Response to Reviewers' comments on Death et al "Antarctic Ice Sheet fertilises the Southern Ocean"

We would like to thank both reviewers for their positive and thought provoking commentary. We provide a response to their comment as follows:

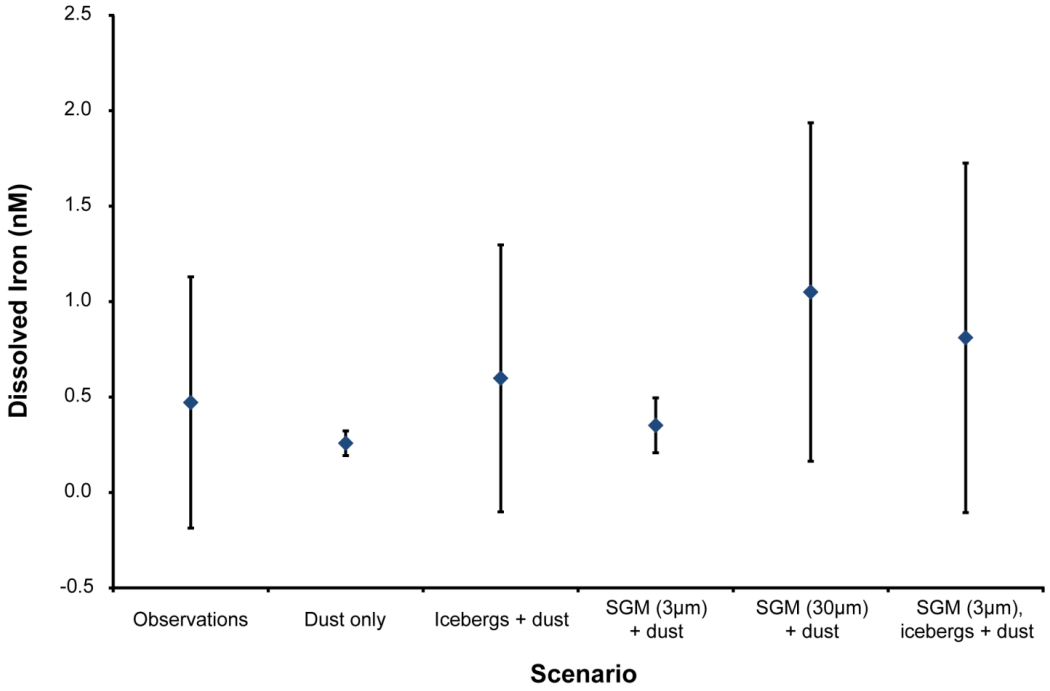
1. Figure 1 - "The measured iron in Fig 1 is very hard to see. More informative would be cross plots showing model-observations with both dust alone, and dust + SGM + icebergs. Does scenario E really do a better job of capturing the observations.
2. There is some evidence to suggest seasonality in iron concentrations around Antarctica (see papers by Sedwick in particular), with high concentrations after ice melt and then depletion through the summer. If the model results are an annual average, and the observations are from single time-points, this needs to be taken into consideration when making a model-observation comparison.

We have now increased the size of the plots in Figure 1 showing the ocean iron concentrations, by reproducing them as a larger, stand-alone figure (Figure 2). Observations of iron concentrations in the Southern Ocean are very limited (as the reviewer acknowledges in point 3 below) and refer to specific points in time and space through the year, unlike the model results which are annual averages. Hence, we do not attempt a correlation between the two, but comment upon general patterns and variability. In order to address the reviewer's comments that the paper lacked more quantitative comparison between our modelled results and available data, we have now included some more detailed summary statistics. The results of this analysis are presented in an additional Figure (Supplementary Figure 3), with the data provided in a table (Supplementary Table 2). Because of the limitations of the data, we do not make a judgment that any single model scenario does a better job. However, the magnitude and variability of iron concentrations in the Southern Ocean are best matched by those scenarios which include additional glacial iron inputs, as opposed to the dust-only run. The latter shows a very limited range and generally lower iron concentrations in surface ocean waters than glacial scenarios B, D and E (see Supplementary Table 2 below). The spatial pattern of measured iron (Figure 2) points to a coastal source being important. There are a number of possibilities for this coastal iron source, of which glacial meltwater and icebergs are two.

