Response to Reviewer Robert Raiswell

Person Review by Raiswell. This is an excellent contribution and is entirely suitable for Biogeosciences. The authors have used a biogeochemical model to examine the delivery of Fe from the Antarctic Ice shelf. I agree with their statement that iceberg and ice shelf delivery have largely been ignored in other biogeochemical modelling studies and this is a welcome attempt to address this issue. The model produces some important new insights which will need validating in further studies, when appropriate data are available. I also agree with the authors that; 1) There is considerable uncertainty in the magnitude of all the different fluxes (and this applies just as much to atmospheric dust, as to the newer, less well-studied fluxes such as icebergs), 2) There are also difficulties in using the data to examine the down-stream impacts on productivity and export. The value of this paper is in recognising these issues and making sensible attempts to address them. I would hope that this study is used by the community to focus on the main areas of uncertainty, and stimulate further observational studies. Especially as a particular difficulty the authors faced is that there are few relevant iceberg data sets and, in fact, there are more observational data from the Greenland ice-hosted sources than from the AIS. I commend the authors on reviewing the literature so thoroughly.

I emphasize that I lack the expertise to comment on the models in detail but other comments below are keyed to page and line numbers.

We thank Robert Raiswell for his review and general support for our manuscript. We present our response in bold and preceded by '>' in case of formatting errors.

Page 2, line 3. The Raiswell reference is not the best as there are numerous studies of dust deposition to the SO. However the Raiswell data (I hope!) is more useful than many others because the extraction used relates to mineralogy, and specifically to ferrihydrite which is potentially the most bioavailable mineral form. You could move the Tagliabue ref to after 'SO' and before the colon, and then maybe cite a Boyd reference, perhaps the Mar Chem 2010 paper. The Raiswell reference would be better in the iceberg citations.

> Acknowledged and addressed

Page 2, line 25. Delete 'through finely ground rocks'. The rock source is larger than the dissolved sources but the latter is not negligible and may be the most bioavailable.

> Acknowledged and addressed

Page 2, line 27. Add 'fueling productivity in surface waters'.

We mean "delivering Fe".
We modified the sentence to clarify this point as follows:
"The melting of icebergs and ice shelves releases Fe to seawater as particulate, dissolved, and potentially dissolvable forms fueling the water column in Fe"

Page 3, line 3. Unfortunately the 50 samples are largely from Greenlandic icebergs and not Antarctica. Clarify this.

Clarified in the article

Page 3, line 14. The impacts on productivity are the point at which my biological expertise starts to fail. The impact critically depends on how Fe affects on productivity and thus carbon export. The authors obviously need to explore this issue but an expression of caution would be wise. Maybe add 'cycles, depending on how Fe inputs relate to productivity and carbon export'.

Expression of caution added in the article

Page 3, line 18. I welcome the attempt to consider vertical distributions of iceberg Fe and their influence on the surrounding seawater. No doubt the distributions will turn out to be very variable, not least because the vertical iceberg Fe contents will alter as icebergs overturns.

Page 4, line 25. 10% is OK but probably conservative. I would think that most ferrihydrite would be bioavailable, especially as ferrihydrite carries a significant fraction of ferrous iron. There is a brief discussion of this in my recent Frontiers paper, v. 6. No 222, doi: 10.3389/feart.2018.00222. You might be interested to look at this and at the EPSL 493, 92-101 paper by Hawkings. The Frontiers paper also raises the issue that ice is not inert and is able to catalyse the reduction of ferrihydrite. Also the freezing of sea ice produces pockets of Fe-enriched, chloride complexed brines that would be released early in melting. I am not suggesting that you need to cite these papers, I am only making the point, as you realise, that there are many areas of uncertainty which could profoundly alter the bioavailability percentage. It might be worth stating that you have not considered ice-water-mineral reactions.

We thank the reviewer for drawing our attention to these two papers.
Text modified

Page 4, line 23. Add wt.% after data.

> Acknowledged and addressed

Page 4, line 30. Reword as 'no observational data are available that allow the shelf Fe fluxes from Antarctica to be constrained, as..' There is a very crude estimate of 5.3 Gmoles/yr in Raiswell et al (2016)

Here we mean "Antarctic ice shelf Fe fluxes" and not "Antarctic shelf Fe fluxes".
Sentence reworded.

Page 5, line 5. It would be good to have a table showing the fluxes and solubilities assumed for dust, sediments and sea ice in the CTL model.

Fe fluxes from other sources simulated in the CTL experiment added in table 1.

Page 6, line 30. This states that the 1.5 and 6.3 nmol/L values are over and above the CTL data. Can the authors clarify what is being derived here? I think the models produce 'dissolved Fe' (see the discussion in the Raiswell Frontiers paper). In any event the data would have to be compared with seawater measurements on water filtered through 0.45 micron, which is 'dissolved Fe'. These model values would be at the upper limit of actual seawater 'dissolved Fe' concentrations outside of coastal regions.

The model values are concentrations in dissolved Fe.
Due to the poor availability of data in the Atlantic plume northeast of the Antarctic Peninsula, it is difficult to compare to real concentrations. However, it is true that these concentrations are probably at the upper limit of Fe concentrations in the open ocean but still potentially realistic in coastal regions (de Jong et al., 2012).

Page 7, line 9. Sentence unclear.

Sentence modified as follows:
"Furthermore, in winter, deep mixing entrained to the surface Fe that was released in summer below the euphotic zone and that escaped consumption by phytoplankton due to the lack of light."

Page 7 line 30. The caption to fig. 5 needs to clarify which are the positive and negative areas.

Caption modified in order to clarify this point.

Page 8 line 17. The potential of this deep reservoir is one of the important insights that your study produces.

Page 10, line 10 on. This seems reasonable. The whole point about icebergs is that they can transport, which is not true for ice shelf sources. But it is good to see this confirmed.

Page 10, line 24. My figure 8 shows the difference in surface Fe concentrations, not chlorophyll. Has a diagram been incorrectly inserted?

The right caption for Figure 8 is:
"Surface chlorophyll concentrations in summer (December, January, and February) from (a) satellite observations (MODIS-Aqua, Johnson et al., (2013)), (b) the CTL experiment, and (c) the SOLUB5 experiment in the Southern Ocean, south of 50° S.

Page 11, line 11. Delete 'the' before Bouvet island.

> Acknowledged and addressed

Page 13, line 30 on. I agree that this difference is hard to understand but you make a crucial point; that modelling the ice-hosted sources is at present difficult; although the attempt is certainly valuable (see above).

Page 15, line 25 on. Yes, delivery will vary as iceberg melting occurs.

Page 16, line 5. I would prefer to be cautious here and describe the most labile source as' potentially bioavailable'. But I agree that there will be a range of Fe mineral reactivities each with different rates of reaction or dissolution or grazing interactions, and thus different bioavailabilities.

OK replaced in the text.

Page 17, line 2. This is another useful finding, although again not unexpected that iceberg effects are spatially variable.

References

de Jong, J., Schoemann, V., Lannuzel, D., Croot, P., de Baar, H., & Tison, J.-L. (2012). Natural iron fertilization of the Atlantic sector of the Southern Ocean by continental shelf sources of the Antarctic Peninsula. *Journal of Geophysical Research: Biogeosciences*, *117*(G1), n/a-n/a. https://doi.org/10.1029/2011JG001679

Johnson, R., Strutton, P. G., Wright, S. W., McMinn, A., & Meiners, K. M. (2013). Three improved satellite chlorophyll algorithms for the Southern Ocean. *Journal of Geophysical Research: Oceans*, *118*(7), 3694–3703. https://doi.org/10.1002/jgrc.20270