Dear Topical Editor,

Firstly, we sincerely apologize for our mistake of the wrong Figure 3 in the previous manuscript. We thoroughly replaced the previous Figure 3 and edited the section related to the Figure 3 in the revised MS.

Secondly, we thank the Editor, Referee #1 and Referee #2 for their useful comments given to our manuscript, as they very kindly gave us many good suggestions and pointed out our weak parts to be improved.

The referees mainly pointed out that (1) the authors did not obtain reasonable improvement of simulated biomass by the 1-D optimization experiments (e.g., Figure 3 in the previous MS) and (2) there were less evidences in the previous MS to judge the improvement by the parameter optimization.

We responded to the referees' comments point-by-point, as in the pages from the next: "Reply to the referee #1 and Reply to the referee #2". In the revised manuscript (another attached file), our revised manuscript is around 50% rewritten, with red-colored fonts (The main body became from 4,120 words to 5,488 words, and figures became 9 sheets to 12 sheets). In addition, we newly added three figures and descriptions to the revised MS, because of increasing the evidences for the improvement. We hope that this revised manuscript will be found to match acceptable level for your journal.

Finally, we apologize for our delayed submission of the revised manuscript.

Sincerely,
Yasuhiro Hoshiba and co-authors

Reply to the referee #1: We express our appreciation to the referee for the careful reading of our paper and giving useful comments. The referee's comments gave us the chance to find that Figure 3 of our discussion paper had a big mistake. We also would like to deeply apologize for taking up your time to read our previous manuscript (MS) with the wrong Figure 3.

Below are our responses to Referee. The referee's comments are in italic style and our corresponded replies are in regular style.

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.

I would like to appreciate useful comments by the referee, which gave us the chance for noticing that Figure 3 of our discussion paper was a mistake (Please refer to 'Errata for Figure 3'). According to the reviewer's comments, we revised our MS as detailed below:

Major points

1)   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 _ 2 types of phytoplankton biomass), while the number of optimization parameters is 23. This is mathematically equivalent to optimizing 24 modeled

We would like deeply to appreciate this comment that gave us the chance of finding our mistake. As you pointed out, in Figure 3, the optimized simulation results were clearly worse than the default results. As explained in the Errata published in the Discussion forum on 29th July, 2017, our mistake occurred in the process of data selection for drawing Figure 3. We used the optimized parameter set obtained for the CORRECT Figure 3 in the new MS. As a result, we did not have to change the rest of original results of simulation using 3-D model.

In the correct Figure 3 of the revised MS, simulated data in the optimized case (dashed lines) is clearly closer to satellite data (solid lines) than that in the default case (dotted lines). The cost functions in the optimized case, 1.61 and 0.17 at KNOT and S1, more improved than those in the default case, 13.55 and 1.11, respectively. Therefore, we revised Section 3.1 based on the new Figure 3 in the revised MS, specifically at Line 168-184.

The parameters for the optimization were selected, referring to those in Yoshie et al.

(2007). Yoshie et al. (2007) suggested that the selected parameters were influenced more than the other parameters such as sinking rate. The parameter selection was not described in the previous MS, and we added it at Line 132-134 in the revised MS.

It was not enough to describe nutrients-limitation in the previous MS, too. As nitrate is not limiting factor of phytoplankton growth at St. KNOT, vertical distribution of nitrate concentration could not be drastically changed by parameter optimization. We added a discussion of vertical distribution of silicate as a limiting factor, comparing with nitrate in Fig. 10 (b, c) in which vertical distribution of silicate in the optimized case is closer to the observation than that in the default case.

