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Comment on os-2021-61

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Referee comment on "Evidence for iceberg fertilization of the NW Atlantic" by Grant Robert Bigg et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-61-RC2>, 2021

Review - **Evidence for iceberg fertilization of the NW Atlantic**

General comment:

The North Atlantic basin bloom has been widely studied, as far back as 1950s with Sverdrup testing his critical depth theory using Spring bloom data. This initial theory has since be challenged with work from Siegel et al 2002 and Behrenfeld a few years later. Nitrate is generally the main limiting nutrient in the North Atlantic since the Sarahan dust supplies enough Fe to surface waters for it not to be limiting. Iron can however sometimes be found at levels too low to support marine productivity, as shown in papers from Rijkenberg et al 2014 and Achterberg et al., 2018.

The Ice Sheets have been recognised as important sources of trace elements to the polar seas, with potentially a significant contribution to the global carbon sequestration. In the Southern Ocean the fertilization effect from large icebergs on local surface productivity can be seen from satellites recording ocean colour. In the North Atlantic, some work by Smith et al 2013 and others suggests that icebergs from the Greenland ice sheet may well play a role in boosting local productivity and this is the hypothesis that the authors are aiming to test here.

I think that topic is extremely interesting, whether the hypothesis is proven right or wrong. However I find the manuscript hard to follow and the execution not very convincing, which is unfortunate. I suggest below some different ways to approach the work and I hope you find some of my comments useful.

Introduction:

The introduction is needs to be approached differently. I would instead use the following structure: 1/ Known drivers are of the NA bloom. you can go as far back as Sverdrup 1953, then Siegel et al 2002, then Berehnfeld. Here what is important is a critical assessment of the role of the wind mixed layer depths and light in initiation of the blooms. The Siegel et al 2002 is especially key since it covers the area of interest that this paper focuses. Do note that 71 - 87% of Fe in NA comes from dust (from Sahara). 2/a section on macronutrients and Fe limitation in the NA, referring to the RijkenBerg work, then 3/ your hypothesis: icebergs may supply additional Fe to a localised region of the NA.

Note that lines 53-61 are already results/discussion and need to be removed.

Figure 1 needs work: a)we need lat/long and main landmarks. It would also be good to see the limits of the study area for satellite derived ocean color analysis, as well as detailed bathymetry. Here for exemple it looks like the enhanced Chla areas match shallow bathymetry, which makes sense. ie. coastal waters being more productive than offshore waters (from upwelling and/or local eddies). b) is confusing. The reader is not quite sure what they're looking at. I would remove it

Methods:

The Methods are confusing as they currently stand. The way I would approach the study is by comparing the climatology of Chla vs frequency of icebergs transiting through the study area. ie. in years that we have higher occurrence of icebergs passing through the study area, do we see anomalies in Chla records? You could stick to the bloom period, ie. April-May. and secondly are these positively and significantly correlated? I would set the scene that way lines 74-80, then explain how the Chla and icebergs data were obtained. One point to emphasize is that the data does not report size, which I think is key. ie. small icebergs are unlikely to alter the area of interest as much as a giant iceberg would.

I would move lines 75-80 to the "iceberg data" subsection, and 80-83 to the Chla subsection.

Line 82: Also it would be worth mentioning that polar areas are quite tricky with ocean colour - clouds decrease data coverage and large solar zenith angles can lead to an underestimation of the in situ chl a - see Sirjacobs et al., 2011.

Line 88: Please define the box/area of study as xx-xx°N and xxx-xxx°W - we need the same spatial limits for Chla and icebergs analysis otherwise we're comparing apples and pears

Line 92-94: Before applying the standardisation did you test the difference between MODISâAqua and SeaWiFS values in the area when co-located data are available. ie. what is the mean difference between the 2 products for your study area, and is this different statistically?

Line 101-103: Here instead you should record the flux of icebergs transiting through the defined study area from any direction - ie. why 48N? and where it that relative to the study area? We really need lat/long on Figure 1.

Line 163: this is a Southern Ocean reference

Line 165: is it really a time lag, or that the fertilisation effect can last for weeks? in the case of Fe, there is no way that this Fe would stay in surface waters for that long. Nitrate might (not that I have any references to support this statement), but not Fe. This idea is carried through the whole manuscript yet I cannot wrap my head around such a long Fe residence time.

Line 175-177: I am not fond of this approach. Region A is influenced by coastal processes,

which is not the case for the "iceberg" region. What I would do instead is simply run a climatology of the iceberg area as I suggested above. ie. do you see enhanced (above average) Chla the years that more icebergs transit through the area? noting that other processes (large scale weather forcings like NAO, eddies) may explain any anomalies in surface Chla

Figure 3: Again here, if you superpose bathymetry I think that you will see that the control area and iceberg area have completely different bathymetry, therefore physical and chemical settings.

Line 181-192: The drivers of the blooms will be different, even though you may have a similar end results. To really nail this down, you would need to run a climatology of bloom start, bloom end and bloom amplitude (and max) in both regions (see Arctic and Antarctic phytoplankton bloom phenology studies). Check e.g., / bathymetry, 2/wind mixed layer depths, 3/Sea surface height (i.e do you have eddies?), or as studied later the NAO.

If results are exactly the same then maybe yes you can say the 2 areas undergo similar environmental conditions (before adding icebergs influence to the mix). But at the moment, the approach is not rigorous enough.

Line 199-204- remove this text

Line 207: I am struggling with this section - I don't think that this approach holds particularly well - Why would you expect a temporal decoupling between icebergs passing and the Chla anomaly? Please explain, set the scene for the reader.

Results & Discussion:

Given the lack of rigorous approach to the study I have to admit that I did not particularly trust the results and discussion sections, nor spent considerable time on them, simply because they are not well founded.

In Figure 4 for example the signals are very patchy and it's difficult to say that weak correlations translate into direct causation. There are a lot of studies on the NA spring bloom that should be cited/explained before suggesting that icebergs drove these trends.

The section on fertilization is also weak - when you say fertilization, is it Fe? or is it that the icebergs create local mixing that may bring NO₃ up? Please explain. One thing to also highlight here is that not all Fe is bio-available. Plus you need enough Fe binding ligands in seawater to keep this Fe in surface waters. This is where the lag time is a really tricky concept to grasp.

Line 375-376: Is there any obvious trends in that flux? I would have imagined that maybe the flux of icebergs may have significantly increased since 1958. If that's the case, then this work could become really interesting from a climate change angle, ie. as the polar ice sheet continue to lose mass.

The whole section of Fe fluxes to the area needs tightening. Go source by source, using data from the literature to report possible Fe flux to the study area, ideally reported in $\mu\text{mol Fe}/\text{m}^2/\text{d}$. Please check the units and the conversions carefully.

Conclusions:

The conclusions are not really supported by the results. The authors are so keen to draw a line between icebergs and phytoplankton bloom, that they dismiss other possible drivers. This needs rewriting.

