

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2021-55

Anonymous Referee #2

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Referee comment on "Enhanced chlorophyll-*a* concentration in the wake of Sable Island, eastern Canada, revealed by two decades of satellite observations: a response to grey seal population dynamics?" by Emmanuel Devred et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-55-RC2>, 2021

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### 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.

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).

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).

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.

### 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?
- 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.
- 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  $0.5 \text{ mg m}^{-3}$  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.
- 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
- Line 289: I think an increase from 100,000 to 300,000 is a 200% increase, not 300%. ( $100 \times (300,000 - 100,000)/100,000$ ).
- Lines 385-400: Does not a lot of this already appear in the method section?
- Legend of Fig. 7: what do you mean by "images"?
- 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.
- Not sure why Figs. 8 and 9 are not included in the main text, like the others.
- 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.
- 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.
- 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?
- 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.
- 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.
- 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.