

I cannot recommend the editor accept the publication for the current manuscript. The reason is as follows.

Major comments

I think that the discussion is feeble and I think we need to deepen the discussion with more analysis. In addition, I suggest the authors reconstruct the section, especially after the biological parts (Section 3.2). The production of chlorophyll relies on the amount of nutrients and light. Therefore the turn of the sections for nutrients should be put before the chlorophyll and discussions should be more with the reproducibility of the nutrients. Furthermore, the purpose of this study is to show the reproducibility of the model. Since the ocean condition in the northwest Pacific is deeply affected by regional oceanic conditions seasonally, at least the authors should show and describe the reproducibility regionally and seasonally. How about making Tables to summarize the seasonal reproducibility for each parameter in the season (spring, summer, autumn, winter) at several depths in each region? If you put in such tables, it will help us and the authors to understand the advantages and disadvantages of the model, which will help us to improve it for the next opportunity.

Minor comments

Figures and Tables

- 1) Figure resolutions are low, and the memory and contour labels are too small. (Figs 3, 4, 5, 6, 7, 8, 9, 10, 11, 12 13, 14 16, 17). The memory size is too small to be easily identified. If printed, the diagram will be even smaller and should be noted in large size.
- 2) Fig 9 b, c d: The ranges of the x and y axis are too large, being not appropriate.
- 3) Fig.11: The authors examine the reproducibility in the Northwest Pacific, subarctic region, East Sea, and YECS in DIC and chlorophyll, but not in nutrients. The reproducibility of chlorophyll and DIC is connected to that of the nutrients. It is better to add the regional and seasonal variability in the nutrients.
- 4) Fig.12: Why don't you include the figure of silicate?

Section1

- 1) L57-158 (However, in the Northwest Pacific PDO and ENSO (Jung et al., 2017; Ma et al. 2020).) : I don't think so. I think the sentence could be misunderstood, so I think it should be reworded.
- 2) L73 (simulation of high biomass) : ?
- 3) L188 (1/12deg): It is helpful to add the distance as well.
- 4) L211 (Each model is ... 'r1l1p1fl'.) : I could not understand this sentence.

Section 2

- 1) L189-191: One of the interesting points in this model includes the climatological monthly mean discharges of 12 major rivers although the target region in the model is the Northwest Pacific. I checked the data availability of this data (L519-538), but there is no information. It is very helpful to write it.

Section 3

- 1) L245-248 (In this study, the water WOA18 data): The sentence is not appropriate here. You can move to the section 2 (l211-212 in the current version).
- 2) L261: Since the authors describe the NPIW between density 26.6 and 27.2 σ_θ . It is better to add the density contours, not the salinity contours in Fig. 5.
- 3) L270-282: Instead of the distributions of the surface salinity in the Northwest Pacific, how about showing the expanded figure targeting the YECS, because the WOA18 does not include the data in the YECS very much (I mean that it is hard to get the data), although some of the readers wanted to know the situation.
- 4) L414-415 “In particular, for the surface DIC concentration, regarding the latitude, the air-sea exchange and vertical mixing are balanced”: I checked Ishizu et al. (2021) and this sentence comes from the misunderstanding. In the subarctic region, biological processes are also impacted.

Section 4

- 1) Since I suggested reconstructing Section 3 by dividing results and discussion separately, in this section conclusions would be written only.