

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

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We thank the second referee of our manuscript for the constructive comments. Our Author Replies are labeled AR, and will follow each Reviewer's Comment (labeled RC). To clearly distinguish between Figures presented in the discussion paper (DP) from those accompanying our replies, we will add the prefix DP or AR to the Figure numbers. For example, Figure DP-3b will correspond to Figure 3b of the discussion paper.

Major comments: RC 1) 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

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dust inputs. I would like to see more discussion on these other sources.

AR 1) Figure DP-10 (i.e., the schematic overview of iron sources at South Georgia) aims at presenting the complexity of the iron cycle at South Georgia. Several previous studies focussed mainly on the relative importance of dust and sedimentary sources, however, as one immediately realizes from the figure dust-deposition and sedimentary sources investigated here are not the only two. The statement that this study focuses on the main sources of dissolved iron around the island will be modified accordingly, to reflect the fact that we specifically considered sedimentary and aeolian dust.

South Georgia is heavily glacierized and almost 50% of its surface is covered with permanent snow and ice (Gordon et al., 2008). It is known that glaciers (similarly to sea-ice) trap deposited dust that is then released with melting but also directly erode the bedrock determining an additional source of iron (Gerringa et al., 2012; Raiswell et al., 2008; Lannuzel et al., 2007). Dissolved iron concentrations ranging between 0.4 – 1.31 nM measured by Gerringa et al., 2012 in the Admunsed Sea provide such evidence. To the best of our knowledge, this information is not available for South Georgia. However, as input of iron from glacier melt is independent from the other two sources considered here, its contribution would likely add up to our observations, mostly at the surface of the water column due to the positive buoyancy of melt water, and close to the island where most glacier-flour plumes are observed. In that case we could expect a slight improvement in our comparisons with underway dFe concentrations measured by Nielsdóttir et al., 2012 over the shelf of the island (Fig. DP-7B). However, due to the large variability in both types of measurements, in the future a dedicated modelling configuration including fresh-water fluxes and iron release from melting ice (see Taylor et al., 2013) could test the relative importance of sedimentary and glacier-related iron inputs. In the current model configuration, the two types of inputs cannot be resolved.

South Georgia is located along the path of the Antarctic Circumpolar Current, and acts as blockage to its flow. Close to South Georgia, the ACC fronts are forced to separate: the Polar Front steers north, while the SACCF veers south then east of the island

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(Orsi et al., 1995; Thorpe et al., 2002; Boehme et al., 2008). To the southwest of the island where the ACC diverges Whitehouse et al. (2008) have found evidence for upwelling. Instead, Brandon et al., 1999 described upwelling to the northeast of the island along a shelf-break front separating on-shelf and off-shelf waters. As mentioned in the manuscript, the current model resolution is not sufficient to fully resolve vertical processes that one could simulate using a finer spatial resolution (i.e., Young et al., 2011) or including tidal forcing. Moreover, as the model topography is smoothed (another aspect linked to resolution), vertical processes that are likely to occur due the interaction of the flow with bathymetry cannot be represented. Future studies could specifically address this aspect of the model, likely providing the means to quantify and discriminate between the contributions of lateral and vertical transport.

RC 2) 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.

AR 2) In Nielsdóttir et al., 2012, a vertical profile of dFe concentrations is available for November 2006 only. Two regions were sampled till the 1000m depth, respectively to the north (i.e., downstream, around 52.86° S 40.10°W) and south (i.e., upstream, around 55.21° S 41.24°W) of South Georgia. Despite the seasonal difference between the data presented in this work (January-February), and the available observations (November), we extracted modeled dFe concentrations from the two regions, with the objective of comparing deeper concentrations only, as the surface values were expected to be discordant due to different seasonal levels in primary productivity. In Fig. AR1 we show with lines model results, and with stars available observations. Colors denote the location of the stations (red: upstream of South Georgia, around 55.21° S

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41.24°W; blue: downstream of South Georgia, around 52.86° S 40.10°W). As seen in Fig. AR1, both upstream and downstream of South Georgia, modeled dFe concentrations increase with depth, and reach at depths of 200m, 500m and 1000m, concentrations that fall in the range observed by Nielsdóttir et al., 2012, but also by de Jong et al., (2012) in other regions of the Southern Ocean.

RC 3) 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?

AR3) The reply to this comment has been included in our AR1.

