Summary

This manuscript by Kodama et al. investigated carbon and nitrogen isotope ratios of particulate organic matter (POM) in the upper water column of the southern Sea of Japan based on multiple years' observation to evaluate the effects of lateral transport of POC from the ECS and identify the nitrogen sources that support the POM production. I applaud their efforts that compiled abundant data for the first time in this region, but recommend the authors revisit these valuable data they collected and interpret them in a better way. The discussion in the present manuscript needs to be strengthened, since there are a lot of handwavy statements and speculated interpretation. I have some major concerns that should be addressed.

1. The abstract needs to be improved. One of the major goals in this work, the effects of lateral transport of POC from the ECS, was not indicated in the abstract. And why was the characteristics of class I samples emphasized here?In addition, the authors attributed variations in the $\delta^{15}N_{POM}$ to the temperature and salinity. How do these two parameters change the $\delta^{15}N_{POM}$? Do the authors mean different water masses with variable N isotope endmember that support the formation of POM? At last, their conclusion suggested multiple nitrogen sources contributing to the primary production in the SOJ, which seems to be consistent with the previous findings stated in the introduction. In this regard, the contribution of these plentiful dataset could be limited.

2. The interpretation of the $\delta^{13}C_{POM}$ variations. I suggest the authors to examine the water masses during the sampling period in the study area, for example, based on T-S diagram. This may help identify the POM sources and the sources of nitrate which is related to the $\delta^{15}N_{POM}$.

   POM with high $\delta^{13}C_{POM}$ were originated from the less saline ECS waters (Line 300-304). Since the salinity in the northeastern ECS is generally higher than 32 in summer (e.g., Kubota et al., 2015; Yang et al., 2021), it is hard to understand that the water mass with the lower end of salinity (<30) observed in this study originates from the ECS/the Changjiang diluted water. Also, when checking the spatial distribution of $\delta^{13}C_{POM}$ in Figure 3d, I do not see higher $\delta^{13}C_{POM}$ at sites close to the Tsushima Strait where the influence of the ECS water may be higher. By contrast, higher $\delta^{13}C_{POM}$ seem to be observed in the eastern part of study area.

Line 312-314: I do not agree that the high $\delta^{13}C_{POM}$ can not be observed at sites with low-nutrient and low-Chl a. For example, in the oligotrophic oceans, e.g., the SCS basin (Liu et al., 2007), high $\delta^{13}C_{POM}$ can be detected in the surface water. In addition, considering the observed range of C/N ratio which were mainly close to the Redfield ratio, this may suggest a predominantly marine origin of POM. Liu et al. (2007) suggested that lower $\delta^{13}C_{POM}$ (theoretically down to ~ -27‰ at 100 m) in the subsurface and deep layers were due to reduced specific growth rate, which may produce larger isotope fractionation. Could this mechanism influence the spatiotemporal variations of $\delta^{13}C_{POM}$ in this study, especially for the data from the subsurface and deep waters and from the growth-limited season (i.e., winter).

3. The interpretation of the $\delta^{15}N_{POM}$ variations. The $\delta^{15}N_{POM}$ increased with depth, which may imply that the POM degradation preferentially remove ^{14}N from particles, as indicated by many previous studies (e.g., Casciotti et al., 2008). In this case, the classification of $\delta^{15}N_{POM}$ which combined surface, subsurface and deep samples together would complicate the identification of nitrogen sources.

   The less saline ECS waters were suggested to be contributed to high $\delta^{15}N_{POM}$ in the SOJ (Line 334-336). Similar to the spatial pattern of $\delta^{13}C_{POM}$, in the western SOJ close to the Tsushima Strait the $\delta^{15}N_{POM}$ values were lower.

   I can not understand the simulation shown in Line 377-389, the authors need to explain in details how the various nitrogen sources change the relationship between the $\delta^{15}N_{POM}$ and nitrate concentration.

   It is interesting that such ^{15}N-depleted signals were observed on the POM. The authors excluded the possibility of phytoplankton growth that produces low $\delta^{15}N_{POM}$ in the nitrate-replete condition. However, the Chl a concentrations for class I were not low, instead, seem to be highest among the four classes. Also, the authors failed to explain why the ULSW may have a very low $\delta^{15}N$ of nitrate.

At last, for the discussion on the interannual variation in $\delta^{13}C_{POM}$ and $\delta^{15}N_{POM}$, what is the message the authors intended to deliver?

Yang, L., Zhang, J. and Yang, G. P., Mixing behavior, biological and photolytic degradation of dissolved organic matter in the East China Sea and the Yellow Sea, Science of the Total Environment, 2021, 143164.

Kubota, Y., Tada, R., and Kimoto, K., Changes in East Asian summer monsoon precipitation during the Holocene deduced from a freshwater flux reconstruction of the Changjiang (Yangtze River) based on the oxygen isotope mass balance in the northern East China Sea. Climate of Past, 2015, 265-281.

Liu, K. K., S. J. Kao, H. C. Hu, W. C. Chou, G. W. Hung, and C. M. Tseng (2007), Carbon isotopic composition of suspended and sinking particulate organic matter in the northern South China Sea - From production to deposition, *Deep-Sea Research Part II*, *54*(14-15), 1504-1527.

Casciotti, K. L., T. W. Trull, D. M. Glover, and D. Davies (2008), Constraints on nitrogen cycling at the subtropical North Pacific Station ALOHA from isotopic measurements of nitrate and particulate nitrogen, *Deep Sea Research Part II: Topical Studies in Oceanography*, *55*(14-15), 1661-1672.

Minor comments

Line 17: original

Line 64: $^{\delta 15N}$s?

Line 75: Sampling. I encourage the authors to add a table to summarize the sampling information.

Line 102-103: why the detection limit of nutrient was shown in a range?

Line 118: The authors did not use the international isotope standards to calibrate the data. How did they prove the precision of these data?

Line 171 and Line 185: check the significant digitals of SD. And the significant digitals of mean $\delta^{13}C$ and $\delta^{15}N$ are different.

Line 205: Explain more about the classification criterion of carbon and nitrogen isotope ratios of POM

Line 226-229: many samples had very low C/N ratio (down to 3), mainly observed at deep depth (Figure 2). Why?

Line 302: Strait

Line 303-305: I can not follow the purpose for the seasonality of estuarine $\delta^{13}C_{POM}$ mentioned here.

Line 355-356: what is the third hypothesis?

Line 375: Any evidence that supports the particles can be entrained from the ECS into the SOJ?

Line 382-383: To me, the POM pool is not fully newly produced and is not only supported by new production. So the assumed contribution of nitrogen fixation to the POM pool here, that is 10-82% (Liu et al., 2013), is too high.

Line 433-434: As mentioned, this study did not identify the main source of nitrogen in this region. The importance of anthropogenic nitrogen inputs in this region was not indicated in this study. I could agree the increasing inputs of anthropogenic nitrogen in the future, but changes in new nitrogen inputs from other sources should be considered. For instance, nitrogen fixation may be inhibited due to higher inputs of anthropogenic nitrogen. In addition, warming-induced stratification in the water column may prevent upwelled nitrate from the subsurface.