Ocean Science Discussions

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Review os-2021-61

Anonymous Referee #1

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-RC1, 2021

Bigg et al., present an analysis of iceberg distribution and chlorophyll analysis in the North Atlantic, investigating the links between iceberg distribution and primary production. The authors use satellite derived chlorophyll a, a historical record of the number of icebergs transported past 48 degrees south 'I48N', and a database of iceberg locations on a 1 degree grid resolution. By contrasting the dynamics of chla and iceberg density in a region of interest, and a control region, the authors investigate the role of N Atlantic icebergs in ocean fertilization. The statistical method used to do this, partial correlations, is novel in this context and is argued to show a weak but statistically significant positive effect of icebergs on chlorophyll a with a month time lag between iceberg presence and chlorophyll change. The conclusion of this paper rests heavily on the application of this technique and whether it not it is capable of distinguishing chlorophyll changes driven by iceberg passage from other 'background' changes.

I am a chemical oceanographer and thus whilst I'm familiar with the literature on icebergs and the use of satellite derived a, some of the statistical techniques herein are unfamiliar. With editorial consent, I did consult with a colleague concerning these aspects of the manuscript in an oceanographic context.

There are existing iceberg-chlorophyll tracking papers which explicitly show in detail (and test) appropriate statistical methods for assessing whether or not iceberg passage is associated with changes in chlorophyll, (Schwarz and Schodlok, 2009; Wu and Hou, 2017). Both of these works cited contain careful critical assessments of the methods used that robustly test whether or not the presence of an iceberg itself, rather than some coincident factor, is responsible for any associated change in chlorophyll, and whether or not any associated change in chlorophyll is different from background changes in the same region with the same timing. This work is not quite so careful, two obvious points could be raised with the method as described. First, given the technique is completely novel, why not test it in an area where icebergs are thought to have a strong influence based on existing work to see if the technique functions similarly? A different control factor would be required, but if the technique works, it should be possible to apply it to a box in a region where other studies have identified strong relationships between iceberg

abundances and chla. The results of such a test would then justify the use of the technique to look for an iceberg influence somewhere more challenging where it would be more difficult to apply well tested techniques. Secondly, concerning the control parameter -the NAO index- used to account for variability due to other causes. It is not really clear to me why the NAO index used as a control parameter? Does this fully account for differences in wind driven mixing, is it completely independent of iceberg density or iceberg arrival within the region of interest? Have other control parameters been tested? Seasonal cycles can, and often do, cause artificially inflated correlations in datasets, so it's very hard to account for this when making correlations between two seasonally variable parameters. To make a convincing case, as has been done in prior work, I think more testing of this is required, the authors could for example select some completely unrelated, but strongly seasonal, monthly time series around the N Atlantic/Greenland and attempt to do the same analysis. The authors' case would be more convincing if they could robustly show that the weak correlation the paper hinges on is not spurious. The same comment can be made concerning a comparison between one region of interest and one control region- a comparison to multiple control regions would be more convincing, and I have some concerns about the choice of control region used.

There are also other specific problems with the statistics which I think preclude any conclusions supporting the authors' hypothesis, particularly concerning a month 'lag' effect between iceberg arrival and fertilization. Such an effect is very difficult to explain using the known mechanisms via which icebergs could affect fertilization in this region and its appearance may relate to factors such as a difference in latitude and spring bloom commencement contrasting the control and ROI boxes rather than anything to do with icebergs in the ROI. More obviously, unless I'm mistaken, the control and the ROI boxes appear to be different sizes, thus statements comparing the two in terms of the number of points within each following application of any technique are not mathematically valid. What is the specific rationale for the size and location of the control box? As above, given the novelty of the technique it would have been better to pick multiple control regions, and to specifically find a control region with bloom dynamics the same as the ROI. The authors comment that the mean monthly chla in the control region is not distinguishable from the ROI, but I am not sure exactly what this means as chla data clearly shows that the spring bloom commences and peaks much earlier in the control region than it does in the ROI.

Main points

- I found the manuscript very hard to read, whilst some of the statistics are described in detail, other points (which could be tested statistically) are made as unsupported qualitative statements. For example, it's not actually clear from reading the text where icebergs are within the ROI, or what the main area receiving iceberg tracks is. The authors refer to this 'iceberg alley' several times, so an obvious question is why not define this region and then we can see if there are/aren't strong correlations specifically for this area of high iceberg tracks? Is the main 'iceberg alley' a subregion with the region of interest? Does this correspond to the patch of significant correlation highlighted within the ROI in several figures?
- In several places the statistical analysis and interpretation is flawed. As an example, there are comments related to the number of points within the control and region of interest (ROI), but the ROI is bigger than the control region, so the two cannot be

compared directly in terms of number of points. Clustering of points (patchiness) is also described as if it supports the hypothesis, but patchiness is inherent in any random distribution, so the presence or absence of a "coherent area of statistically significant partial correlation" is meaningless in terms of testing the hypothesis unless this cluster corresponds to something quantitatively.

