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Interactive comment on "Biological data assimilation for parameter estimation of a phytoplankton functional type model for the western North Pacific" *by* Yasuhiro Hoshiba et al.

Anonymous Referee #1

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Review comments on "Biological data assimilation for parameter estimation of a phytoplankton functional type model for the western North Pacific" by Hoshiba et al.

1. Summary

The authors conducted numerical simulations of a lower trophic level (LTL) ocean ecosystem model for the northwestern Pacific Ocean. To simulate a realistic temporal evolution of phytoplankton biomass, the authors employed a 1-D ecosystem model and optimized 23 ecosystem model parameters by a micro-genetic algorithm. The optimization was performed in two specific locations in the northwestern Pacific Ocean; one is the subtropical region (Station S1), the other is the subpolar region (Station

KNOT). The optimized model parameters were applied to a LTL 3-D ecosystem model simulations in the northwestern Pacific Ocean, for which a physical field obtained from an eddy-resolving ocean model were used by an off-line technique. The authors performed 3 different simulations by the 3-D ecosystem model: a simulation with an unoptimized ecosystem model parameters, a simulation with the optimized parameters for the subtropical and subpolar regions, and a simulation with the optimized parameters varying with the sea surface temperature from the subtropical to the subpolar region. Based on these experiments, the authors reported that they successfully improved the seasonal variation and vertical distribution of phytoplankton biomass.

2. General comments

It is interesting to see how the state-of-art LTL ecosystem model simulates the realistic temporal and spatial evolution of plankton biomass, since it constitutes an important part of the geochemical cycles simulated by a model. The LTL ecosystem model used for the current study is the one including iron cycle and its interaction with biomass distribution, which is considered to be necessary to improve the biomass distribution simulated by a model in high nutrient low chlorophyll (HNLC) regions such as the northwestern Pacific Ocean. I think the basic strategy of the current work is well-considered and reasonable for a step-by-step improvement of the LTL ocean ecosystem model - i.e., tune the model parameters by a 1-D model which is computationally inexpensive, and then apply the tuned parameters for a 3-D ecosystem model and examine how the spatial distribution of plankton biomass is simulated. However, it looks to me that the tactics employed in the study is not necessarily suitable for the purpose of the study as described below. The authors did not obtain reasonable improvement of simulated biomass by the 1-D optimization experiments, probably due to an inappropriate choice of tuning parameters, and failed to simulate realistic biomass distribution in the study area. Since the basic strategy is reasonable, I would recommend the authors to rework this issue addressing the following points.

3. Major points

- I would say that the result obtained from the 1-D parameter optimization is quite miserable and frustrating. If I understand the setup of the experiment correctly, the cost function is composed of only 24 values (12 months $\times$ 2 types of phytoplankton biomass), while the number of optimization parameters is 23. This is mathematically equivalent to optimizing 24 modeled values by 23 model parameters. For such an experiment design, one can expect nearly perfect fit of the modeled values to the observation, if the model appropriately describes the process concerned and the choice of the tuning parameter is appropriate. Nevertheless, the authors failed to fit the modeled phytoplankton biomass to the observed ones, nor even reproduce annual mean phytoplankton biomass: the simulated PS biomass at the subtropical station S1 is nearly 10 times larger than the observed one; the simulated PL biomass at the subpolar station KNOT is more than 5 times larger than the observed one during the winter period (Fig. 3), both of which are essential to describe the dominant species of phytoplankton in each area. To be honest, I do not find any benefit to apply the parameter sets, which provides such a large discrepancy from observation, for 3-D model simulations. I would recommend the authors to redo the optimization experiments by taking the points described below into account.

- How did the authors select the parameters used for the optimization? It looks to me that the authors selected a number of physiological parameters for the modeled phytoplankton, while did not select any parameters describing physical processes of the system (e.g., sinking rate, etc). As shown in Fig. 3b, the model exhibits too much PS biomass throughout the year, indicating nutrients in the euphotic zone are repeatedly recycled without being extracted from the system, probably due to insufficient parameterization for sinking/scavenging processes. This situation can be also seen in Fig. 7 - the discrepancy between the modeled and observed NO3 becomes larger after optimization (the unoptimized parameter set gave more reasonable result). Therefore, my recommendation is to reconsider the selection of tuning parameters, so as to tune the nutrient input to/output from the euphotic zone, and examine the improvement of nutrient distribution before examining modeled phytoplankton biomass. Otherwise the

derived optimal physiological parameters are not describing the realistic function of phytoplankton physiology.

