Reply to the referee #1: We express our appreciation to the referee for the careful reading of our paper and giving comments. The below are our responses to Referee. The referee's comments are in italic style and our corresponded replies are in regular style.

As a whole the manuscript becomes better than the previous version, while I think it still needs a further clarification at least the points mentioned below. In addition, I'm still frustrated to the discussion, for example, those in lines 311-319, which looks to me contradicting (or inconsistent) to the aim of this study described in introduction. Anyway, it might be a matter of taste, and I have no further comments contributing to improvement of the manuscript.

I would like to appreciate your understanding of our previous revision. According to the referee's comments, we revised our MS (manuscript) as detailed below:

Specific comments

1) Line 70-74: The description of the dynamical field used for the experiment is still ambiguous (or confusing). Additional sentence(s) are necessary for clarification. The manuscript describes that "... 3D variational analysis scheme that synthesizes the observed information such as temperature, salinity and sea surface height", while it also describes "... but the amount of water mass was conserved (Fujii and Kamachi, 2003)", without any reasoning. The following sentence in the response from the author also made me confusing: "Because the 3D-Var system used in this study only changes the temperature and salinity, and does not directly change the SSH and velocity". What does "directly" mean? Are SSH/velocity changed "indirectly"? The answer is "No", if I understand correctly the 3DVar system in Fujii and Kamachi (2003). I suggest to revise line 72-74 as follows,

"The T/S fields are obtained from a free model run plus T/S increments. These increments are derived from the 3DVar system so as to minimize model - observation misfits of T and SSH. Different from the T/S fields, dynamical fields (e.g., u, v, psi, SSH etc.) are not modified by the data assimilation (i.e., the physical field holds mass conservation, which is necessary to run the ecosystem model with a consistent manner)."

If I'm wrong and the authors disagree the revision, further explanation is necessary in this part.

We addressed the more information about the 3D variational analysis scheme at Line 72-75 in the MS.

2) Line 210-216: Since the time series are composed of monthly mean of 12 data, the 2-month lag-correlation is calculated by only 10 data points, which looks to me not always sufficient for scientific robustness. I suggest to provide a limitation regarding robustness of the result due to the short time series.

We added the maps of significance level in Figure 5 and mention it at Line 212-215 in MS.

3) line 243-254: I agree that the diagram shown in Fig. 9 is a concise way to summarize the results, whereas it is misleading to show this figure without mentioning a limitation of this analysis. Since the current experiment deals with monthly mean data, the phase (angle of the arrows) have a resolution of 30 degree. Therefore "True" does not mean a perfect match to the satellite data, while it means a match with 1-month (30 degree) error range.

We deleted "perfect" and revised the expression at Line 249 and the figure caption in the MS.

4) Line 290: ".. in which much smaller gradients than the observed gradients are found". This is valid for silicate only. Isn't it?

We revised the sentence at Line 293 in the MS. We meant that the vertical gradients both of silicate and nitrate in the OPT are closer to the observed data than those in the CTRL.

5) line 311-319: After reading the paragraph here, I'm wondering what is the benefit of the current parameter optimization. If the ecosystem model parameters are changing with the condition mentioned in the manuscript (e.g., SST, nutrient abundance, light intensity, ..), what can we learn about the mechanisms governing ecosystem behavior from the optimized parameter sets for a specific situation? It looks to me that the dependence of the parameters on such environmental condition reveals insufficiency of the model formulation. Maybe further discussion/explanation which is consistent with the aim of this study should be necessary here. I guess at least the last co-author of the manuscript has an idea explaining the background philosophy of this experiment.

As the referee suggested, our philosophy for data assimilation and ecosystem models was described at Line 319-326 in MS.