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Interactive comment on "Sensitivity of ocean biogeochemistry to the iron supply from the Antarctic ice sheet explored with a biogeochemical model" *by* Renaud Person et al.

Anonymous Referee #1

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Review 'Sensitivity of ocean biogeochemistry to the iron supply from the Antarctic ice sheet explored with a biogeochemical model'

The authors present a model study investigating how Fe input into the Southern Ocean from icebergs and the Antarctic Ice Sheet affects the distribution of Fe and primary production in the marine environment. Recognizing the uncertainty in the magnitude and nature of these Fe sources, and thus several difficulties in meaningfully parametrizing them to date, the authors opt to model several scenarios with important differences in Fe solubility and the distribution of melt-derived Fe in the water column. The results, with respect to primary production and C export, fall within the (very broad) range of

other model studies suggesting a modest impact of this Fe on Southern Ocean productivity. A key strength of this specific study is that it makes considerable effort to highlight the many uncertainties surrounding this Fe source. Numerous other recent works have proposed much stronger effects but neglected to consider some, or all, of the uncertainties highlighted herein. Whilst there are a few areas in the text where I think some improvements can be made, I generally therefore consider this to be a valuable addition to the field, suitable for publication in BGS and, in my opinion, one of the most comprehensive manuscripts on the subject of modeling these Fe fluxes to date.

My expertise is in biogeochemistry, I defer to a more qualified reviewer for issues concerning details of the model used. Before returning the text to the journal, it would benefit slightly from a read through from an English editor.

General comment; have the authors considered the meltwater 'pump' effect outlined in some recent work (see comment on page 4, (Cape et al., 2019; St-Laurent et al., 2017, 2019)? I wasn't clear if this effect would be captured in the model or not.

General comment: How is C export scaled to primary production in the model, does the model successfully replicate the observed relationship between the two? Looking at some other models and calculations in the literature, it appears to me that a key reason why very broad ranges are often quoted for C export from specific Fe fertilization scenarios is simply because of the way Fe or productivity/chlorophyll a is scaled to C export. The 'high' C export estimate of (Duprat et al., 2016) is scaled linearly with chlorophyll/productivity –which is not consistent with observational Southern Ocean data. It is not clear to me if this is also a problem with the (Laufkötter et al., 2018) model which matches the Duprat calculation surprisingly well producing a fertilizing effect significantly above that found herein. (Observations with multiple methods show that C export efficiency declines sharply with increasing productivity in the Southern Ocean, although the precise reason(s) for this seem to be unclear (Maiti et al., 2013; Le Moigne et al., 2016)).

Specific comments by Page/line Title: Antarctic Ice Sheet 1/12 'seasonal variations' in the timing of melting? If I understand correctly, this sentence would read better 'Seasonal variations make almost negligible differences...'

2/3 Raiswell 2016 does not contain extensive atmospheric dust work, I am sure there are better values/references for dust deposition

2/14 'the mean flux'. You mean the total flux?

2/16 'the few modelling studies conducted to date'

2/27 'fueling surface waters'. You mean 'fueling' productivity or just delivering Fe?

2/28 Not sure this is accurate, it has been speculated that glacially derived Fe was fueling primary production in the Southern Ocean for some time e.g. (Hart, 1934), it just has proved very difficult to quantify.

2/32 See also (Wu and Hou, 2017) - a particularly interesting read as it, when compared to (Duprat et al., 2016) demonstrates that there are significant differences in observational data constraining the effect of icebergs, not just in the models.

2/34 'the Prydz bay'. Delete the

3/12 . . .will increase the supply of Fe. . . Assuming that the Fe input scales linearly with ice-melt, which may be a little speculative

3/18 'along the water column' means horizontal, you mean 'through'

4/10 Here something concerning the 'meltwater pump' may be relevant. High Fe concentrations adjacent to Icesheets (in the ocean) would generally be attributed to direct input from melt/sediment release etc, but release of meltwater can also 'pump' ambient to the surface and thus bring Fe from shelf sediments and the sub-surface Fe reservoir into surface waters. These effects are difficult to tease-apart from field data. But some model calculations suggest that the magnitudes of Fe from 'pumping' and from direct input (melt/freshwater/freshwater derived particles) are comparable – all be it with large

uncertainties. See (Cape et al., 2019; St-Laurent et al., 2017, 2019) for overviews of this effect and what we do/don't know about it.

4/23 It is not clear to me what the % here refer to, I guess the % weight of sediment which is ferrihydrite, but please clarify (also specifically what is the mean – a global mean??)

4/24 This is a muddled concept in the field in general. All labile Fe could be potentially 'biologically available' if processed/delivered in the right way, I would stick specifically with the 'soluble' fraction rather than trying to define a 'biologically available' fraction as this is an arbitrary exercise. The concept of 'utilization' (Boyd et al., 2012) is perhaps more useful as 'bioavailability' is a qualitative term.

4/30 Seems like an odd thing to say. 'no data allow the constraining of. . .' or 'allow us to'

5/30 The 'buoyancy effect' is widely attributed with bringing iceberg-derived components (e.g. particles/Fe) to the surface, but as far as I'm aware there isn't much clear evidence of it actually doing this, or even much data to show how ice melt behaves in the real world. An alternative argument is that something akin to convective cells develop up the sides of the iceberg, and that these reach neutral buoyancy before they reach the surface i.e. most melt doesn't 'rise' to the surface. In any case, there is certainly very limited data to show how ice melt behaves around icebergs (Helly et al., 2011; Stephenson et al., 2011).

