Dear Prof. Koji Suzuki

The two referees and I were pleased to find your significant improvements in the revised manuscript. However, the two referees still have some concerns about your discussion (see the attachment). So please make a further revised manuscript with point-by-point responses to the referees' comments.

We appreciate the opportunity to revise our manuscript again. We revised our manuscript based on all the comments from two reviewers and checked the manuscript again by ourselves. We believe our manuscript is now prepared for publishing from Biogeosciences.

Response to Reviewer #1

I have read the revised manuscript by Kodama et al. and am pleased to see that the authors have taken time and effort to improve it. I have some minor concerns as follows.

We appreciate Reviewer#1 again for his/her careful reading and valuable comments. We also thank the opportunity to revise our manuscript. We revised our manuscript based on the comments.

1) As mentioned in the Introduction (Lines 65-68), previous studies have found that the nitrate supplied to the SOJ surface water originates from Kuroshio, ECS, Changjiang diluted waters, atmosphere, and local deep waters. In my opinion, these findings suggest multiple nitrogen sources that can support phytoplankton growth. Can authors clarify whether the present data support the previous results or provide any new understanding?

We appreciate the comments. We are sorry that our previous revision is insufficient. Our descriptions which were pointed were confusing. To clarify our description, we arranged the sentences (L60–71).

2) I still have a question about the explanation of the 15N-depleted signals of POM for class I. The authors excluded the possibility of phytoplankton growth that produces low δ15NPOM in the nitrate-replete condition. I do understand that the authors claimed no relationship between δ15NPOM and Chl a, which implied that δ15NPOM may not be determined by Chl a. As I stated before, the Chl a concentrations for class I were not low; instead, they seemed to be the highest among the four classes, while the authors argue that the bloom generally occurs in spring. Higher Chl a concentrations indicated phytoplankton growth. Moreover, the δ13CPOM values for class I were moderate and C/ N ratios for class I were close to 6, suggesting that the marine-origin POM significantly contributed to the bulk POM. These results together demonstrate that the in situ and/or remote produced POM by phytoplankton may not be fully ruled out.

We appreciate the comments again. We reconsidered Reviewer#1's comment on this phenomenon, and we recognized this comment is more reasonable than our discussion. The phytoplankton carbon and nitrogen assimilation in winter is not clearly reported in the Sea of Japan, and we could not reject the Reviewer#1's comments. We revised the discussion and added the descriptions in the discussion part (L394–398).

Response to Reviewer #2

I am happy to see the revised version of the manuscript. It has become clearer and more succinct. The responses to my comments are satisfying and I can understand what the authors aimed to present. I have minor comments on the text.

We appreciate the careful and kind comments from Reviewer#2. We revised the commented parts based on the comments.

Line 180: would it be Fig. 3f-j?
We appreciate the comment. We revised (L181)

Line 278: delete "utilization".
We revised as suggested (L279).

Line 283: The negative relationship between "d13CPOM and" salinity...
We added (L284).

Lines 301-305: Let me clarify the logic. Does this mean that "in class III, there is deep mixing of water column and thus high nitrate; however, light intensity was low in winter and spring (it seemed that most points in class III were spring and winter samplings) and deep mixing brought phytoplankton to deeper water, which lowered the phytoplankton activity and d13CPOM"?

We thank the suggestion. Most of Reviewer#2 comprehension is correct, but we note not only deep water mixing, but also the sampling depth lowered d13CPOM due to the low light intensity and low phytoplankton activity. We clearly state this (L306–308).

To the comment 1-(3) from Reviewer #1, I guess what the reviewer meant is that you mentioned that previous studies (Umezawa et al. 2021; Umezawa et al. 2014) already found that the primary production is supported by multiple identified N sources in the northeastern ECS and what this research found does not contribute much to new findings. However, your research area is not exactly the same as previous studies (though SOJ receives influences from the ECS). I think this is still a good dataset to explore the relationships between N sources and its effect on ocean production.

We appreciate this comment. Thankfully, Reviewer#1 commented it us again. We revised the sentences following your comments (L60–71).

And the comment 2-(3) from Reviewer #1 may infer to this statement "Additionally, the elevated d13CPOM may be due to sediment resuspension in the Changjiang estuary..." (Lines 285-290), and "These results suggest that POM with high d13CPOM is transported into the SOJ and influences the spatiotemporal variation of d13CPOM in SOJ,..." (Lines 295-299). Indeed, this is confusing. Maybe you can mention that the salinity is not the main factor affecting d13CPOM in the end of this paragraph to emphasize that chl-a and phytoplankton photosynthesis matter.

We also appreciate this comment. As suggested by Reviewer#2, we revised the paragraph and reordered the sentences. We cannot reject the effect of salinity, so we did not mention "salinity is not the main factor affecting d13CPOM" (L393–396).