Supplementary Table 2 Summary statistics (mean, RMS, standard deviation) for observed (Tagliabue et al, 2012) and modelled surface ocean (0-100 m) iron concentrations

Scenario	Mean (nM)	Standard Deviation	RMS
Observations	0.47	0.66	n/a
A-Dust only	0.26	0.06	0.65
B-Icebergs and dust	0.60	0.70	0.87
C-SGM (3 μM) and dust	0.35	0.14	0.63
D-SGM (30 μM) and dust	1.05	0.89	0.90
E-SGM (3 μM), Icebergs and dust	0.81	0.92	1.03

Supplementary Figure 3 Summary statistics (mean and standard deviation) for observed (Tagliabue et al, 2012) and modelled surface ocean (0-100 m) iron concentrations, illustrating the differences between the modelled scenarios.



3. While Fe data are relatively sparse, making model-observation comparison difficult, the satellite record of chl is much more detailed by comparison (though far from perfect in this region). I was therefore very surprised that a comparison between modelled and satellite-derived surface chl was not included. This could be done both annually (Fig 2) and seasonally (Fig 3). This is important because it's certainly possible that annual PP in the near coastal zone is in fact not limited by iron but is limited by light (ice cover, day length).

Our decision not to include a modelled chlorophyll field to compare against satellite derived chlorophyll or PP data is based on the significant issues with converting satellite-based estimates of phytoplankton into PP. This is a particular issue in the Southern Ocean with satellites underestimating chlorophyll-a concentrations (Kahru and Mitchell, 2010; Guinet et al., 2013; Johnson et al., 2013). There are several reasons for these issues. First, the standard satellite algorithms used to calculate the chlorophyll concentrations are parameterised for temperate and tropical regions, and therefore do not account for the unique and dynamic nature of the Southern Ocean chlorophyll profiles (Johnson et al., 2013). Second, persistent cloud cover and patchy sea-ice make satellite data retrieval challenging (Guinet et al., 2013). Third, the satellites are only able to return surface water productivity and are therefore unable to measure any sub-surface chlorophyll profiles. The latter is important in the Southern Ocean since fluorescence maxima at depth have been found both in the frontal zone of the ACC (Queguiner and Brzezinski, 2002) and near the ice edge (Waite and Nodder, 2001). Our modelled output for PP is integrated over the top 100 m of the ocean in order to account for this sub-surface productivity effect. Hence, this also limits the utility of making a comparison to satellite-based estimates of surface PP. Although we use the satellite derived product in

Supplementary Figure 4 to show that the modelled pattern of PP seasonality concords with that indicated by satellite observations, we were not surprised that our modelled results were higher. Our model results of PP are consistent with the recent results of two studies that find that the MODIS and Sea-wifs estimates are underestimating the chlorophyll by 50 % in their study regions (Guinet et al., 2013; Johnson et al., 2013)

We feel that the seasonality of the iron measurements will not be driven by the sources, as there should not be a strongly seasonal component to the subglacial meltwater output or the release of icebergs. The measured seasonality in the iron observations is more a consequence of sea-ice retreat allowing sunlight to reach the nutrient rich waters and under-utilized bioavailable iron to stimulate primary productivity. Through the Antarctic summer season this will result in a drawdown in nutrients including iron. The model includes this biological uptake of iron when there is primary productivity.

