for very high seasonally observed PP in near-coastal zone, then authors should present evidence based on data.

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