Response to Reviewer 3:

The authors present a numerical study of Arctic planktonic ecosystem productivity and trophic organization in response to major physical drivers, such as sea ice extent, light availability, temperature, several nutrient concentrations, stratification, advection, etc. Their study allows for a detailed exploration of a biogeochemical model of intermediate complexity that considers functional biodiversity and size-based dynamics as well, for a ~60-year time span at the scale of the whole Arctic domain. It leads to a rich harvest of numerical results that are well presented and analysed: despite the ambitious scope of the study, the paper does not feel too complex nor too long. I command the authors for having chosen an interesting balance between a more detailed inter-regional and seasonal approach, and approaches deployed at the larger interannual scale and trophic network level.

With all its qualities, I still think that there are some elements that should be addressed, especially regarding the comparison with observations (validation is too strong a word in the Arctic context).

First, regarding the comparison of simulated and observed surface chlorophyll a level (especially L270-274), I do not agree with the authors that "The comparison between model and satellite chlorophyll shows that in many cases the phenology of chlorophyll, if not absolute magnitudes, corresponded reasonably well". In *most* instances, the comparison is not convincing… the only instance where the *phenology* corresponds are the Nordic Sea (Fig. 3d), while the few instances where the *concentrations* match are again the Nordic Sea, Baffin Bay (Fig. 3a) and Central Arctic (Fig. 3e) for one month, while Chukchi and Barents Seas are relatively close for June-July, but with and obvious issue for the latter (increasing observed values in winter and fall…). I also stress that in all the chlorophyll a figures a log scale has been chosen, which obviously tends to downplay the discrepancies.

It does not mean at all that the modelling study is not valid, since the model behaves logically and that satellite-based observations of Arctic chlorophyll a are notoriously challenging to handle. I think though that the authors should not downplay this difficult comparison; I'd rather prefer them to recognize it and try to refer to other studies that might have run into the same issue (e.g. Popova et al. 2012. 10.1029/2011JC007112; my reference here is old, the authors might know more recent ones). Moreover, I think the authors could provide an estimation, at least in the discussion, of how the use of a different color algorithm that takes into account the high CDOM content of these waters could change the satellite-based biomass estimates (e.g. Li et al. 2023 10.1364/OE.500340).

- Thank you for your comment, which is similar to Reviewer 1 and 2's concerns about the comparison with satellite chlorophyll estimates. In the revised paper, we will use chlorophyll estimated with an algorithm tailored to the Arctic Ocean (Lewis and Arrigo, 2020; https://doi.org/10.1029/2019JC015706). We updated Figures 2 and 3 and updated a