Minor points:

1. **The sentence beginning on p. 12554 line 12 is way too long.** We have shortened this sentence in the main manuscript (Page 3, line 74)

2. **Curious why WOA01 is used and not 09** We used WOA01 since we were using the model scheme that was published by Monteiro et al., 2010, which included these fields and were used to configure the model. This is now stated in the manuscript (Page 3, Line 87)

3. **p. 12556 line 20. Presumably some of the iron that upwells will also be lost during mixing with surrounding water during the upwelling- another loss that is not accounted for. It's not clear why the iron isn't released into a subsurface layer- what is the technical difficulty?** Most erosion of icebergs occurs by wave action and hence, the losses would be greatest at the ocean surface. We agree that the iron released from subglacial meltwater would be initially released at sub-surface levels. However, the subglacial meltwater would be naturally buoyant in seawater, as it is fresher, and would rise to the surface. The buoyancy of these freshwater plumes has been modelled and has been observed to cause sub-ice-shelf channels (Jenkins and Doake, 1991; Gladish et al., 2012; Le Brocq et al., 2013). Subglacial meltwater would rise to the ocean surface within a single grid cell, since the model resolution is approximately 100km. Hence, we made the simplifying assumption that the introduction of this iron was at the surface (see Page 5, line 144). We recognize that during this process there would be losses through mixing and scavenging.

4. **p. 12557 line 24. It's unclear, do you mean the sediment density of the debris layer?** We have amended this to "sediment content" (Page 6, line 174).

5. **p. 12557 line 28. What photographic evidence? How would debris get into englacial layers?** We have now included a reference for the photographic evidence in the main manuscript (Page 6, line 179). There are two main mechanisms by which sediment can be incorporated englacially within Antarctic ice, either through thrusting (Goldthwait, 1951) or basal freezing (Weertman, 1961). The entrainment of debris through these mechanisms has been observed in West Greenland (Larsen et al., 2010).

Reviewer 2

1. **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.**

While we recognise that sedimentary iron is missing from our iron budget for the SO, we feel that there is not a suitable parameterisation of iron from shelf sediments in this region at present. All estimates of marine benthic sediment iron fluxes derive from a single study (Elrod et al, 2004) based upon the California Shelf in the USA. Previous estimates of the benthic iron flux to the Southern Ocean (eg. 21 Gg Fe yr^{-1}, Lancelot et al, 2009; 260-640 Gg yr^{-1}, Tagliabue et al., 2010) are also based upon this value, due to a lack of data from more representative Southern Ocean sites. A newer version of the MIT model has recently been developed and does include sedimentary sources of iron, also employing the Elrod et al. (2004) iron flux of 4.3 µmol Fe m^{-2} d^{-1}. We spent some time running this model for our different scenarios and found that PP is overestimated in the Southern Ocean, to the extent that little iron limitation observed. This indicates that the parameterization of sedimentary iron based upon Elrod et al, 2004, may not be appropriate for the Southern Ocean.

Second, none of the above estimates (Tagliabue et al, 2010, Lancelot et al, 2009) of the Southern Ocean sediment iron flux takes into account export efficiency. Iron is most efficiently delivered from sediments to surface waters in areas of upwelling at the continental shelf margin. Estimates of the efficiency of iron delivery from sediments to surface ocean waters vary by 10-50 % (Siedlecki et al, 2013). Elrod et al (2004) estimates of export efficiency range from 2.5 to 30 %. This highlights that there is great uncertainty surrounding the fate of iron released from the sediment. Raiswell and Canfield (2012) estimate a value for export efficiency of 5 %, applied to the Tagliabue et al, 2010 benthic flux and therefore calculate 13-32 Gg yr^{-1} of iron sediment fluxes to the SO. This value suggests that the sedimentary iron flux may be considerably lower than that from either dust, icebergs or our upper subglacial meltwater case (Table 1, main manuscript). Third, there is uncertainty in the estimates of the shelf generating area around the SO, and the relative influences of oxidation and removal to particulates as compared to ligand complexation (and retention of solubility). To this end we feel that the state of the science is that we cannot adequately model sedimentary iron fluxes to the surface waters around Antarctica at present. However, we now acknowledge the potential importance of sedimentary sources of iron in the Southern Ocean, and have now added some text within the main manuscript (Page 4, line 120). We also include the sedimentary iron fluxes presented in Raiswell and Canfield (2012) in Table 1 for comparison with the glacial iron fluxes (also see Page 3, line 70). We also make it clear that our dust-only run is a minimum estimate of non glacially stimulated PP because of the lack of inclusion of sedimentary iron in the model (Page 4, line 122).