RC 4) 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).

AR 4) Although our experiments confirm results obtained from previous studies (Johnson et al., 2010; Meskhidze et al., 2007; Tagliabue et al., 2009) providing additional modelling evidence that dust deposition is a minor source of iron when compared to sedimentary sources (with little effect on phytoplankton growth), certainly one must keep in mind that in natural conditions dust-deposition, as well as its solubility and iron content may be very different from the estimates used in this study. For example, because dust outbreaks at the source dry areas (i.e., the Patagonian region) are episodic and variable in their magnitude (Johnson et al., 2010), in some years the dust-deposition climatology of Tegen and Fung (1995) is likely to misrepresent the spatial and temporal variability that one would observe in the vicinity of South Georgia. More-

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over, Johnson et al., (2010) show that variations in mineralogy, in particular CaCO₃ content, can influence the solubility of iron in dust, because CaCO₃ effectively buffers the acidification of mineral aerosols during atmospheric transport. Numerous authors identify acidification of aerosol as a key processes increasing solubility of iron during atmospheric transport (Baker and Croot, 2010; Johnson et al., 2010; Mahowald et al., 2005). Once in sea-water, a combination of post-depositional agents (Baker and Croot, 2010), including photochemical reactions, complexation with ligands and interaction with all size classes of biology (from bacteria to zooplankton, see also Tovar-Sanchez et al., 2007) can increase iron solubility. Where primary productivity is highest (i.e., the main bloom area), biology and higher ligand concentrations (Hunter et al., 2007) are likely to increase the contribution of iron from dust-deposition. Nevertheless, taking into account model results shown here (see DP-Fig. 8c), if solubility were increased by a factor of 5 (i.e., to 10%, which is the maximum value proposed by the second reviewer), over the main bloom area the contribution of iron from dust deposition would possibly rise to 0.05 nM, and hence still fall below the contribution of iron from sedimentary sources considered alone (i.e., 0.5 nM).

RC 5) This paper needs editing by a native English speaker.

AR 5)The revised manuscript will be checked and edited accordingly

Minor comments: RC 6) P. 10812, lines 21-24: please rephrase

AR 6)These sentence will be rephrased accordingly in the revised manuscript.

RC 7) P. 10813, lines 14-16: a reference is needed here to support this statement

AR 7)Brzezinski et al., 2001 and Campanelli et al., 2011.

RC 8)P. 10814, lines 1-10. Sources like glacial inputs or upwelling should be mentioned here. Local sources of dust should be mentioned too.

AR 8)The revised version of the manuscript will be updated accordingly.

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RC 9) P. 10816, line 5: could you please define the size of these particles?

AR 9)0-100 um and 100 um - 5 mm

RC 10)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 1 to 10% in the Southern Ocean. How would this influence your results?

AR 10)In the current model configuration we set iron content and solubility respectively to 3.5% and 2 %. The former is in line with the iron content utilized by Johnson et al., 2010 who specifically investigated Patagonian dust deposition to the Atlantic Southern Ocean. Solubility of iron in dust is not well constrained, because it depends on different factors (Breitbarth et al., 2010); in fact, several ranges for solubility are proposed (i.e., Baker and Croot, 2010; Mahowald et al., 2005). We used a value that is similar to previous modelling studies (1-2 %, i.e.,) but that also matches the value proposed by Jickells and Spokes (2001). Nevertheless, if in our study solubility was increased by a factor of 5 (i.e., to 10%, which is the maximum value proposed by the second reviewer), over the main bloom area the contribution of iron from dust deposition would likely rise to 0.05 nM, and hence still fall below the contribution of iron from sedimentary sources (i.e., 0.5 nM, see DP-Fig. 8c).

RC 11)P. 10817, line 1: justify your choice of 1mol dFe m⁻² d⁻¹

AR 11)The choice of the parameter is based on Aumont and Bopp (2006), who set this parameter to the value that minimized the RMS between the model and the data on the global scale.

RC 12)P. 10817, line 5: can you justify as well the chosen ligand concentration?

AR 12)0.6 nM is the canonical value that is used in biogeochemical models. This value is derived from the study by Johnson et al (1997). Now, this value is definitely not constant over the global ocean which is general increase from the bottom of the ocean toward the surface and from the southern hemisphere to the northern hemisphere.