On the other hand, vertical *gradients, not concentrations,* of nitrate and silicate, that are tightly related to the nutrient input/output from the euphotic zone as you pointed out, are much closer than those in the default case in the optimized case. That is, in the optimized case, concentrations of nitrate and silicate both at the depth of around 50 m and 250 m are less than the observed ones, while those at the depth of around 50 m in the default case is higher than those in the optimized case, resulting in much smaller *gradients* in the default case than the observed gradients. As for nutrient concentrations at the depth of 250 m, the parameter optimization cannot improved them, because they are determined by physical processes in the ocean-basin scale as well as local biological processes. Descriptions including the above explanation was added to Line 270-285 of the new MS.

3) 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.

First of all, I would apologize for our mistake of wrong Figure 3 and for less informative figures, to judge the improvement by the parameter optimization. In order to increase the evidences for the improvement, we newly added horizontal surface correlation maps and vertical distributions of phytoplankton in the meridional section along 165°E to the revised MS as Fig. 5 and Fig. 6, respectively. We also added the vertical temperature and salinity distribution along 165°E for comparing and discussing

the reproducibility of mixed layer depth and location of subtropical-subpolar boundary to the revised MS as Fig. 7. The description as the above was added in Section 3.2 (Line 203-227) of the revised MS.

Specific comments

1)   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.

We deleted and added some referenced papers by other groups at Line 38-41 in the revised MS.

2)   Line 33: vales –> values?

Thank you for pointing out. We corrected it.

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

We revised the sentence in the revised edition at Line 51-57.

4)   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 height (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?

The physical field used in the offline ecosystem model (NSI-MEM) does not satisfy the mass conservation, but the passive tracers of the NSI-MEM in the offline setting do not have artificial sink/source without other boundary forcing and correction terms.

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

We wrote more the detailed description in conjunction with the below 6).

6)   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.

The description of iron process lacked in the previous MS.   We added the description process with some important parameter values to Line 75-79 in the revised edition.

7)   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?

The nitrate supply was calculated from the freshwater supply of CORE ver. 2 by the concentration of 29 μmol/l, and the silicate was by 102 μmol/l.   The description was added to Line 79-82 in the revised MS.

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

Nitrate and silicate sources were only from the rivers in this setting. Iron supply was only from the dust origin. The description was added to Line 82 in the revised MS.

9)   Line 79-80: This sentence needs citation.

The three-dimensional ecosystem model used in this study has not published yet in any journals, so that we added the web site address as the citation of this model participating to MARine Ecosystem Model Intercomparison Project at Line 86 in the revised MS.

10)   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?

  The complete reproduction of the water exchanging between the Sea of Okhotsk and the Pacific is very difficult to model, even with 0.1 degree grids. The iron supply from the Sea of Okhotsk could not be well reproduced in this study.   So, we did not focus on the coastal regions like around the Kuril Islands where the water exchanges between the Pacific Ocean and the Sea of Okhotsk.

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

  In the parameter-optimized and SST-dependent cases of the 3-D simulations, the physiological parameters were the same as the default case from 1st Jan. 1985 to 31th Dec. 1996. During the next one year (1997), the simulations were spun-up with the optimized or SST-dependent parameters, which simulation results on 1st Jan. 1998 were used as initial conditions for the 1998-year simulations in this study.   On the other hand, the parameters of the default case were not changed during 1985 to 1998.   The explanation about initial conditions was introduced at Line 104-108 in the revised edition.

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

  In order to buffer artificial high concentrations of phytoplankton around the side edge of the model domain, the restoring was conducted only in a few grids of the side edge with the time scale from 43 minutes to 3.6 hours.   The description about boundary conditions was added to Line 82-86 in the revised MS.

13)   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.

The vertical and horizontal physical field in this region had been confirmed to well reproduce the temperature, salinity and velocity by Usui et al. (2006). Therefore, we think that the mixed layer depth is also reproduced well. According to the comment, we also added the new description and figure (Fig. 7) at Line 223-227 in the revised MS.

14)   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., World Ocean Atlas)?