2. **Model data 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?**

Please see our comment in response to a similar point made by reviewer 1. We did present the seasonal variation in PP, comparing it to the Arrigo et al, (2008) estimates, see Supplementary Figure 4.

2. Regional impact. 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.

We acknowledge this point, but feel that the spatial patterns of PP increase in the glacial iron cases (relative to the dust) are very clearly shown by Figures 3 and 4 of the main manuscript. Additionally the coarse resolution of the physics model means that any mesoscale variability in the PP is not captured. It is the combination of these mesoscale processes and glacial iron sources that lead to the patterns of variability, and thereby identifying the regions more sensitive to glacial inputs would be difficult.

3. Organic ligands Iron complexation with organic ligands prolongs residence time of iron in seawater and increases potential of transport. If there is such mechanism for subglacial meltwaters, discussing it will strengthen the importance of glacial iron sources.

This is an interesting point. Unfortunately, there are no data on the presence of organic ligands in subglacial meltwater at present. The iron from the icebergs is present as a nano-particulate iron phase which is also bioavailable (Raiswell et al., 2008). The bioavailability of this iron is preserved whilst entombed in ice, providing a mechanism by which bioavailable iron may be transported long distances and released to ocean waters far from the Antarctic Ice Sheet margin. This is mentioned on Page 3, line 77.

1. Value of PP in the abstract is incorrect. We agree that the value in the text is correct and that the subglacial meltwater flux is 0.009 to 0.09 Tg yr-1. We have amended this in the abstract.

2. Explanations of iron cycle in section 3.1 in Supplement should be added in body of the text We have added the missing parts of this text to the main manuscript as the reviewer suggests.

3. Description of the ligand parameterization in the model. This has been amended (Page 4, line 104)

4. 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. We have now changed the text to improve clarity upon this point, and just refer to "dissolved iron" as opposed to Fe II (Page 4, line 119).

5. Figure 1 – difficulty of seeing the points and confusion of longitude values with data points. Suggestion to add a bar graph showing regionally integrated values of subglacial meltwater fluxes. We hope that we have now addressed this issue by making the plots of Southern Ocean iron concentrations stand-alone figures. We have also removed the longitude legend as the reviewer suggested. Please see Figure 2.

6. Present Figures 2 and 3 as anomaly maps. Please see our response to comment 2 (Reviewer 2)

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

We present this figure primarily in order to demonstrate that the model simulates the annual cycle in Southern Ocean primary productivity well. The absolute magnitude of the observed seasonality of PP is likely to be highly underestimated, based upon the finding that the satellite estimations of chl-a using the standard SeaWiFs or MODIS algorithms are underestimated by a factor of 2-3 times (Kahru and Mitchell, 2010; Guinet et al., 2013; Johnson et al., 2013). We believe we do present evidence that would support a glacial contribution to PP in the coastal zone, by referring to direct measurements of PP in summer in these regions which are of the order of several 1000 mg C m^{-2} d^{-1} (Smith and Nelson, 1990; Smith et al., 1996; Smith et al., 1998; Arrigo et al., 2008; Westwood et al., 2010; Gerringa et al., 2012), compared with dust-only simulated values of less than 300 mg C m^{-2} d^{-1} (Figure 4, Supplementary figure 4). Please see Page 7, line 215 and page 8.

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