Dear Editor and Reviewers,

We would like to thank both reviewers who have provided a detailed review of our manuscript and provided valuable comments and suggestions, which have helped us improving our manuscript. We have done our best to account for all the comments and have responded individually to each of them below (our responses are written in blue for clarity). The major changes of the manuscript include:

- A reorganization and simplification of the ms (e.g., moved some text from the results and discussion to the method section, removed redundant sentences)
- We have toned down the statement about the N-fertilisation contribution to the chlorophyll-a standing stocks in relation to climate change
- We have updated the Seal population model from Hammil et al. 2017 to Rossi et al. 2021 as the new model was published while our manuscript was under review. Note that new model has marginally changed our results and claims. The main change is that we did not need to extrapolate seal abundance of Age1+ for 2017 and 2018

We hope that our response and modification to the manuscript will satisfy the reviewers and editor so that our manuscript will be recommended for publication.

Best Regards,

Emmanuel

REVIEWER #2

General appraisal

The paper by Devred and co-workers presents an attempt to quantify the role that an increasing marine mammal population (grey seals in this case) can have on water quality parameters such as the chlorophyll concentration, through the release of nitrogen in coastal waters. The study area is Sable Island (SI) on the Scotian Shelf in eastern Canada.

For this purpose, they use satellite ocean colour-derived chlorophyll and inherent optical properties, namely the particulate backscattering coefficient at 443 nm, $b_{bp}(443)$ and the coloured dissolved organic matter plus detrital matter absorption coefficient at the same wavelength, $a_{dg}(443)$).

The topic is relevant for publication in BGS.

Overall this is a rather interesting paper. As the authors say (lines 23-24), the absence of in-situ data to confirm the results, which are entirely based on satellite remote sensing products, is a limitation but, certainly not a significant flaw that would prevent publication of their work.

The study site and methods are rather well described, although the rationale for using self-organising maps (SOM) and their presentation are not that clear.

Thank you for your comments, we have made substantial improvements to the manuscript following the reviewers comments/suggestions with the goal of improving the fluidity and simplifying the manuscript

I did not notice much that would require clarification here, except maybe to know whether seal-generated nutrients are rather produced on land and then have to be washed out to the sea to have an impact there or a significant part of it is directly generated at sea. Looks a bit trivial but might have a significant impact on whether this nitrogen added in the ecosystem can indeed have an impact on phytoplankton growth (what's written in section 2.5.2 seems to assume all is produced on land).

It is very possible that some of seal excretion occurs at sea, however, most of it is assumed to happen on the island and is washed out at sea, this is the reason why we looked at meteorological data (precipitation and temperature) and believe that a lag between the peak of seal N fertilization and phytoplankton response exists.

The paper is however quite lengthy and could be shortened significantly. The message that the authors want to convey is somewhat lost. A reorganisation is needed as well, with bits of method-like descriptions to be removed from the results/discussion section. Overall, the methodology has to be clarified. This paper is hard to read in particular because the Figures do not make a very good job in conveying the important results (quite often hard to locate/identify on Figures what we read in the results/discussion in particular).

We have made an extensive effort to simplify and reorganize the manuscript as suggested by both reviewer. We thank the reviewer for this constructive comment that will make the manuscript easier to read.

I also found that analysing the results based on both 12 arbitrarily located boxes and 9 SOM-derived regions is somewhat confusing. At least I could not find a clear justification for doing so. Looks like using only the latter might make more sense.

We thank the reviewer for the comments. However, we took an objective approach that could demonstrate that the IME exists and we believe that by determining first an area or a threshold for chlorophyll-a concentration might bias the study. We found that delineating small boxes around sable island without a priori knowledge of the phenomenon would be the best, most sound approach. We updated the approach following reviewer's #1 comment to make easier to follow and we have emphasized the main message.

Detailed comments

- Figure 1: I did not understand what the boxes labelled B1 to B12 were until I reached section 3.1, but then I wonder why the seasonal cycles in Fig. 2 are displayed for these arbitrarily defined 12 "square" boxes, instead of being displayed for the 9 regions that the SOM has identified?

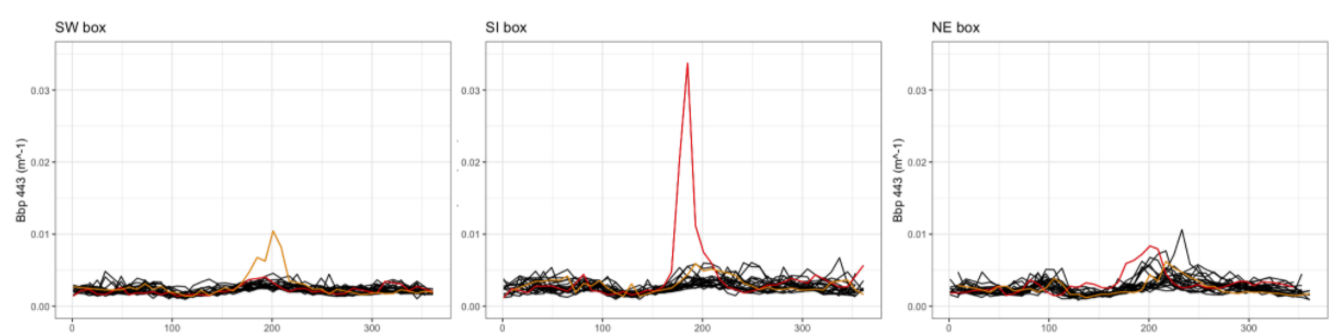
The SOM analysis provide the spatio temporal distribution of chlorophyll-a on the Scotian Shelf while the small boxes analysis focuses on the seasonal cycle of chl-a, adg and bbp in the vicinity of Sable Island to demonstrate the IME. These are two different approaches that do not have the same aim.