The nutrients and phytoplankton distribution do not strictly reach the equilibrium state, due to the sequential change of the physical field. In this study, we focus on the simulation results of surface phytoplankton fluctuation in 1998, and the physiological parameter-estimation for 1998 was conducted. There are not many in situ nutrients data in 1998 in the western North Pacific. Dissolved iron that is the main limitation factor for phytoplankton was hardly observed in 1998. In the revised MS, vertical section of phytoplankton and nutrients distributions along 165°E in June, 1998 from WOA was newly added to Fig. 6.

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

We improved the sentence at the revised Line 93-94.

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

There are some differences of the parameters between 1-D simulation of Shigemitsu et al. (2012) and the 3-D default case in this study. The parameters slightly changed from the 1-D simulation of Shigemitsu et al. (2012) were applied to the 3-D simulation as the default case. The difference from Shigemitsu et al. (2012) was described in the revised MS as new Table 2.

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

We changed the word in the revised MS, and in the following responses after this 17), we use 'control-case' instead of 'default case'.

18)   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.

The SST-dependent case was introduced to reduce an artificial gap in phytoplankton concentration at the boundary between the two regions (Fig. 1 (b)) due to a sudden change in parameter value.   The description was rewritten at Line 94-97 in the revised MS.

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

In the satellite phytoplankton data, the spatial resolution is approximate 0.042° and the temporal resolution is monthly mean in 1998.   On a daily scale, satellite data have a lot of missing value and is not appropriate for model validation.   The description was added to Line 112-123 in the revised edition.

20)   *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.*

We introduced the SST-dependent case just to smooth parameters around the boundary between two regions used in the Parameter-optimized case.   While we determined

parameter values using annually-averaged SST, there was not a significant difference between the AVHRR data and modeled data.

21)   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.

  Thank you for the good suggestion.   The description was added to Table 2 of the revised.

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

  The other non-estimated parameters of Control case were the same as those of Parameter-optimized.   We rewrote the line as follows:
  "The other parameters of the NSI-MEM were the same as those in the Control case." at Line 134-135 in the revised MS.

23)   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"?

  The parameter set which provides the lowest cost is reserved, and moreover, the μ-GA applies crossover to other parameter sets which have relatively lower costs for generating new parameter sets.   These process were repeated 2,000 times in this study. The sentence was changed at Line 138-142 in the revised MS.

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

  It is known from the previous study (Krishnakumar, 1990) that the number of the population should be similar numbers to that of the estimated parameters from the

perspective of computer resources.

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

It is based on a previous publication by Shigemitsu et al. (2012).   We used the same low value as some weights in the previous study.   We described it at Line 150-151 in the revised MS.

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

Thank you for the good comment. We changed the expression.

27)   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.

In this study, we focus on the seasonal variation of phytoplankton.   The physical condition of 1998 was used also in the 1-D model over the 1-year (1998) time window. The scope of this study (i.e. seasonal variation of phytoplankton in 1998) was added at Line 108 in the revised MS.

Considering the referee's comments of the following 28) to 31), and due to the mistake of the previous Fig. 3, the previous Section 3.1 was rewritten entirely.

28)   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?

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

30)   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.

31)   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).

32)   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?

Nutrient flux from the seabed was not considered in this study. As the referee suggested, it might be the reason for the low-biased phytoplankton biomass close to the coast.   However, the main focus of this study is phytoplankton seasonal fluctuation in the pelagic and open ocean (deeper 200 m).   Considering this referee's suggestion, we improved the part (Line 197-202 of the revised MS).

33)   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.

We sincerely apologize for the wrong Figure 3 in the previous MS.   After the

correction to Fig. 3, we think that the estimated parameters and the following analyses are decently useful. In order to verify the simulation validity, horizontal distributions of correlation to satellite data, as well as a comparison with observed data of vertical section along 165°E from WOA, were added as Fig. 5 and Fig. 6 in the revised MS, respectively.

34) 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.