- There is presently a lot of qualitative discussion concerning biogeochemistry, but whilst the authors correctly identify the few known mechanisms that could explain a link between icebergs and primary production (upwelling/mixing or direct nutrient fertilization), they don't provide any quantitative analysis of existing data to show which is likely to be effective in the North Atlantic. There are a couple of cruises that have iron/macronutrient data for the region and I'm not sure these support the authors' hypothesis that addition of iceberg-derived-Fe could be increasing primary production. The Fe calculations that are provided seem out of place without commenting on what Fe and nutrient concentrations are in the region of interest and to what extent there could be Fe or nutrient-limitation in the region at the time of year concerned. They also seem circular, the authors summarize that Fe input into the region of interest could be massive compared to dust input (which is generally thought to be the main input by oceanographers), but if this were the case, and the authors' hypothesis that any iceberg derived nutrients are being mixed in surface waters for a month before driving any increase in chla were correct, surely Fe concentrations across the region would be high and thus small changes in iceberg Fe delivery would not be needed to change primary production (there would be an excess of Fe relative to other nutrients)?
- The timing of the 'lag' investigated herein also seems dubious as this is not really consistent with the Fe fertilization discussed. The (Arrigo et al., 2017) reference cited for example shows a rapid effect of meltwater arrival on chla in a specific box off SW Greenland which is interpreted as possibly being Fe fertilization– but this statistic concerns bloom timing not total chla, so the mechanism doesn't really compare to what the authors discuss herein. Fe cannot be transported long distances at the ocean surface, it is rapidly scavenged and drawn-down by primary producers even in areas where it isn't considered a limiting nutrient (e.g. see discussion by (Birchill et al., 2017)) so the idea of an Fe plume from icebergs remaining in the ocean during the growth season, having no effect on primary production until one month after it was deposited is implausible. Fe addition experiments usually show a positive fertilization effect after 1-5 days (e.g. see Browning et al., 2019, 2021 in the ROI) consistent with the timing effect suggested off SW Greenland, and similarly where strong Fe fertilization is observed in the Southern Ocean elevated dFe concentrations are only noticeably observed within a few km of icebergs (Lin et al., 2011; Lin and Twining, 2012) with chla changes detected within days (see main comment for papers doing this well).

Line comments:

13-14 This is not really consistent with any known mechanism of iceberg fertilization. If a nutrient required for primary production in the N Atlantic was upwelled or released as the iceberg melted, it would be drawdown much faster than this during the growth season. Fe in particular does not have a long residence time in the ocean surface, especially during the growth season, so it's not clear to me what this is 'consistent' with. It's quite bizarre and unexpected.

15 By range, I assume the authors mean either upwelling or release from melting ice? So

why not just say these two.

33 Do any icebergs from Greenland not enter the North Atlantic? – it would be very useful on some figure to show iceberg tracks, or areas of high iceberg observations as at present it is not clear at all where the icebergs are or where they generally go. Thus it's very difficult to comment on the validity of the chosen boxes used in all statistics.

40-41 This is not a correct inference. High secondary production, or high abundances of seals/birds/fish etc does not necessarily imply local high primary production. E.g. it's well documented seals rest on icebergs after sea ice loss, and there is generally a hotspot of feeding activity at glacier termini in the Arctic (Lydersen et al., 2014), but is not because local primary production is high - quite the opposite in this case, these zones have low primary production but the water column disturbances kill /stun prey making convenient hunting grounds. So 'enriched ecosystem locally' does not imply iceberg fertilization.

42 The reference cited does not show micronutrient limitation of the Labrador Sea, they suggest Fe limitation could occur in one specific region off SW Greenland, but only at a window between the spring and summer blooms, not across the Labrador Sea during summer as cited. Canadian GEOTRACES data would be better cited here as this shows low-nitrate/low-Fe conditions in summer in the Labrador Sea.

44-45 What iceberg depth / mixed layer depth is being referred to here? Based on the comments on iceberg dimensions herein, I would not expect more than a minority of large icebergs to penetrate the mixed layer depth in the Labrador Sea, only in well stratified coastal regions where the MLD is shallower.

47 Is there data on the nutrient content of icebergs? I would think this was generally very low for more macronutrients but I'm not sure you refer to any data to suggest the contrary. I'm not sure 'bypasses removal in fjords' is quote correct. Icebergs are subject to intensive melting in fjords around Greenland, loosing a majority of their volume and sediment, e.g. (Azetsu-Scott and Syvitski, 1999)

51 Raiswell et al,. don't explicitly show this, I think it is worth asking how much of the iceberg derived sediment is actually exposed to UV light and for how long, a large fraction likely melts and gets deposited in fjords without ever being exposed to sunlight. e.g. above ref shows most of this sediment never enters the ocean surface (Azetsu-Scott and Syvitski, 1999)

52 What mechanism would icebergs promote spring productivity via? There's no suggestion in the literature, or in this manuscript, of a mechanism via which icebergs could do this in the North Atlantic.