- Figure 3: not sure I understand this one, in particular: "The centroid of each month in SOM space is shown as the dark grey path, starting with January (1) near the centre, proceeding counter-clockwise to December (12)". And what is the chlorophyll concentration displayed in each of the 9 panels in (a)? Annual average? Why not rather display a map showing the spatial distribution of the 9 phenotypes (I guess one can call the 9 seasonal cycles shown on panel (b) "phenotypes"?). That would clearly show where the different seasonal patterns occur. But are panels in (b) actually showing seasonal cycles? Sorry but I realise I am actually confused by this SOM analysis. And, still on this figure: if the spatial patterns are important, then the maps should be much bigger.

  Following the reviewer #1 suggestions, we have added explanations to the Figure 3 legend. The SOM analysis is similar to a cluster analysis where images with similar features (i.e., spatial distribution and magnitude of chlorophyll-a concentration) are grouped together, the final result is 9 maps (i.e., clusters of similar images) that show the main distribution of the chl-a in the area of interest. The right panel (-b, histogram) shows the frequency of occurrence of each pattern. In this manner we were able to show that the pattern number 5, which show a plume of chl-a leeward of Sable Island occurs mainly in Winter, which corresponds to the seal breeding season. We have added text to clarify this.

- Section 3.1, lines 282 to 290: you cannot explain an increase in $b_{bp}(443)$ by a factor of 2 to 3 when chlorophyll does not change at all by a change in the phytoplankton size only. The only reasons I can see here to explain the huge increase in $b_{bp}(443)$ from week 17 to 25 in all 12 boxes (when Chl is steadily around 0.5 mg m^{-3} in all boxes) would be coccolithophorids or mineral particles (sediments). Therefore, it seems that the elimination of these events is not actually completely performed by what you describe in section 2.2.

  Thank you for the comment, we agree that phytoplankton community structure alone cannot explain the large increase in $b_{bp}(443)$. We have removed large coccolithophore blooms that have been documented in the literature (see response to reviewer #1) but we cannot remove smaller coccolithophore blooms or resuspension events that do not have a strong signature.



The figure above shows the mean time series of $b_{bp}(443)$ for the 3 large boxes, where all years are plotted on top of one another (the x-axis is the Julian day of the year, and y-axis is $b_{bp}(443)$). 2 large coccolithophore blooms are highlighted: the orange solid line corresponds to 2003 and the red line corresponds to 2010, seen to occurr in the SW box and the SI box. These have been removed from the analysis. You can see that the NE box has other events of note, but we removed the images occurring during the 2003 and 2010 events that would have significantly impacted the statistics for the SW and SI boxes.

- Beginning of section 3.2. This is stuff for the method section, not for discussing results. Considering my comment above, I cannot really comment on Section 3.2

  We have reorganized the manuscript and the text at the beginning of section 3.2 has been moved to the method section.

- Line 289: I think an increase from 100,000 to 300,000 is a 200% increase, not 300%. (100 x (300,000 – 100,000)/100,000).

  We have correct the percentage from 300 to 200%

- Lines 385-400: Does not a lot of this already appear in the method section?

  Yes, a lot of the material in this paragraph has already appeared in the method section. This was a reminder, however, we have removed the text.

- Legend of Fig. 7: what do you mean by “images”?

  We have replaced the term “images” by “mean.

- Lines 420-421: well, instead of telling us that the “mathematical formulation of the model was rather arbitrary” you could tell us what the model is. Would definitely be more useful.

  We have modified the text to explain the model, thank you for the comment

- Not sure why Figs. 8 and 9 are not included in the main text, like the others.

  We used the Biogeosciences latex format. The position of the figures will be determined for the final version before publication. We will ensure that the figures are appropriately located.

- Frankly, all page 19 is really hard to follow. There is too much in there, without a clear message on what you want to tell us. That section is where you definitively lost me.

  Reviewer #1 has made a very similar comment, we have modified and shortened page 19.

- Line 466: you may have shown a correlation but, claiming that you have identified a “causal link” is probably a bit of a stretch here. You cannot say this.

  We have toned down our statement.

- Figure 9 is another one that is not that easy to understand. The values on the right scale are about ten times lower than those we read on the left scale, so that I think the right scale should read “Change in the Chl-a standing stock”, right?

Thank you for the reviewer to point this out, we have updated the legend of the right scale.

- Line 479 (related to Fig 9): I do not see these numbers on Fig. 9. The final value in P4 is close to 3,000 actually.

The values P1N, P2N, P3N and P4N are not shown on Fig. 9. We have added these to Fig. 9.

- Line 526: does anyone really said that the island effect is only supposed to occur for oligotrophic waters? References would be good to have here.
- We did not find in the literature a study about IME that occurred on a continental shelf, most of the studies focused on island located in oceanic basin surrounded by oligotrophic waters, however, we decided to remove the sentence to avoid confusion.
- Appendices A and B are not called in the main text. Appendix A could actually be incorporated in the method section, and appendix B does not seem that useful.

  We have left the SI in its current form as we believe that it contains interesting information that can be consulted at the discretion of the reader.