We think that mesoscale eddies are reproduced in some extent in the model, due to the multivariate 3D variational analysis scheme already built in the physical model. As you pointed out, however, there is the possibility that the location of a mesoscale eddy on the station cannot be completely reproduced. We added error bars which depicted the maximum and minimum values in ± 0.3° around the grid of St. KNOT to Figure 10 of the revised edit.

35) 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.

We newly compared the data in situ and the simulation results of nutrients and phytoplankton concentrations at Line 210-222 in the revised MS.

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

We have received English proof-reading before this resubmitting the manuscript.

Reply to the referee #2: We express our appreciation to the Referee #2 for giving useful comments for our paper. We also would like to deeply apologize for taking up your time to read the previous manuscript (MS) with the wrong Figure 3. Below are our response to the comments. The referee's comments are in italic style and our corresponded replies are in regular style point-by-point as follows.

General comments and major points

Parameters estimation in biogeochemical models is a critical challenge and assimilation of data is an important tool to better constrain these parameters. This manuscript is thus dealing with a topic of relevance. It is clearly written and presented, figures are of good quality. I have however important criticisms concerning the methodology used in the manuscript. Mainly, I find that the paper does not provide enough convincing arguments that the way data assimilation is used here clearly improves model performances. The use of data assimilation requires a thorough quantitative assessment of model performances before assimilation and after assimilation and this assessment is insufficient here. Only the seasonal cycle of monthly averaged surface concentration of phytoplankton is presented and broadly compared with observation and this is not sufficient. I would recommend that the authors strongly improve that point. Then, I would like that the authors comment of the propagation of errors that may happen when 1) interpolating a global chlorophyll product to their grid (is there not a regional product available for the region?, 2) converting global chlorophyll to PFT, 3) converting chlorophyll to nitrogen values. The paper does not convincingly demonstrate that model performances are really improved thanks to the assimilation of such data (conversely looking at e.g. figure 3, we have the feeling that data assimilation degrades model performances see my details comments below). The readers would be better convinced by the gain obtained by the assimilation of such data if the authors presents a thorough error assessment computing error statistics like bias, RMS,: : : Therefore, I find that the paper cannot be accepted for publication in its present form and would require a substantial revision.

First of all, I would like to sincerely apologize to you for our mistake of Figure 3 in the previous MS. As you mentioned, the simulation results by the parameter-optimized case were clearly worse than those by the default case in the wrong Figure 3 in the previous MS. We have explained how the mistake occurred in Errata published on the Discussion forum on 29th July, 2017. As explained in the Errata, our mistake occurred in the process of data selection for drawing Figure 3. In the new MS, this was corrected. On the other hand, we found that we did not have to change the rest of original results due to the mistake of Figure 3. In the correct Figure 3 of the revised

MS, simulated results in the optimized case (dashed lines) are clearly closer to Satellite data (solid lines) than those in the default case (dotted lines), showing a significant improvement. In addition, we revised descriptions related to the previous wrong Figure 3 (i.e. Section 3.1 of the revised MS).

We agree the referee's suggestions that the previous MS had too less figures and it was difficult to judge the improvement by the parameter optimization. We newly added Figs. 5 and 6 to the revised MS. Figure 5 shows horizontal correlation maps between the model and the satellite, and Fig. 6 depicts vertical distributions of nutrients and phytoplankton. Figure 5 shows a broader horizontal assessment of model results rather than the point comparison (e.g., the previous Fig. 5). Figure 6 is drawn for the assessment of vertical distributions of chlorophyll in the meridional section along 165°E. The results and discussions of the figures were added to Section 3.2 in the revised MS.

As for your comment about interpolating a global chlorophyll product to their grid, the detailed evaluation is difficult, the interpolation might be one of the error sources, as you pointed out. On the other hand, the previous study (Gregg and Casey, 2004) showed that the global satellite chl-a compares well with in situ data even in the WNP region (r^2=0.71, RMSE % log error of 31.6). The description was added to Line 120-123 in the revised MS.

