

Interactive comment on “Experimental tests of phytoplankton response to ornithological eutrophication in Arctic freshwaters” by Heather L. Mariash et al.

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1. This study provides interesting new knowledge on the important role of goose droppings in affecting water quality in Arctic freshwaters. It shows that these droppings have a greater short-term effect on water quality than a sedge plant. These results are not unsurprising. The paper is well written and clear. The experimental design is simple and straightforward. The parameters measured are basic water quality parameters, although chlorophyll-a concentrations were not measured.

Thank you for the positive feedback on several aspects of the manuscript. We underestimated the amount of organic matter in the goose treatments when designing the

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experiment, as such several aspects such as chlorophyll-a and primary productivity failed to be realized.

2. The findings from using small containers should not be over-interpreted. These small containers have a high surface area to volume ratio which can become important in terms of biofilm growth on walls. So the effect in the first few days is the most ecological relevant. This limitation should be discussed. We have included an additional sentences to P.9 to acknowledge the potential container effects: “Our experimental design was not without issues: more replicates would have made the results clearer, and the use of small containers has the potential to contribute technique-related artefacts (e.g., biofilm growth, altered physiochemical conditions, and species interactions due to container area-to-volume relationship; Liber et al., 2007), effects that we attempted to mitigate through the use of a short experimental duration. Despite these caveats, the responses in the phytoplankton communities were pronounced.”

3. Nitrogen versus phosphorus limitation is only relevant if concentrations are low. Therefore N:P ratios should be with caution. Additionally, N fixing cyanobacteria are only promoted if N concentrations are low, not just because N:P ratios are low.

On P. 10 of the Discussion we have a sentence that highlights when low nitrogen concentrations promote cyanobacteria, not just low N:P ratios. “This is an environmental concern from a water quality perspective, because when N is limiting, N₂-fixing cyanobacteria are competitively favoured (Guildford and Hecky, 2000; Schindler et al., 2008)” To decrease the emphasizes on the N:P ratios in this same paragraph, we have removed this sentence (“In the wintering grounds with highest goose densities, TN:TP ratios of the waterbodies had a mean of 15 (Kitchell et al., 1999), indicating N- limitation. While the” Lastly we adjusted the last sentence to include that we have both low N and decreasing N:P ratios. “The wetlands across Southampton Island have relatively low nitrogen concentration and TN:TP ratios of approximately 30 (Mariash et al. 2018), on par with other shallow Arctic freshwaters (Rautio et al., 2011), there is an indication that these wetlands are becoming more N-limited with decreasing TN:TP

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ratios (Mariash et al., 2018).”

4. The study did not measure primary production, that is a rate process. So the paper should be explicit that what was measured was accumulation of biovolume. We are now more explicit throughout the manuscript using biovolume and not the rate of primary productivity. Also Reviewer 2 made several suggestions in this regard, please refer to those comments for specific amendments.

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