164 I don't think (Schwarz and Schodlok, 2009) show this, they only test 6 day periods before and after iceberg transit.

176 "little iceberg meltwater" It is not clear to me what this means -melt from icebergs from the point they enter the ocean? Most freshwater from Greenland (which includes most iceberg melt as most of this occurs near-shore largely overlapping with where runoff enters the ocean) is advected counter-clockwise around the Labrador Sea and then follows the Labrador shelf precisely where the authors state there is no impact (?) e.g. see manuscripts modelling this (Luo et al., 2016)

Figure 3. Why are the ocean areas of the control and main regions different? The region of interest looks to be considerably larger than the control region? Several of the comparisons are invalid as there are fewer datapoints in the control region so it is not valid to comment on 'less/more' of anything relative to the control region.

187-188 This does not prove causation. What factors lead to high iceberg transfer into this region? These factors likely include wind/current forcing which also affects plankton dynamics. If icebergs did have an impact at this time of year, via what mechanism is this plausible? It is not clear to me what factors the NAO does/doesn't account for.

191-192 This is not robust if the only test is whether or not correlation with chl is higher than one control region based on the fact that the control region has similar environmental forcing (and see my earlier comment concerning the timing of the spring bloom between the two regions). This should really be done compared to multiple iceberg free regions and ideally in regions with similar bloom dynamics rather than significant temporal offsets in the initiation of the bloom. If it is the case that only the main region of interest has a strong iceberg signal, then this basic statistic (that the correlation between chla in this region, and a control region with similar forcing is stronger in low iceberg years and weaker in high iceberg years) would hold generally with a comparison across all of the adjacent cells. The sensitivity of the statistic to the threshold of 100 should also be shown. Looking at Figure 1 for example, it looks like the best possible fit between spring mean chla and spring I48N would be obtained for the ranges I48N >900, or I48N >200 i.e. a correlation may be very sensitive to what threshold is used just because the dataset is small.

Figure 4 This is not particularly convincing without showing sensitivity analysis. For example, why is there also a positive 2 month threshold for the control region? If I understood correctly, what is shown here is the correlation between a list of numbers (I48N) that show a peak building from march, peaking in May, and then shouldering in July, with chla dynamics. The timing of the spring bloom varies by region/latitude and thus in a correctly selected region, the timing of the spring bloom peak and spring I48N peak will coincide creating a better correlation than is possible in a region where the events don't coincide temporally. Looking at regional Chla, the spring bloom starts earlier in the control region peaking in April (i.e. too early to match the I48N) peak, whereas the bloom starts later in the region of interest and is more intense. So there is a better temporal overlap between the timing of I48N and the spring bloom in the region of

interest. If the control region were instead at the same latitude with the same seasonal timing of the spring bloom, it may no longer be the case that there is a difference between the Person correlations shown. As above, multiple control regions around the ROI would be more convincing.

Figure 5 again shows a correlation between a list of numbers, I_T, and local chlorophyll anomalies within small grid cells in the control region and region of interest. In looking at whether there are positive or negative correlations here, the number of cells must be considered. Again, if icebergs had no effect, I_T could be considered a random list of numbers, in which case the correlation would be random i.e. equal roughly numbers of positive and negative correlation scattered across the regions. With a p value of 0.05 how many boxes within the regions would show a correlation in such a random scenario? Is this more or less than actually show a correlation? For the amount of grid cells and a p value of 0.05, the displayed data does not appear to show any convincing relationship. It's hard to comment without knowing the number of grid cells, but from what I can see the balance between positive and negative values and the number of cells picked out by a p value of 0.05 seem to show not much difference from a random distribution.

273 Here and elsewhere the authors comment on the patches of statistically significant correlations, but patchiness would be observed in any random distribution. Do these patches actually correspond to anything meaningful, i.e. can you plot the intensity of iceberg distributions onto cells? If not, this discussion is relatively meaningless concerning Figure 5.

288 But (a) the control region is smaller and (b) randomly dispersed patched would be present in a random distribution. Patchiness is inherent in a random distribution, so patchiness doesn't make any argument more convincing unless it can be shown quantitatively that the patches mean something i.e. do they correspond to areas with the highest iceberg intensity? As noted above, more correlation between chla and iceberg presence could simply reflect latitude as in the area of interest the timing of the spring bloom is better matched to the timing of iceberg arrival. It would be useful to have control regions distributed around the area of interest, not just one SW of it with an earlier bloom dynamic.

Figure 6. Similar comment to the above statistics. I'm not sure what this really shows, there's a positive lag effect across the N Atlantic including in the ROI, but also in areas that don't receive any iceberg influence outside the ROI? The region of interest doesn't appear to show an iceberg 'hotspot' that can be delineated from any other effect, (unless as above the authors can specifically highlight the area of highest iceberg intensity) so I'm not sure this adds any evidence to support the authors hypothesis.

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