6/30 How do these concentrations compare to 'real' Fe concentrations in these areas?

7/9&10 This line 'Furthermore, in winter,. . .' does not make sense

7/6 These concentrations are not feasible, how is scavenging constrained? Such a high dissolved Fe concentration (27 nM) would, practically immediately, precipitate.

3.1.4 Whilst the effect is poorly defined, the meltwater 'pump' should be at least mentioned here.(St-Laurent et al., 2017, 2019)

10/33 The mains

11/11 the Bouvet Island. Delete 'the'

11/20 Nevertheless, though small

11/25 CHL at the blooming season, you mean 'throughout' or 'during the season' (general comment CHL is, at a glance, similar to CTL, maybe use 'Chl a' or similar)

12/30 equal to

12/33 'are almost unchanged.' Compared to?

13/27 'leads to a significant increase in'

13/32 Indeed. The first thing I did after reading this study was to refer to (Laufkötter et al., 2018). I was very surprised to find that both studies use very similar parameterizations for the total Fe input. As a biogeochemist, my simplistic conclusion is therefore that these results (collectively) are not reproducible between models, as completely opposite conclusions are reached using practically the same Fe input. More surprising is that the results of the studies don't even overlap- given that both studies use very broad ranges in Fe input which were designed to span all environmentally relevant scenarios. This is problematic, because it makes the studies (again, collectively-this is not a specific critique of this study) impossible to interpret from a biogeochemical perspective. So the critical question is why is there such a large difference? The authors herein do a generally good job of discussing the differences between existing iceberg models, but perhaps this information (presently in the text) could be thinned a little and compiled in the form of a table which would at least eliminate some causes of differences between independent models. As a biogeochemist it is difficult to comment further other than to raise a flag that model results should be treated with extreme caution until some consensus can be found between different model studies.

14/2 Yes, but be careful here concerning 'regionally sig. C export'. Compare (Wu and Hou, 2017) and (Schwarz and Schodlok, 2009) with (Duprat et al., 2016), the later

study claims a much larger effect, but only in the C export calculated, I suspect this is largely because of how the observed data (chlorophyll) is scaled to C export and thus reflects different assumptions in the calculation rather than actual differences in the raw data.

14/29 Does (De Jong et al., 2015) not conclude that much of the Fe is sub-surface?

15/19 'runoff' [as a macronutrient source] this is a bit of a misleading statement, even in the North Atlantic, where macronutrient concentrations are much lower in the mixed layer, runoff dilutes the concentration of N and P macronutrients (Meire et al., 2016), so a missing macronutrient-runoff source couldn't plausibly explain the problem herein. Similarly ice contains very low macronutrient concentrations.

15/22 This seems more plaussible, see for example (Cape et al., 2019), although even these 'upwelled' nutrient fluxes would be modest and I doubt sufficient to explain the model problem-plus they would come with Fe. In these references here, I think the authors mean (Hopwood et al., 2018) rather than the Hopwood paper listed. Alternatively, how scavenging is accounted for in the model (a difficult thing to do) presumably could cause this effect, if Fe is removed a little too slowly, it will 'over-fertilize' in the model world and thus, all other things being equal, drawdown macronutrients much faster than would be the case otherwise. As noted, I am not a model expert, but I would guess that macronutrient distributions in the model match real data better than Fe distributions and thus would speculate that problems are more likely to arise from how Fe is parametrized than with macronutrient sources/sinks.

16/2 See also (Boyd et al., 2012) – specifically the 'utilization' of Fe shifts significantly along 'Iceberg Alley'

16/7 Perhaps, but then this becomes a question of organic ligands and to what extent these are able to transfer Fe into the dissolved phase. I'm not aware of any work around icebergs looking at ligand-iceberg interactions, but this has been investigated with respect to glacially derived particles, for general discussion of how ligands may limit the transfer of Fe between labile particulate and dissolved phases see (Hopwood et al., 2016; Lippiatt et al., 2010; Thuroczy et al., 2012)

16/13 I think these fluxes have been defined, Raiswell (et al.,) has conducted very extensive work on the different fractions of Fe present in glacially derived particles (Raiswell et al., 1994, 2010; Raiswell and Canfield, 2012) and what this means for lability. It was this early work, to my understanding, which lead to the more recent focus on the labile ferrihydrite fraction – because this is, to a first order approximation, the labile sedimentary Fe fraction which may plausibly affect primary production.

17/10... onwards. Given that models cannot agree on how important Fe-fertilization is in the present, how can you robustly conclude that the Fe source will increase in the future? I'm not sure the authors present anything that supports this statement and think the conclusion would be stronger without it. It is (unless you can produce literature to support this) presently an unsupported argument that increasing discharge will increase Fe fertilization.

Figure 1: Just to clarify, on (b) the 'day-1' means as if the flux was uniform across the year (i.e. an annual value divided by 365)? This seems a little strange way of displaying the data as presumably the actual melt rate during summer is much larger than this and for much of the year it is 0.

Figure 3: What does the white area correspond to? Maybe define, I guess something like no meaningful change?

Figure 5: I assume the colour bar should be the same as 4?

Figure 8. The caption for this figure seems to be completely incorrect.

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