Response to reviewer #1

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 modelling 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.

We thank reviewer #1 for his detailed review and general support for our manuscript.
We present our response in bold and preceded by '>' in case of formatting errors.

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.

In our model configuration, the cavities below the ice shelves are not opened. To mimic the overturning circulation driven by these unresolved ice shelves, we used the parametrisation of Mathiot et al. (2017) which prescribes a meltwater flux of ice shelf uniformly distributed over the depth and width of the unresolved cavity opening, from the mean ice front draft down to the seabed, or the grounding line depth if it is shallower. Mathiot et al. (2017) showed that this parametrisation of the ice shelf melting drives a buoyant overturning circulation along the coast, i.e. the meltwater pump, similar to that simulated by cavities when they are explicitly resolved.

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).

We completely agree with this comment. But in the actual context of non-consensus about the export ratio in the Southern Ocean, it is very difficult to estimate whether our model replicate "realistically" the observed relationship between C export and primary productivity due to the poor data spatial and temporal coverage. In our model, the relationship between PP and C export does not show a linear pattern as illustrated in Figure 1. Nevertheless, there is a clear trend that shows higher export with higher primary productivity which is highly variable at the local and temporal scale. We don't know if in the COBALT model used by (Laufkötter et al., 2018), the relationship is different which could explain the differences. In fact, a detailed and thorough comparison with that study is really challenging because we lack many information that would be necessary. These differences are really intriguing and would probably deserve a careful analysis involving a collaboration between the two groups.

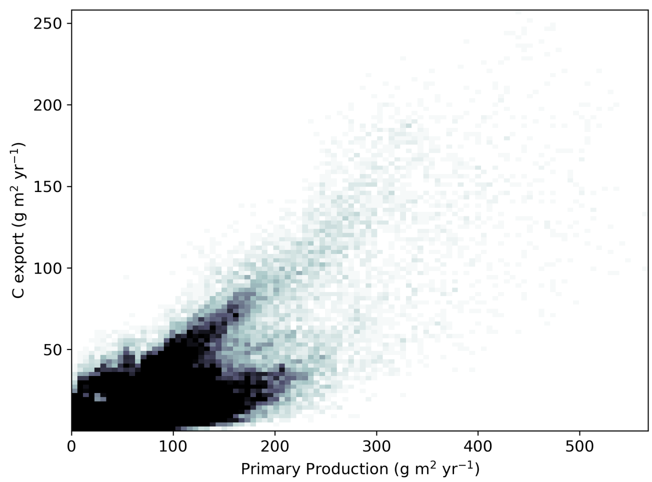

Fig 1. Density relationship between primary production and C export at 150 m depth over the Southern Ocean, south of 50°S, in the SOLUB5 experiment.

Specific comments by Page/line Title: Antarctic Ice Sheet

> Acknowledged and addressed

1/12 ‘seasonal variations’ in the timing of melting? If I understand correctly, this sentence would read better ‘Seasonal variations make almost negligible differences...’

To clarify a possible misunderstanding, we modified the sentence as follows :
“seasonal variations of the iceberg Fe fluxes have regional impacts which are small for annual-mean primary productivity and C export at the scale of the SO”

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

Other references cited for dust deposition.

2/14 ‘the mean flux’. You mean the total flux?

We mean the total mean flux. Modified.

2/16 ‘the few modelling studies conducted to date’`

> Acknowledged and addressed

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

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”

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.

We thank reviewer #1 to introduce this reference.
Text modified in accordance.

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.

OK.

2/34 ‘the Prydz bay’. Delete the

> Acknowledged and addressed

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

We agree with this comment.
Sentence changed as follows:
“The projected decline of the AIS will potentially increase the release of Fe from icebergs and ice shelves in the SO with possible significant impacts on marine productivity and biogeochemical cycles, depending on how Fe inputs relate to productivity and carbon export.”

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

> Acknowledged and addressed

4/10 Here something concerning the ‘meltwater pump’ may be relevant. High Fe concentrations adjacent to Ice sheets (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.

Please, see our answer to general concerns.
Text modified to detail that the parametrisation of the ice shelf melting from Mathiot el at. (2017) simulates the buoyant overturning circulation along the coast and the associated meltwater pump.

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??)

wt.% added after data
Here we mean the mean content of the estimated range from Raiswell et al. (2016)
Modified in the text.

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.

We agree with Reviewer #1 that the concept of bioavailability is rather vague. Bioavailability depends on numerous factors such as the nature of the iron particles, the interactions with the ligands, the environmental conditions, … As a consequence, the fraction of iron that can be ultimately available to phytoplankton (and bacteria) is highly variable and very difficult to infer. Boyd et al. (2012) have studied the Fe utilization by phytoplankton based on observed Fe/Chl ratios. They compared this utilization to the magnitude of different sources (dust, sediment resuspension/mobilization, meltwater, …) to evaluate if these sources are related to a higher Fe utilization. The concept of utilization is thus very useful to qualitatively investigate the potential fertilization effect of different iron sources. However, this remains qualitative and based on many assumptions, among which the values of the Fe/Chl ratios are among the highest. Furthermore, the comparison to supply mechanisms still requires the definition of a bioavailable iron fraction to evaluate the magnitude of the sources. Finally, in a prognostic model, utilization is prognostically predicted based in part on the amount of iron that is available which turns back to the definition of bioavailable iron.

