Ocean Science Discussions

Ocean Sci. Discuss., author comment AC2
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Reply on RC2

Grant Robert Bigg et al.

Author 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-AC2, 2021

Reply to Reviewer 2

We welcome the reviewer's careful reading of our paper. **However, as with Reviewer 1, we believe that they have unconsciously exaggerated the confidence of our findings. We are claiming that despite the complexity of the region and its forcing we think there is sufficient evidence to suggest that icebergs probably do have an influence on production in the NW Atlantic, but that this is difficult to isolate. In other words, chlorophyll peaks have a range of causes, as the Reviewer rightly points out in their overview of the subject at the beginning of the review, but we are only proposing that an iceberg influence is discernible, rather than dominant, or even important. Our paper is more a call for targeted studies to investigate such a link, to confirm or deny our speculative findings. We would be happy to make it clearer that the purpose of the paper is to provide a first analysis of a complex question.**

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.

We would be happy to rewrite the Introduction towards the approach suggested, however, it does change the purpose of the paper to be explicitly about what explains chlorophyll behaviour in the NW Atlantic. This is changing the intention beyond what the paper is trying to do, which is see whether there is any evidence for icebergs to influence productivity in the North Atlantic as it is known to do in the Southern Ocean.

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

These lines are helping to set the scene for the discussion in the paper and so pertinent to the Introduction. We think the focus of the paper would be lost by removing this text.

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

Figure 1 could be improved by adding bathymetry and more labelling. Part b is not confusing – it shows very clearly that there is no strong link between the established measure of iceberg numbers in the region and chlorophyll levels during the peak iceberg season. It clearly shows that icebergs are not the dominant cause of variability in chlorophyll and that we need to look deeper to seek what influence they do have.

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 occurence 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.

The Reviewer is recommending we swap the initial results to become a motivator for the methods used. While we can see this argument, isn't that what Fig. 1b has already done? We prefer to leave the order of the methods as it is.

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

We were trying to say there were issues with both main data sources for our analysis. The respective texts could be moved as suggested.

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 chlâ□□a - see Sirjacobs et al., 2011.

This is a helpful suggestion and can be added in a revision.

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

This can be added, although note that we obtained much more of the North Atlantic surface with the chlorophyll data than icebergs, to be able to compare within iceberg-influence areas from non-iceberg areas.

Line 92-94: Before applying the standardisation did you test the difference between MODISâ□□Aqua 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?

We did look at the difference and found it small but non-zero. We can give values in a revision.

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.

We agree we should show 48N on Figure 1 – it is level with the southern coast of Newfoundland approximately. Icebergs are largely taken through the study area by the Labrador Current along the western shore of the Atlantic basin, so an east-west line is most sensible for this analysis (and has been the standard used by the International Ice Patrol for over 100 years). A map of iceberg density could be shown, as was also suggested by reviewer 1 (please see Figure 1 from the reply to this reviewer).

Line 163: this is a Southern Ocean reference

Wording here can be altered to make it clear we are seeking information from a Southern Ocean source. This is where the vast majority of iceberg analysis has originated.

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.

There have been few in-situ observations so the answer to this question is unknown. However, it is well established that fertilization effects after iceberg passage in the Southern Ocean are visible for up to several weeks. There may be a range of causes: direct input of nutrients/trace nutrients from melting icebergs; enhanced vertical mixing of nutrients from below the surface in meltwater plumes; slow mixing of initial localised concentrations to larger areas; a requirement for time to pass for the conjunction of light, nutrients, currents and chlorophyll growth to occur. It is not the aim or purpose of this paper to explain the time delay, but to present it. See l. 354-359 for a discussion of this point.

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

The suggested approach was already presented in Figure 1b – this is not enough to demonstrate any link that might occur between iceberg numbers and chlorophyll

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.

No control area is perfect. However, both areas have significant coastal regions, areas with significant riverine fluxes and also areas of deep water. Both also cross the Gulf Stream, so contain some polar-source water and sub-tropical source water. The reader also needs to note this approach begins the analysis but does not drive it beyond the starting analysis.

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.

We agree with the reviewer about the diversity of causes of blooms. However, the paper is not about untangling this question in the NW Atlantic but about trying to see whether icebergs are a non-trivial factor that has previously been neglected.

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.

This is just the first step of the analysis, and the approach suggested would distort the direction of the paper's argument.

Line 199-204- remove this text

We do not understand this comment. Surely we need to show the novel approach of partial correlations used here is an established technique in the wider fields of science?

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.

We explain above why there might be a time lag. This is a well established observed fact in the Southern Ocean.

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 exemple 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.

This is a first attempt at addressing a problem that has been mentioned in the literature (Smith et al. 2013), but never approached before. The whole point of Figure 4 is that it suggests a weak, but statistically significant, link between icebergs and chlorophyll. We are not suggesting this is a causal relationship, but merely that the results merit deeper investigation that we then go onto.

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.

We have already addressed these issues in replies above. We are aware of the bio-availability question – iron is not necessarily the answer to what aspect of an iceberg's melting assists production and we stress this throughout.

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.

Please see Bigg et al. (2014) for more discussion of the long-term variability of iceberg flux in the NW Atlantic. During the time during which remote sensing data has been available, however, fluxes are highly variable but do not show any clear trend.

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 umol Fe/m2/d. Please check the units and the conversions carefully.

We can be more careful about units. However, please note the intention of the Discussion is to show that Fe might be important from iceberg sources, not to prove it is. More fieldwork and modelling would be required for the latter. This is beyond the scope of our exploratory analysis.

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.

The reviewer exaggerates what we are claiming here. As we close by saying: "Smith et al. (2013) speculated that this iceberg effect was likely to exist in the North Atlantic and here we have moved towards confirming its presence. The analysis has also shown that it is possible that it is iceberg-delivered Fe that contributes to this enhanced productivity." Neither of these statements are decisive and they do not exclude other causal factors, indeed they suggest these are most important. Our final sentence gives the way forward: "However, the suggested enhancement of productivity by icebergs in the highly productive region of the 408 NW Atlantic means that this effect would be worth quantifying."