As for your comments about an error propagation from chlorophyll to nitrogen values through the conversion from chlorophyll to PFTs, the propagation cannot be assessed at the moment due to a lack of data of the in situ nitrogen-chlorophyll ratio for each PFT matched up with the satellite data (Uncertainties in chlorophyll a and PFTs are published in Gregg and Casey 2004 and in Hirata et al. 2011, respectively). Hence, we used the Redfield Ratio of 6.625 for Carbon-to-Nitrogen ratio, and then the Carbon-to-Chlorophyll values of 50 (PL) and 125 (PS) that result in the N:Chl ratio of 1:1.59 (PL) and 1:0.636 (PS), respectively, as previously used in Shigemitsu et al. (2012). We described this point in the descriptions of Fig. 3 of the revised MS for readers as follows:

"The unit conversion between the simulation data (molN/m^3) and the satellite data (gchl-a/m^3) is referred to as the nitrogen-chlorophyll ratio of PL= 1: 1.59 and PS= 1: 0.636 (Shigemitsu et al., 2012). The same conversion of nitrogen-chlorophyll is used to Fig. 4, Fig. 6, Fig. 8 and Fig. 10."

Detailed comments
1)   Line 16-17: The authors say "The approach used a one-dimensional emulator that

referenced satellite data". Please clarify what you mean by "emulator" and "reference to satellite data". I guess that you mean that the estimation of parameters is based on a 1D simulation using satellite data for constraining biogeochemical parameters.

We improved the sentence at Line 16-17 of the revised MS.

2)   Lines 17-18: The comparison with other models that do not assimilate data is only limited to the model used in this paper and cannot be considered as a general feature. Please reformulate.

The simulation results of NSI-MEM in this study are not compared with those of other models.   We reformulated it at Line 17-19 of the revised MS as follows:
"The 3-D NSI-MEM optimised by the data assimilation improved the timing of a modelled plankton bloom in the subarctic and subtropical regions compared to the model without data assimilation."

3)   Line 33-35: "Physiological parameters have often been tuned up empirically and arbitrarily, although ecosystem models have recently added more parameters to increase the number of prognostic and diagnostic variables". It is not clear here why there is an opposition of the two parts of the sentence (use of although). The fact that the number of models parameters increases makes it more and more difficult to tune them automatically. On the other, I agree that ecosystem models are more and more complex with the aim to increase their reliability but the increased number of uncontrolled parameters may make them less realistic.

According to the referee, we rewrote the sentence at Line 43-45 of the revised MS as follows:
"Moreover, physiological parameters have been often tuned up empirically and arbitrarily. The fact that the number of parameters increases with prognostic and diagnostic variables makes it more difficult to tune them."

4)   Line 36: Please clarify what you mean by Â´n a reasonable estimate of the physiological parameters Â˙z Line 40: set and not set?

This description was rewritten at Line 45-47 of the revised MS as follows:

"In order to reproduce observed data such as spatial distribution of phytoplankton biomass and timing of a plankton bloom, it is required to reasonably estimate the physiological parameters."

5)   Line 42: I find that the use of "This algorithm"" may be confusing. I guess that here the authors refer to the model NSI-NEM and so I would use "model" instead of "algorithm".

"This algorithm" here means the micro-genetic algorithm (μ-GA). We changed the words to "the μ-GA" at Line 52 in the revised MS.

6)   Line 45: I guess that the sentence is incomplete. I would add something like 'based on" before following the…

We improved the sentence at Line 50-57 of the revised MS.

7)   Lines 44-49: This part would be better placed in the section on materials and methods

We would like to put this part in Introduction, because the point here is that it was developed by the previous study in Shigemitsu et al. (2012). We revised the part at Line 50-57 in the revised MS.

8)   Line 50 : "and the Parametres-optimized approach" this is vague I would say and a micro genetic algorithm to optimize..

The part was eliminated, due to the substantial revision of Introduction.

