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Interactive comment on “Seasonal shifts in the contributions of the Changjiang River and the Kuroshio Current to nitrate dynamics at the continental shelf of the northern East China Sea based on a nitrate dual isotopic composition approach” by Y. Umezawa et al.

Anonymous Referee #2

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The authors presented seasonal nitrogen and oxygen isotopes of the nitrate from four transects in the Eastern China Sea in this paper. Given the novelty of the proxy itself, and its limited applications in this region, the data itself is worth of being published on Biogeosciences. The authors attempted to use the isotopes of nitrate to dissect the different nitrate sources to the shelf regions of the Eastern China Sea, and to study the different biological processes that may contribute to the changes in the isotopes. The complexities of the processes and uncertainties in the proxy itself have made this

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a very challenging paper to write and to understand. Given these considerations, I recommend acceptance for publication after minor revisions. I am listing my comments and suggestions below to hopefully reconstruct the paper for easier understanding.

My main suggestion for this paper is to isolate the physical processes and biological processes more clearly. It is challenging, but may be achievable with some simple model/calculations. For instance, can the authors use the mixing curve based on their T-S diagram to calculate the end members of the nitrate concentration and isotopic signature? And then proceed with discussions of the biological processes.

In order to achieve the above, it will be important to characterize the isotopic signatures of the water mass sources. The authors have referenced some random isotopic values, but recognizing that the values could be variable, except the Kuroshio, which seems to be well constrained. It may be better if a few more words being said about the selections of the values and the range of the observations.

In the top paragraph on page 10159, the authors seem to indicate that diatom or phytoplankton productions are limited by temperature and nitrate supply. But are there really evidences to support this?

Fig. 4 and 5 are just too busy. It seems that the symbol size on Fig. 4C has changed, but nothing has been said to explain what it means. Is this intended? There are too many depth information on the plots. I would recommend that you first remove the DCM info, because it is not really needed on the plot. You could consider adding the information on your tables. The sampling depths for the water samples may instead be indicated by the color scale or symbol size scale.

It is an interesting finding that the nitrogen and oxygen isotopes increase close to the bottom of the shelf. Although it is less likely that water column denitrification has occurred, sedimentary denitrification coupled with nitrification has been observed to cause the upper water column nitrate $\delta^{15}\text{N}$ and $\delta^{18}\text{O}$ to increase, accompanied with a loss of N (Granger et al., Coupled nitrification-denitrification in sediment of the eastern

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bering sea shelf leads to ^{15}N enrichment of fixed N in shelf waters, JGR, 116, C11006, doi:10.1029/2010JC006751, 2011). I wonder if the authors think this could explain their observation? Why and why not?

I agree that based on the current knowledge of the region and the proxy, it has a lot of uncertainties to interpret the low $d^{15}\text{N}$ and high $d^{18}\text{N}:d^{15}\text{N}$ ratio below the euphotic zone. It would be interesting to quantify the sinking flux at different depths and their $d^{15}\text{N}$ values.

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