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.
Line 180: would it be Fig. 3f-j?
Line 278: delete “utilization”.
Line 283: The negative relationship between “$\delta^{13}C_{POM}$ and” salinity…
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 $\delta^{13}C_{POM}$”?

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.
And the comment 2-(3) from Reviewer #1 may infer to this statement “Additionally, the elevated $\delta^{13}C_{POM}$ may be due to sediment resuspension in the Changjiang estuary…” (Lines 285-290), and “These results suggest that POM with high $\delta^{13}C_{POM}$ is transported into the SOJ and influences the spatiotemporal variation of $\delta^{13}C_{POM}$ in SOJ,…” (Lines 295-299). Indeed, this is confusing. Maybe you can mention that the salinity is not the main factor affecting $\delta^{13}C_{POM}$ in the end of this paragraph to emphasize that chl-a and phytoplankton photosynthesis matter.