9)   Lines 55-60: I would suggest to add on Figure 1 that shows the region of interest, a schematic representation of the main circulation features (i.e. Kuroshio, Oyashio currents, + transport of iron from the sea of Okhotsk).

According to the referee's suggestion, the schematic representation of the main circulation features were added to Figure 1 (a) in the revised MS.

10)   Line 62: It is not clear whether this paper is the one that force NSI-NEM by a 3D circulation fields or whether the novelty lies in the fact that the physics was of better quality than before due to data assimilation. Please clarify.

In this study, while the NSI-MEM runs offline using the physical fields simulated (and assimilated) using an OGCM, the physiological parameter estimation (using biological data assimilation) was also applied simultaneously to the NSI-MEM using µ-GA scheme .   We clarified the part in conjunction with the below 11).

11)   Line 62: what is the "data assimilated physical field"? Model results with data assimilation. I miss in the introduction a clear description of the objectives of the paper. It is mentioned that the region presents a high variability (and so models have limited capacities in presenting the physical and biogeochemical characteristics of the region) but then the authors focused on monthly averaged values. This is confusing. I also recommend a brief description of the sections of the manuscript.

The physical field were also data assimilated by 3D-VAR, but that was not conducted by our study. For understandability and comprehensibility, we improved the part and added the brief description of the sections at Line 58-63 in the revised MS.

12)   Line 69: please clarify which physical fields you are using. I guess that you mean OGCM model outputs.

We added some words for clarity at Line 66-71 of the revised MS.

13)   Line 88: What do you mean by "employed by those"?

We meant "For each province, the respective parameters estimated by the µ-GA and the 1D NSI-MEM were employed to those in the 3D NSI-MEM.".   The sentence was

rewritten at Line 93-94 in the revised MS.

14)   *Line 113: What about lateral inputs/exports?*

Lateral inputs/exports, horizontal advection and diffusion were not considered in the 1D model.   The description relating to that was added to Line 263-265 in the revised MS as follows:

"These suggests that effects of horizontal advection such as mesoscale eddy is important for the daily reconstruction of the profile in the upper layer as the effects are not included in the 1D model."

15)   *Line 114-115: Usually the set of parameters on which the optimization efforts are concentrated are selected based on sensitivity/idenifiability studies. Here this is not really clear how they are selected. (how many parameters are in the model?)*

We determined that the 23 of 107 physiological parameters used in NSI-MEM were optimized, according to the previous study (Yoshie et al., 2007).   The description was added to Line 131-134 in the revised MS.

16)   *Lines 130-131: Please comment on the assign weights in the cost function. 0.1 µmol/l seems to be quite low.*

We adopted the same low value as some weights used in Shigemitsu et al. (2012) who successfully implemented the µ-GA for our target region.   We describe it on Line 150-151 in the revised edition.

17)   *Lines 149- +Figure 3: The performances of the model at the two stations with data assimilation are not convincing at all. The model without data assimilation has better performances than the model with data assimilation. The authors mention "The seasonal variations of PS in the Parameter-optimized case (dashed lines) for the two stations simulated by the satellite data (solid lines) were more accurate than those in the Default case (dotted lines)." Looking at Figure 3b, this is not what we can see.*

We sincerely apologize for our mistake of the previous Figure 3. We totally changed Section 3.1 related to Figure 3 in the revised MS.

18)   Figure 4: I would recommend to compute errors metrics like bias, RMS, .. in order to quantify more objectively model data misfit and to assess whether the assimilation of data really helps to improve model performances.

Thank you for good suggestion. We newly added Figure 5 and Figure 6 to the revised MS, to increase the evidence for improvement by the parameter optimization.

19)   Figure 1: please explain why you have different colors for the boxes?

The colors are divided by the features (e.g., phytoplankton, zooplankton, particulate matter and nutrient). The description was added to the caption of Figure 2 in the revised MS.