Ocean Science Discussions

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Reply on RC1

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-AC1, 2021

Reply to Reviewer 1

We welcome the reviewer's careful analysis of our paper. **However, 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. Our paper is a call for more 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.**

Previous papers investigated the link between icebergs and production have studied the Southern Ocean, where the icebergs are larger and even casual inspection of images of iceberg motion compared to chlorophyll values provides strong suggestion that there is a signal to find. The various papers quoted by the reviewer do indeed carry out very careful analyses but in a region where the circumstantial evidence is stronger, and the database of the location and size data for icebergs is better than available to this paper. The NW Atlantic is a difficult region in which to seek a signal: the main iceberg flux corresponds with the timing of the spring bloom; the concentration in iceberg science there is for protection of shipping; and the region is one with significant climatic and weather variability. This is why we sought to use the novel technique of partial correlation, which is novel in oceanography but used as a standard statistical technique in other fields, as we note in the paper. Because the technique is not novel per se, and the circumstances of the study region are so unique, we did not consider it worth using the approach elsewhere. This could be done, but we do not agree with the Reviewer that this would strengthen our specific analysis.

The reviewer also questions why we used the NAO index as the control variable. We did explore a number of other possible control variables, as mentioned in the paper, however, we found the NAO, as a well used and acknowledged measure of North Atlantic climate variability, gave the strongest correlations. It is not correlated with iceberg numbers (Hanna et al., 2011; Zhao et al., 2016), if indirectly linked to iceberg calving and survival (e.g. Bigg et al., 2014; Zhao et al., 2016; Bigg et al., 2019). This is already referred to in the paper.

The reviewer also questions our control region choice. There are few other options in the region. We needed an area of a similar latitude, close to the coast, so experiencing similar seasonal river fluxes and climate, but not visited by icebergs. Areas north or east of the core region would not be suitable control analogues as they violate one or more of these conditions. We agree that no region would be a perfect control but argue that the region shown in Figure 3 is the best choice, given our core region. We defend our analysis in the paper showing that the mean chl in the control region is significantly correlated with that in the core area.

Addressing main points:

- The reviewer assumes the quality of the iceberg data is much better than it is in reality. We describe the various ways we have tried to reconstruct iceberg data in the paper, but using the basic number data provided by the International Ice Patrol, which includes all icebergs observed out to 35W, restricts options for a large-scale study. A more focused study could look at individual years and individual tracks but the quality and amount of the available data means that this is a task for a next step, rather than the first attempt to examine the question here. The iceberg tracks are sufficiently variable from year to year that we decided not to seek to define a smaller area. If it would help with assessment of the analysis we could provide a map of iceberg density. As an example, the density map for all icebergs observed in the high iceberg year of 2015 is given below. This shows that icebergs are present over much of our core area.

Figure 1: iceberg density map for icebergs seen in our region during the year 2015.

- We are not aware of discussion about "numbers of points" anywhere in the paper. We usually compare mean quantities of areas, whether chlorophyll a or iceberg number, with 1x1 degree fields. We are aware that our mention of clustering of points is not a statistically robust statement as random numbers will cluster, as the review points out. However, the spatial coherence of significance is at times suggestive and it would be remiss not to point this out to the reader.
- We would be happy to remove any discussion about iron in the Discussion. However, the reviewer has not appreciated that this is a "back of the envelope" approach showing that such fertilization by icebergs is not impossible in the NW Atlantic. We do not claim it is occurring. Further fieldwork would be needed to verify this. However, we stand by the spirit of the qualitative discussion given. This is a field that promotes polarization of viewpoints and we do not attempt to join this but merely explore possibilities.
- The Reviewer questions the one month lag suggested between iceberg presence and productivity maxima. This has previously been found in the Antarctic as possible, both for icebergs (Duprat et al., 2016) and for iron-fertilization experiments (e.g. SOFEX). We do not claim to know what drives this, which was a clear signal in the analysis, but present several possibilities linked to ocean circulation. We are not proposing an iron explanation for this phenomenon. More fieldwork is needed to establish the robustness and cause of this phenomenon. Our analysis is only a first step.