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

Sentence reworded as follows:
"no observational data are available that allow the ice shelf Fe fluxes to be constrained, as the Antarctic estimates from..."

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).

OK. We thank the reviewer for drawing our attention to these papers.

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

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, these concentrations are probably at the upper limit of Fe concentrations in the open ocean but potentially realistic in coastal regions (de Jong et al.,

2012).

7/9&10 This line ‘Furthermore, in winter,: : :’ does not make sense

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.”

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

In coastal regions, Fe concentrations can be very high as shown in the article of de Jong et al. (2012) with measured surface Fe concentrations up to 50 nmol L^{-1}.

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

Text modified to mention the meltwater pump.

10/33 The mains

> Acknowledged and addressed

11/11 the Bouvet Island. Delete ‘the’

> Acknowledged and addressed

11/20 Nevertheless, though small

> Acknowledged and addressed

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)

We mean during the season. Modified.
We choose SChl for surface chlorophyll concentrations instead of CHL

12/30 equal to

> Acknowledged and addressed

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

Compared to the CTL experiment.
Added in the sentence.

13/27 'leads to a significant increase in'

> Acknowledged and addressed

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.

Please see also our answer to the general concerns.
We totally agree with your last comment and we will modified the conclusion section in accordance to this point.

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?

Yes, this is their main conclusion regarding the iceberg Fe delivery.

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.

Supply mechanism removed.

15/22 This seems more plausible, 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.

Reviewer #1 is correct in the fact that macronutrient distributions are better simulated by models, including ours, than Fe distribution. This is illustrated in the reference paper of PISCES (Aumont, et al., 2015). Models tend to have difficulties at properly simulating the iron distribution in the ocean ((Tagliabue et al., 2016) even if PISCES tends to perform quite well in comparison with other models that participated to the FeMIP exercise. The drawdown of nutrients close to the coast is explained by an intense primary productivity that drives an intense export of carbon and nutrients. Due to the lack of data, primary productivity is difficult to evaluate as well as chlorophyll values. We have to rely on satellite-retrieved values which may be biased in that specific region and in areas closed to the coast. This comparison indicates that we don’t hugely overestimates chlorophyll levels even if they tend to be too high on average. Thus, a too intense fertilization by iron may be part of the explanation, either because scavenging is too low and/or iron input from sediments and ice shelves is too large. Another probable reason is that export is too large and efficient in the model in that region. However, due to the lack of data, this proves to be impossible to investigate properly.

You are right, we mean (Hopwood et al., 2018).
Reference modified.

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

Effectively, their results suggest that the rates of iron utilization appear to be considerably less than that potentially supplied from iceberg melting along their drift. They also revealed the impossibility to evaluate to which extent because of the contributions from other sources of Fe (sediment and dust) in this region. Moreover, the Fe utilization was computed from the net primary production derived from satellite products which might be potentially severely biased. Indeed, in the Southern Ocean, satellite products were pointed out to particularly underestimate chl a concentrations (Johnson et al., 2013), and inferred net primary production are associated with very large uncertainties (Saba et al., 2011).

The shift in the "utilization" of Fe from iceberg of Boyd et al. (2012) has been added in this section.

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)

Ligands clearly control the amount of iron that can remain in the soluble fraction when particles released by icebergs and ice shelves dissolve in sea water. The studies mentioned by Reviewer #1 show that meltwater contains quite significant amounts of ligands that increase the amount of iron that can dissolve or remain dissolved. As a consequence, the apparent solubilization of glacial particles is controlled partly by these ligands. In our model, we don't include a potential source of ligands from meltwater because as said by Reviewer #1, we do not have any data to constrain that input. Thus, the ligands concentration in the vicinity of the icebergs is supposed to be identical to that of the open ocean. If meltwater is an important source of ligands, this would mean that our model is underestimating the supply of soluble iron from icebergs (and ice shelves).

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.

OK. We thank the reviewer for drawing our attention to these papers.

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.

You are right, however a mechanistic increase in the AIS supply will at least increase surface Fe concentrations in the model. We might better say that as no agreement are found between models in their biogeochemical response to the AIS Fe supply, it is for now impossible to evaluate the impacts of climate change on this external source of Fe and their consequences on marine biogeochemistry in the Southern Ocean. This also points to the necessity to understand the mechanisms that explain the very large differences that are simulated by the models.

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.

We modified Figure 1 to express the AIS Fe fluxes in kg m^{-2} yr^{-1}.

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

Caption modified as suggested.

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

It is the same colour bar as in Figure 4. Added to Figure 5.

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

Here, the right caption 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.

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