Response to Reviewer#2

This study presents the spatial and temporal distribution of carbon and nitrogen stable isotope signals in euphotic particulate organic matter (POM), which is primarily made up of autotrophic phytoplankton, in the southern Sea of Japan. Elucidating the sources of POM and the potential biogeochemical processes that modify POM isotopic characteristics in the marginal sea, which is impacted by multiple currents and complex biogeochemistry, is interesting and important. In my opinion, the manuscript's overall structure and analyses are satisfactory. However, I concur with Referee 1's remarks and would like to see more in-depth discussions of the data. Here are a few additional comments regarding the analyses.

We appreciate Reviewer#2 comments very much and also apricate to revise our manuscript. We revised manuscript based on Reviewe#2 comments as well as Reviewer#1 comment. We found some careless mistakes in my data sets. We revised the sentence for the class I samples due to our careless mistake.

The data points in class II are significantly more abundant than the other classes and exhibit high variations in both stable isotope signals and environmental conditions. However, the majority of data points in this class were collected during the summer, which may render seasonality insignificant. If only data from this class are used, may it be possible to establish correlations between δ13C/δ15N and environmental factors, particularly Chl-a and nitrate, that could explain the variation in δ13C/δ15N?

We appreciate reviewer #2's comments. We agree with their opinion and have already done what they suggested. We noted in our manuscript that we also analyzed the data for only the summer. When we limited the data to class II, we observed some differences. The $\delta^{13}C$ was explained by season, temperature, salinity, and nitrate concentration. The trend was the same as with the full data set. The sampling layer, latitude, and chlorophyll *a* concentration were not selected. In the case of $\delta^{15}N$, only sampling depth and salinity were selected as the explanatory variables. The model of $\delta^{15}N$ was poorly explained by these two parameters (the deviance explained was less than 10%). We believe these results are more difficult to explain, and considering the deviance explained, these models are inadequate compared to the model we described in the manuscript. We carefully considered the data and

used appropriate statistical methods. We believe that our results are valid and that our conclusions are justified.

In reference to the first comment, classes III and IV mainly consist of summer and a few spring samplings, but the δ13C values in these groups differ significantly from class II. The proposed explanation that high and low δ13C is related to the strength of photosynthesis is accepted, albeit inferred indirectly from nitrate depletion and high C:N ratios. Although there is no data available to confirm it, the stable isotope difference between nitrate and POM may reflect the depletion of nitrate caused by phytoplankton activity. Also, please provide relevant literature that links high phytoplankton C:N ratios with high photosynthetic C assimilation.

