Comment on bg-2021-251

Emma Cavan (Referee)

Le Mezo et al present a well written and informative article on an important emerging topic, the influence of fishing activities on ocean processes, here specifically nutrient availability and turnover. The study builds on the recent Bianchi et al paper using the same BOATS model but with a focus on nutrient cycling. I think this paper should be published after some revisions to the text outlined below.

The large spatial nature of the study means that the authors need to take average nutrient concentrations for fish, which they explain well. They then calculate fish nutrient concentration in an unexploited (pristine) scenario, which is basically a product of fish biomass. I fully support the methods used, but would suggest the authors:
1. Explain Fig. 3a in the text clearer. It is not intuitive to have three nutrients represent the same spatial pattern as in Fig. 3a, so explaining again in the results that fish nutrients are held constant and that Fig. 3a is fish biomass distribution would help the reader quickly understand the figure. Especially as in the Introduction the authors recognise that fish nutrients do vary.
2. Either discuss more on the issue of constant nutrients or remove the lines in the abstract (8-13) around Fig. 3b-d, that cycling of certain nutrients in certain regions is important. For instance if P is limiting in the North Atlantic, does that mean that P is reduced in fish biomass? How does the lack of nutrients for phytoplankton propagate up the food chain? Will the fish actually release P in the North Atlantic, or at least enough to have an impact, if they themselves might be P limited? These questions are relevant to the other nutrient limiting regions too. I think if these statements are going to be made upfront in the abstract they should be better supported by discussion on the limitations of the methods and citing other literature, and also including what the main proportion of fish biomass is made up of (i.e. which species) in each region and what the nutrient concentrations might be in those important species groups.
Focussing the abstract on the main conclusions of the study - total global nutrient cycling with and without fishing - is more inline with what the results can show. I am not convinced showing the spatial patterns is accurate if only biomass changes spatially.

In addition, I think it is important to highlight where the limitations are (again spatially) in the BOATS model and algorithms/data used. For instance, the Southern Ocean often does not perform equally well compared to other areas in global algorithms. The biomass will be quite high for the size range of organisms used in some Southern Ocean regions, but is consistently a region of low fish biomass and therefore low fish nutrients according to this study. This should be flagged and acknowledged, or the poles removed from the analysis.

A final point is to discuss what the limitations are of keeping the lower size spectrum (< 10g) constant. There would be feedbacks to the ecosystem of harvesting up the food chain. These include reducing nutrient release which may reduce primary production but also reducing grazing pressures that could influence (increase or decrease) food supply to the resolved size-spectrum. I am not suggesting the authors do this analysis, but I think is a valid point to make given this paper aims to increase our knowledge of all fishery impacts to marine life.

Text sometimes has stand alone sentences not in paragraph.