detailed discussion of model-satellite chlorophyll comparison with this satellite product, as detailed in our response to Reviewer 1. Note, that due to different satellite grids, we have modified the seasonal comparison following regional masks by Lewis & Arrigo 2020 shown in Figure 1.
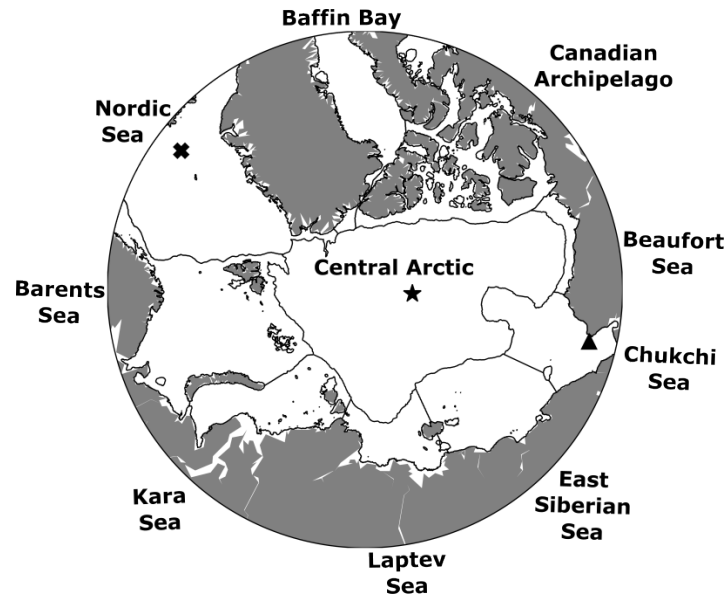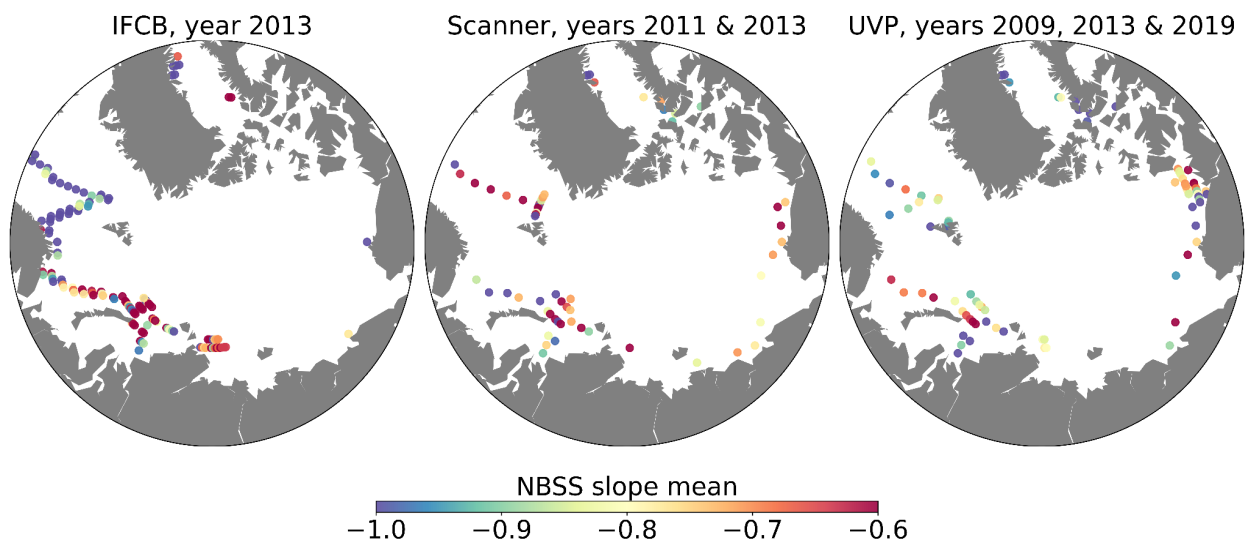

Figure 1. Map of the Arctic Ocean divided into ten sections included in the analysis, following Lewis & Arrigo (2020) regional mask. Additionally, symbols represent three grid cells selected throughout the Arctic Ocean for growth limitation analysis. The cross symbol shows the Western Nordic Seas (68.5^oN, 348^oE), the star symbol shows the central Arctic Ocean location (85.5^oN, 200^oE), and the triangle symbol shows the Chukchi Sea location (68.5^oN, 168^oW).

Second, starting at section 3.3 approx., it seems to me that about half of the results shown are not / cannot be directly compared to observations, from the slopes of the size-structured community to the trophic position of the zooplankton groups. It would strengthen the paper if the authors could find information on the slope & intercept of the size structure of plankton community, from Tara Ocean, Mozaik expedition or other sources like this, if possible. What is certainly possible, though, is for the authors to present some ideas of experiments that should be done to test the many hypotheses their model has generated! I think it would be very useful for the community of Arctic oceanographers as a whole, since there are so many outcomes of their model that could provide guidance for future in situ experiments.

- Thank you for your comment. We agree that comparing the size spectra relationships with observations would be desirable. There is a new database on plankton size structure with global coverage, prepared by Dugenne et al. (2024) (https://essd.copernicus.org/articles/16/2971/2024/) . The Arctic data in the Pelagic Size Structure database (PSSdb) is from the Tara Polar Circle expedition. However, the

availability of data for the Arctic Ocean is limited, with substantial data only available for 2009, 2011, 2013 and 2019. Below we show the slope of the normalized biomass size spectra (NBSS) from three different instruments, the Imaging FlowCytoBot (IFCB; primarily phytoplankton and surface only data), Scanners (e.g., ZooScan, primarily zooplankton size ranges and 0-200m depth-integrated), and Underwater Vision Profiler (UVP; primarily zooplankton size ranges and 0-200m depth-integrated). Compared to the size abundance relationship used in our study, the NBSS is an alternate way of computing a size spectra, with similar implications (lower numbers = steeper slopes, with more small plankton compared to large plankton, and vice versa).