- The current work did not take the advantage of the physical field obtained from the high resolution ocean model with data assimilation. During the last two decades, 3D ocean ecosystem models have been suffered from insufficient descriptions of physical fields obtained from low-resolution ocean models (e.g., reproducibility of mixed layer depth, its spatial distribution and seasonal variation, location of subtropical-subpolar boundary, coastal upwelling, etc.). Although the current study utilizes a physical field which is supposed to be free from such problems, I cannot find any distinguishable improvements compared to the studies using the low-resolution physical field. This is probably due to an insufficient implementation of nutrient cycles in the model, particularly the process describing input/output of nutrients into/from the system. My recommendation is, first of all, optimize the modeled nutrient cycles before tuning, examining or discussing the physiological parameters of the LTL ecosystem model. As far as I know, the coauthors listed here have done a number of excellent works and should have sufficient knowledge and experience for this.

- Description for the experiment design is insufficient. I think more description is necessary for a further review (as described in specific points).

4. Specific Points

- Line 24-64: Although the authors provided a concise review in introduction, the papers cited here seems to be largely biased toward the papers written by the co-authors of this work. I think it would be one more advantage of this work, if the authors mention the work by other groups.

- Line 33: vales –> values?

- Line 45-49: The construction of the sentence seems strange.

- Line 71-72: The authors described that the physical field used for the 3D experiment is obtained from a 3D-Var data assimilation, in which temperature (T), salinity (S) and sea surface hight (SSH) are assimilated. This means increments of T, S and SSH are added to the analysis field in each analysis time step. Is the physical field satisfy the mass conservation after the SSH assimilation? If not, how much amount of artificial sink/source of passive tracers should we expect? Is it not essential for the LTL ecosystem model simulation, particularly close to the sea surface?

- Line 76-77: What does "similar" mean? The authors should describe the difference from the cited work.

- Line 76-77: How did the authors provide dust flux for dissolved iron? The earth system model (Watanabe et al., 2011) contains iron in the dust? If not, how did the authors define the amount of iron concentration in the dust flux? Description is needed.

- Line 78: How did the authors define nutrient supply from the river? Does the CORE-2 provide nutrient concentration in river run-off? If not, how the author defined the value?

- Line 78: How did the authors define the nutrient supply from sediment over the shelf area?

- Line 79-80: This sentence needs citation.

- Line 68-83: Where does the iron in the system come from? Many studies addressed the importance of iron supply from the Sea of Okhotsk to the northwestern Pacific Ocean. How did the authors describe the iron supply from the Sea of Okhotsk?

- Line 68-78: How did the authors define the initial condition of nutrient distribution (nitrate, silicate and iron)? Description is needed.

- Line 79: Describe the range of restoring boundary layer and the restoring time scale.

- Line 80-82: How about the seasonal cycle of mixed layer depth and its spatial distribution? Is it well reproduced? I'm asking this question because I believe the mixed layer depth is the most important factor to regulate the nutrient supply into the euphotic

zone.

- Line 82-83: Are the nutrients in an equilibrium state after 1985-1998 integration? Is the nutrient distribution of the equilibrium state consistent with observations (e.g., Wold Ocean Atlas)?

- Line 88-89: The construction of the sentence seems strange.

- Line 89-90: What does the "similar" mean? The authors should describe which parameter(s) had been changed from Shigemitsu et al. (2012).

- Line 90: I suggest to use 'control-case' instead of 'default case'.

- Line 91-97: This experiment design is interesting, while I would suggest the authors to explain the basic philosophy behind this. It looks to me that introducing temperature dependency on many physiological parameters of the LTL ecosystem model is equivalent to rewrite the governing equation drastically, since some of the phytoplankton model parameterization already involve temperature dependency.

- Line 100-104: Description for the temporal and spatial resolution of the data is needed.

- Line 105-107: Why the authors used AVHRR data for SST-dependent case, instead of using SST obtained from the physical model? I think this experiment design may introduce a discrepancy: the modeled phytoplankton is controlled by two different temperatures (one is from the physical model and the other is from AVHRR). If I'm wrong, please explain which temperature is used to calculate the modeled phytoplankton biomass.