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However, because we don't have data for ligands around South Georgia we decided to use this typical constant value. Certainly, using a higher value could make a difference and could especially increase the residence time of the sediment iron in the upper ocean. But we think that the main conclusion of the paper would not be different.

RC 13)P. 10817, line 22: Chelton et al.,1998 is missing from the reference list

AR 13)The reference will added accordingly.

RC 14) Fig. 8: would be nice to see a similar comparison for 30-100m depth for example

AR 14)As stated above, there aren't any dFe concentration measurements available for the South Georgia region for austral summer 2008 other than the underway measurements used in the manuscript, therefore, a comparison at greater depths is not possible. However, we provide a subsurface plot (at 100m) of modeled dFe (Fig. AR-2), which shows highest levels (>3 nM dFe) at grid cells close to the shelf region.

Literature cited in our replies, but not included in the discussion paper.

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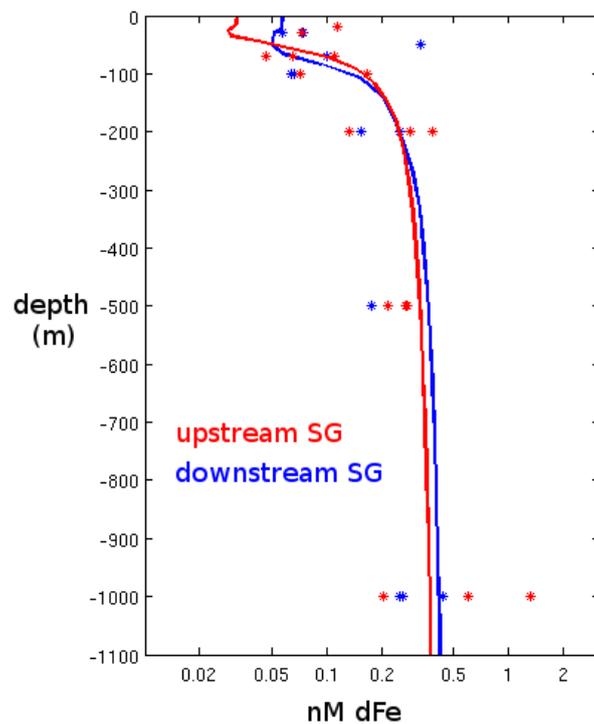


Fig. 1. Modelled (lines) and observed (stars) dFe concentrations downstream (around 52.86° S 40.10°W, in blue) and upstream (around 55.21° S 41.24°W, in red) of South Georgia.

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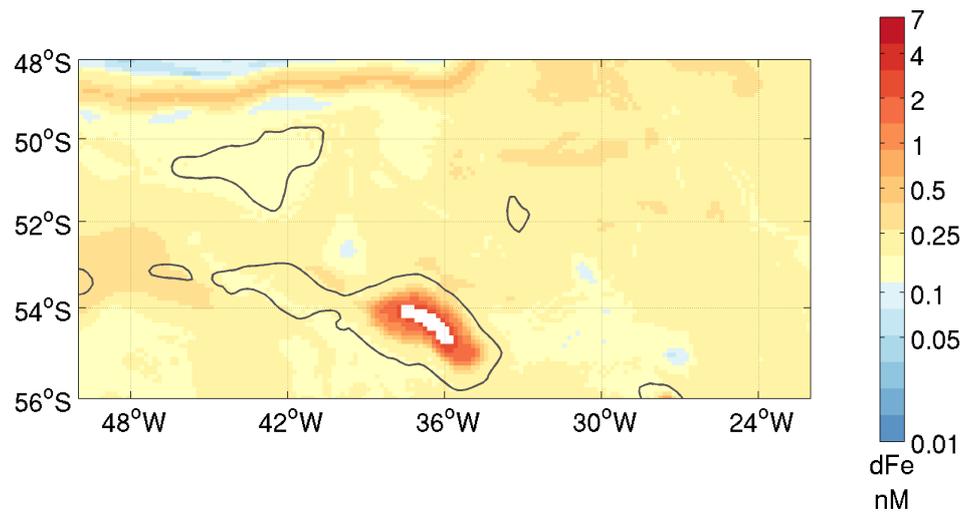


Fig. 2. Simulated dFe concentrations at 100m depth from January-February of the last modeling year (SEDDUST scenario). 2000m depth contours are also marked.

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