- However, because of the very limited data availability in the Arctic and the fact that the PSSdb data only overlaps with our model simulations in the year 2009, with the UVP instrument, we do not feel that a formal comparison would be appropriate at this time, but hope that such a comparison could be done in a future study where the dates of the model simulation and the observations have a greater overlap.

IFCB, year 2013   Scanner, years 2011 & 2013   UVP, years 2009, 2013 & 2019

NBSS slope mean

−1.0   −0.9   −0.8   −0.7   −0.6

- Research into zooplankton trophic positions, such as the study by Choi et al. (2020) on trophic dynamics before and after polar night, reveals seasonal changes in diet and survival strategies in polar regions influenced by varying solar radiation and oceanographic conditions. Nitrogen isotope analysis in their study indicated stable trophic positions for species like *Parasagitta elegans (chaetognath)* and *Calanus spp. (copepod)*, whereas others like *Oithona similis* (copepod) showed elevated trophic positions post-polar night, potentially due to sustained energy intake enabled by adaptable feeding behaviors (Berge et al., 2015). Given the food-limited environment during polar night, most zooplankton are likely to reduce their food consumption (Grigor et al., 2014) or enter a period of rest until conditions improve. In contrast, opportunistic zooplankton can easily shift from algae to

protists (Iversen and Seuthe, 2011). Therefore, the observed seasonal variation in trophic positions in our study aligns with a higher representation of opportunistic zooplankton in our model simulations.

- We incorporated some of this text in the manuscript in L364-366: "..., and reflect seasonal variations in diet and survival strategies of more opportunistic zooplankton with adaptable feeding behaviors (Berge et al. 2015), as observed by Choi et al. (2020) in *Oithona similis* (copepod) exhibiting elevated trophic positions post-polar night."

Detailed comments

1. **L76-80: a simple schematic of the different types and groups of plankton would help.**
   - Figure 1 in Negrete-Garcia et al. 2022, the paper that first documented the model, has a nice figure describing the model structure. We will add text referring readers to that paper.
   - L78: "(Negrete-García et al., 2022, Figure 1)" .
2. **L99: "[…] is suited to study Arctic Ocean dynamics." I think a few more details on issues specific to the Arctic ecosystem, such as usual temperature, light and nitrate limitation should be provided before stating that.**
   - Thank you for your comment. We added the following text (L99-102):"This balance is crucial for studying the Arctic Ocean, where complex interactions between plankton communities and physical environmental factors play significant roles in ecosystem dynamics. The model's ability to represent diverse plankton functional types and their responses to varying nutrient and light conditions enable a nuanced understanding of Arctic biogeochemical cycles and food web structures."
3. **L140: while I do not suggest the authors modify the MARBL-SPECTRA model, I would like them to recognize that the choice of a type II functional response is not always optimal and can have a destabilizing influence in NPZD-type models (e.g. Gentleman & Neuheimer 2008, 10.1093/plankt/fbn078; Flynn & Mitra 2016, 10.3389/fmars.2016.00165).**
   - In the development process for MARBL-SPECTRA (described in Negrete-Garcia et al. 2022), we conducted extensive testing, tuning, and sensitivity analyses on the grazing formulation, including testing alternative functional forms (e.g., Holling Type III functional response). In total, we conducted nearly 400 test simulations, some of which is shown in the formal sensitivity analysis that we conducted for Negrete-Garcia et al. 2022 (see supplemental information in that manuscript). In our simulations and testing, we found no evidence that the grazing functional response induced oscillations and instabilities. Further, we found that the Holling Type II functional form performed the best in simulating the global biogeography of a number of key metrics, including chlorophyll, phytoplankton size structure, macronutrients, and zooplankton biomass.
   - Nonetheless, we acknowledge the reviewer's point and recognize that the grazing response choice may not reflect reality. Thus, we added the following text in the