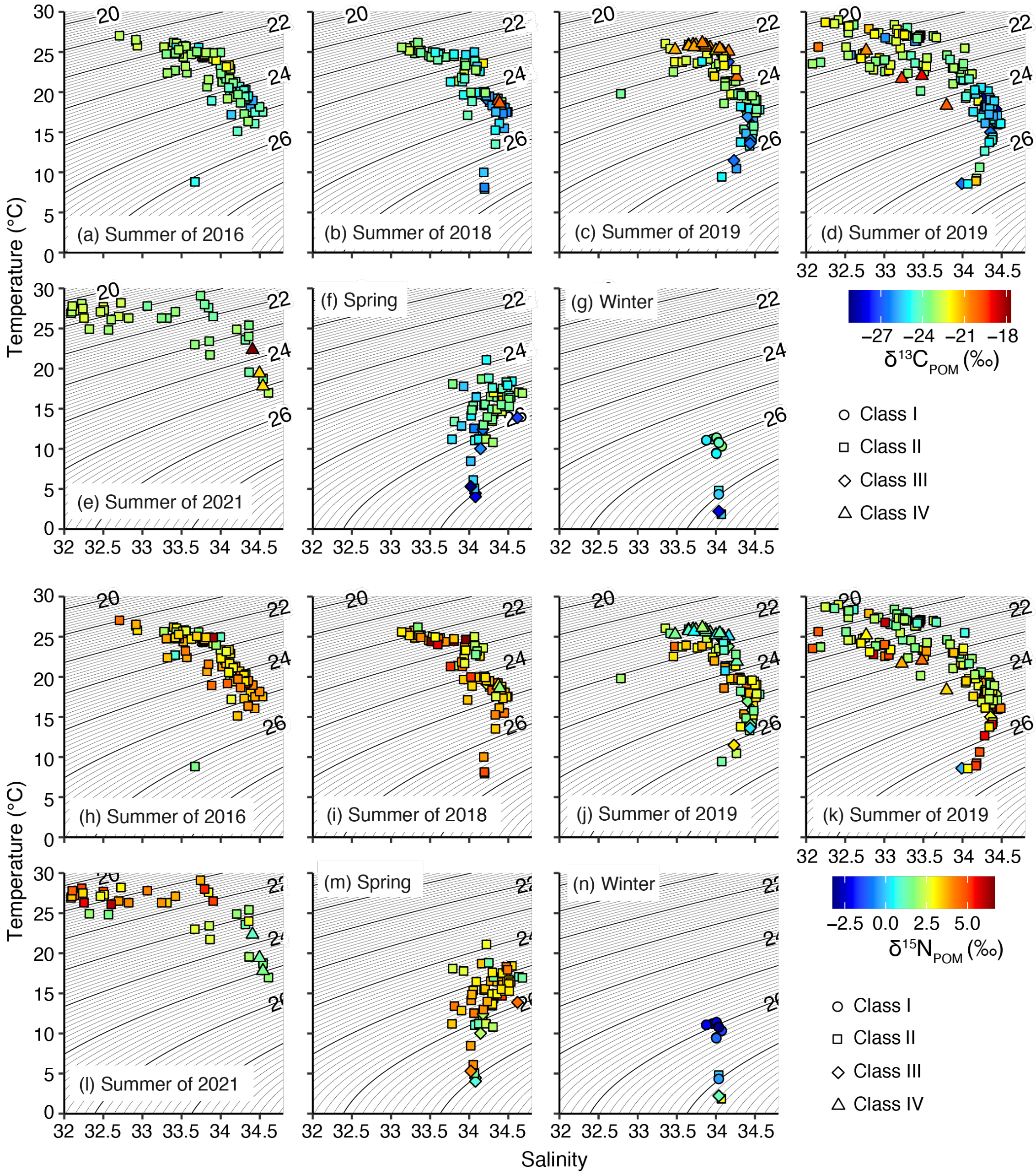
We appreciate the comment. We added the description about the C/N ratio as follows: "In addition, the C/N ratio was not selected in the GLM but had a positive relationship with $\delta^{13}C_{POM}$ (Figure S1). Tanioka and Matsumoto (2020) reported that C/N ratio elevates with the increase of light based on the meta-analysis, supporting the active photosynthesis elevates both C/N ratio and $\delta^{13}C_{POM}$" (L278–281).

I am unclear about why it was mentioned that "class IV samples were mostly from the surface layer during the summer, but the surface layer samples during summer were not classified as the class III (Lines 310-311)." Does this suggest that biological processes differ between the surface and deep waters? For instance, in deep water where nitrate levels are high but light intensity is low, and thus phytoplankton photosynthesis is limited, resulting in less significant δ13C fractionation.

We are sorry for our confusing descriptions. We want to say that class III and class IV water was present in different season and/or layer. Since reviewer #2 could not understand, we revised the order of the sentences and removed the unclear sentences, and we emphasize the photosynthesis (L303–309).

I am curious as to why the nitrate-σt biplot (Figure 7b) was investigated. Given the high frequency of below-detection limit NO3-, this biplot may not be particularly useful for identifying water masses and grouping POM stable isotope signals.

We thank this comment. Now we agree with this comment, and thus we only show the T-S diagram. The T-S diagram was revised based on reviewer#1 comment (Figure 7).



Minor comments:

Lines 393-394: USLW should be ULSW in the two sentences.

We appreciate this comment. We revised (L383 and 385)

Figure 9: According to Fig. 5a, Class 1 includes almost exclusively winter data. The δ15N distribution should be similar in 9a and 9b. However, the average δ15N shown in in 9b is much higher, which obviously deviated from the data distribution. Please check the codes creating this subplot and fix it.

We appreciate this comment. To calculate the lsmean values, the categorical values were set as the majority ones. And thus, the $\delta^{15}N$ value in winter was calculated as class II and surface. This is why the winter lsmean value looks high in Fig 9b. In addition, there are some plots behind the circles and bars. We added the description in the caption of Fig 8 and 9.