Line 114: A description for the selected parameters is necessary. The names of the selected parameters seem to be the same with the definitions in Shigemitsu et al. (2012), while it is not clear to the most of (potential) readers what do they mean. I suggest to implement a short description for each parameter in Table 2.

- Line 115: Again, what does the "similar" means? The difference should be described.

- Line 119-120: The construction of the sentence is strange, and I cannot really understand the meaning of the sentence. Does the sentence mean "the parameter set which provides the lowest cost is reserved"?

- Line 125: Why should the number of the population used in the genetic algorithm optimization be the same with the number of tuning parameters?

- Line 131: Why do the authors used the same weights for PS and PL? Is it based on uncertainties of satellite-derived biomass?

- Line 139: 'too small' –> 'smaller than the prescribed threshold'.

- Line 118-116: If I understand correctly, the parameter optimization by the 1-D model used a 1-year time window. Why the authors do not use a longer time window for the optimization? If the authors define the cost function by a multi-year window, the cost is more reliable. The computational cost is not essential in this case.

- Line 150-153: It looks to me that the following two sentences contradict each other; "the PS biomass was larger than the PL biomass at both St. KNOT and St. S1," and "Moreover, diatom, represented as PL, are a major group in the subarctic region.". Isn't it?

- Line 154-155: How do the authors evaluate the uncertainty of the biomass derived from satellite data?

- Line 149-159: Why does the optimized case exhibit such a large discrepancy from the satellite data? As I mentioned in the major points, I guess the selected parameters are not relevant to improve the discrepancy.

- Line 149-159: How much (percentage) is the reduction of the cost compared to the 'default case'? I cannot believe that the 'parameter-optimised case' for S1 gives smaller cost than the 'default case', since the PS biomass exhibits such large discrepancy from

the satellite data. Please show the total cost for 'default case' and 'parameter-optimised case', and cost for each month (e.g., a figure with the same abscissa as Fig. 3, while the ordinate is defined by cost for each month).

- Line 170-172: I'm a bit skeptical to the specification provided here. The authors employed a physical field obtained from an eddy-resolving model, yet they argued that the lack of the small-scale mixing is still responsible for the low-biased biomass close to the cost (or over the shelf). If this is true, what is the advantage to use the eddy-resolving physical field in this study? Since Fig. 4 and 9 successfully reproduced an eddying physical field, I guess a lack of nutrient supply from the seabed is a likely reason for the low-biomass close to the cost. How did the authors implement nutrient flux from seabed? Is it suitable to reproduce nutrient cycle over the shelf and/or close to the cost?

- Line 181-194: I think Fig. 6 (and associated analysis provided here) is an useful measure to characterize the performance of a LTL ecosystem model. But I would say, due to the large discrepancy between observed and simulated biomass (Fig. 3, 4 and 5), the analysis is not necessarily useful. I suggest to redo the analysis after a re-optimization of the model parameters as described in the major points.

- Line 196-203: How did the authors take into account the spatial and temporal representativeness of the modeled phytoplankton (and nutrient) for the comparisons? Since the horizontal distribution of the modeled properties has an eddy-scale fluctuation (e.g., Fig. 4), a direct comparison with in-situ data is meaningful, if and only if the physical model accurately reproduced the location and evolution of respective eddies. I'm not sure this is the case or not, since the physical model assimilated SSH (the reproducibility of realistic eddy fields depends on the spatial and temporal resolution of the assimilated SSH and the assimilation interval). If the location of respective eddies are not necessarily realistic, a mean value should be used for the comparisons (and standard deviation of the field should be used for the measure of uncertainty). Otherwise, we cannot argue which line in Fig 7 is closer to the in-situ data.

- Line 204-218: I'm also skeptical to the usefulness of the analysis and discussions provided here, since the background field for the modeled LTL ecosystem (i.e., nutrient fields) are not thoroughly examined nor confirmed to be realistic. The authors compared the vertical distribution of NO3 between model and observation only at one location. I think it is necessary to check the reality and weakness of the modeled nutrient fields before proceeding analyses for the modeled phytoplankton physiology. I suggest to compare the spatial and vertical profiles of nutrient fields (nitrate, silicate and iron) with available atlas and data sets.

- I found a number of strange construction of sentences. I think a consultation of the English sentences is necessary.

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