model limitations section (L501-507): "Additionally, there are significant uncertainties and omissions in the representation of zooplankton, their grazing, and population dynamics in MARBL-SPECTRA. Zooplankton grazing (including food web structure, grazing parameters, and functional responses) is one of the largest sources of uncertainty in ocean biogeochemical models (Rohr et al. 2023, Hansen et al. 1997, Gentleman and Neuheimer 2008), and in this model, we do not resolve key zooplankton life histories or migration dynamics such as dormancy or diapause, which are important traits that allow organisms to survive unfavorable environmental conditions in the Arctic (Baumgartner and Tarrant, 2017)."

4. **L189: why integrating over 150m?**
   ○ In CESM, the default model diagnostics for most marine ecosystem variables are provided for the top 150 m. This is a practical choice, as it balances the need for increased diagnostics with the need to save disk space for the diagnostic-heavy ocean biogeochemical model. Thus, some of the key model diagnostics are depth-integrated biological variables over the top 150 meters, which typically encompass the euphotic zone. We chose to keep this default for a number of reasons: integrating over this depth ensures that the model captures the most significant biological activity related to carbon fixation and nutrient cycling. Additionally, the top 150 meters often include the mixed layer, where physical mixing distributes heat, nutrients, and gasses, further influencing biological and chemical processes. By focusing on this depth range, the model can more accurately represent the complex interactions and gradients that drive ocean productivity and biogeochemical cycles, while making sure we stay within our disk space quota.

5. **L251-253: I am not sure whether the authors speak about a usual spatial trend or a temporal trend resulting from the impacts of climate change?**
   ○ Thank you for your comment. We realize that the text was not very clear in distinguishing between spatial and temporal trends. In this context, we are referring to a temporal trend throughout the hindcast simulation. To clarify this, we added a reference to the Chukchi Sea anomaly plots, which illustrate these temporal changes more clearly (L256-257): "Specifically, the model illustrated a trend towards a more oligotrophic western Arctic Ocean basin (Fig. 8e)."

6. **L295: where do the nutrients come from? Remineralization or advection? Both?**
   ○ In this context, we mean that nutrients accumulate over the winter due to remineralization and lower consumption by microbes. However, horizontal and vertical transport also play a role.

7. **L451: this first sentence seems to contradict what was just said in the previous paragraph, probably because this model does not take into account sympagic production and its export towards the benthos in spring.**
   ○ Thank you for your comment. We acknowledge that the text may appear contradictory at first glance. In the previous paragraph, we discussed the general

trends observed over the hindcast period, highlighting the variability in fisheries production across different regions of the Arctic Ocean and the role of mesozooplankton biomass and carbon export to the benthos.

The sentence in L451 refers to specific years with lower ice fraction and higher temperatures supporting higher fish production. This observation is consistent with the overall trend that reduced ice cover and increased temperatures can enhance primary productivity and, consequently, fish production in some regions.

However, as you rightly pointed out, our model does not explicitly account for sympagic production and its export towards the benthos in spring. The absence of this factor in our model may lead to an underestimation of the contributions of ice-associated production to benthic and pelagic ecosystems during periods of high sympagic activity. This limitation might explain the perceived contradiction, as the model primarily captures pelagic processes and their direct impacts on fisheries production.

To clarify this, we incorporated the following text in the Model study limitations section (L497-L500): "Sea ice algae play crucial roles in shaping sea-ice associated ecosystems and biogeochemistry (Kohlbach et al., 2016), and their absence in our model may lead to an underestimation of the contributions of ice-associated production to benthic and pelagic ecosystems during periods of high sympagic activity."

8. **L465-466: please provide in one or two sentences indications on how this could have affected your conclusions, much like you did in the following paragraph.**
   ○ Thank you for your comment. We included the following in lines L507-509 "The omission of these traits may lead to an incomplete representation of zooplankton population dynamics and their seasonal availability as predators and prey. Consequently, this limitation influenced the underestimation of mesozooplankton biomass (Fig. 2o), potentially influencing our assessments of trophic interactions and fisheries production."

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