Line Comments:

- 13-14: We agree the abstract sentence is misleading. We suggest it is changed to "In addition, a spatial time-lag analysis is consistent with the main cause for the iceberg-chlorophyll relationship being linked to advection of the nutrients entrained in iceberg meltwater." We stand by the 2-4 week delay found in the analysis, which is consistent with Southern Ocean work, as suggested above.
- 15: we kept the cause vague deliberately, so as not to miss any currently unknown mechanisms.
- 33: Of course not all Greenland icebergs enter the Labrador Current. Probably only a few per cent reach the area of interest (see Marsh et al., 2019). This comment clearly

suggests we should add a map of iceberg density in a final paper version (e.g. like Figure 1 above).

- 40/41 & 42: We are happy to omit the inference from higher trophic levels and alter the reference in l. 42.
- 44-45: Much of the region discussed is coastal or on the Grand Banks and so relatively shallow. We can certainly change the statement to make it clear it is larger icebergs that are referred to in terms of causing vertical mixing.
- 47/51: There isn't much information available on nutrient levels in icebergs but sufficient evidence just from open ocean pictures that some exists (see figures in chapter 2 of Bigg, 2016). We are not questioning that most icebergs don't escape fjords, but some clearly do (see l. 33 comment above). We are focusing on those that reach the Newfoundland area.
- 52: The reviewer misreads our statement. We merely say the iceberg peak corresponds to the timing of the spring bloom. Nowhere do we say the latter is caused by icebergs. It is this co-incidence of timing that makes our analysis much more difficult.
- 164: We suggest modifying the line to "Remote sensing analysis in the Southern Ocean also suggests that the impact of fertilization from melting icebergs can have a time lag of some days (Schwarz and Schodlock, 2009) to weeks (Duprat et al., 2016)."
- 176: we are referring to meltwater from nearby icebergs clearly. We suggest changing this sentence to "A nearby coastal area, which will experience many of the same environmental forcings as the main area, but which almost no icebergs reach (see Fig. 13 of Wilton et al., 2015), was also defined as a control area."

Figure 3: the areas are different in size. However, analysis is in terms of comparisons between means. There is no discussion where point numbers play a role in the analysis. Nevertheless, if desired, we could alter the box sizes to make them comparable. Some of the analysis already only goes to 52N, removing 80 grid boxes and making the areas roughly the same.

- 187-188: causation is not intended. We can change the wording to remove this connotation: "The next step is correlating the monthly mean chlorophyll between the control and iceberg areas for just the months during the peak iceberg season of April-June."
- 191-192: The argument in this whole paragraph is to suggest that the control and main areas do experience different behaviours when icebergs are present in one. It is not definitive, merely suggestive. We could remove the whole paragraph but feel the reader loses some of the rationale behind our thought processes by doing so.

Figure 4: we agree this is not particularly convincing. It is why the analysis then takes on its novel approach, to try to strengthen the hints Figure 4 provided. The reviewer is being too critical of what is clearly the first step of the analysis. Note that the 2 month control area correlation is not statistically significant and we do address this aspect in the text (l. 264-5).

Figure 5: we agree that this is not strong. This fact is why we then go on to the lag study, as is stated in l. 291. This is a difficult problem and unlike some scientific approaches we here show our steps to arrive at the best answer we could with the data available. Science is not perfect and the process of argument reasoning is often not shown. (l. 273 and 288 are also part of the discussion that ends needing to move to a deeper analysis so we will not directly reply to these points)

Figure 6: we feel the Reviewer had decided against the paper's theme by this point and was not prepared to examine the evidence in Figure 6 – and most importantly the video – in a positive way. We believe Fig. 6 and the video clearly show an increase and movement of regions of positive correlation. We do not claim to know what causes this but the Discussion attempts to address this question. The answer awaits further work.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2021-61/os-2021-61-AC1-supplement.pdf