General comments:

This is a well-written and relatively concise manuscript that uses $\delta^{15}N$ of nitrate data to trace the distribution of Pacific versus Atlantic waters in the Northwest Atlantic. This type of analysis is not new, the manuscript by Granger et al. (2018) previously laid the groundwork. However, while the focus of these two papers is similar, the current manuscript presents new relationships that estimate the fraction of Pacific water (based on both N* and the $\delta^{15}N$ of nitrate) using a more extensive dataset and discuss some possible applications (food-web studies, paleoceanographic reconstructions, etc…). I have some minor comments to improve the manuscript. First, it would be best to separate the results from the discussion to improve clarity and focus, if at all possible. Most of the text before section 3.4 could be moved to a Results section, as these sections are mostly descriptive, the remainder could be re-organized into a proper discussion section. Second, some analytical detail or background information in the discussion needs to be added (see specific comments below). Finally, I wonder if the $\delta^{18}O$ data could be explored in more detail. These data are shown in Figure 3, but poorly discussed in the manuscript.

Specific comments:

Materials and methods:

Lines 96-107: Is using different types of filters (0.45 μm versus 0.22 μm) affect nutrient concentrations? Was this tested?

Lines 121-122: Was USGS 32 used to correct $\delta^{15}N$ data? Since its $\delta^{15}N$ is much different from the $\delta^{15}N$ of the samples, I assume this would be problematic.

Lines 123-125: Why was NO_2^- not removed? Even small NO_2^- concentrations can affect the $\delta^{15}N$ and $\delta^{18}O$ of NO_3^-, especially at low NO_3^- concentrations. What was the lowest NO_3^- concentration for samples analyzed for isotopic composition? Since nitrate concentrations are generally high below the mixed layer, and for most water masses discussed in this manuscript, I don't think this is a major concern.
Another concern is that the "denitrifier" method is sensitive (to some extent) to the $\delta^{18}O$ value of the sample water because of O exchange with water during the conversion of NO_3^- to N_2O. This is an issue if the $\delta^{18}O$ of the samples and standards are drastically different or in polar regions where the $\delta^{18}O$ of water is greatly variable due to mixing between freshwater from rivers and glaciers (with a low $\delta^{18}O$ around -20 ‰) and seawater ($\delta^{18}O$ of about 0‰). Was this taken into account while analyzing their samples? See Kobayashi et al. (2021) for more detail. Finally, what was their blank size (i.e., for the bacteria method)?

Lines 144-145: The authors should make a clearer distinction between regenerated (calculated using Redfield P:O_2 ratio and AOU) versus preformed PO_4^{3-} here. Preformed nutrients are those

that were present in solution when the parcel of water sank from the surface and are characteristic of different water masses:
pre-formed [PO_4^{3-}] = measured [PO_4^{3-}] + regenerated [PO_4^{3-}]

Lines 160-161: Explain what cause that kink in the NO_3^- vs PO_4^{3-} relationship at low nutrient concentrations (i.e., nitrate assimilation).

Results and Discussion:

Lines 367-371: I am curious about the isotope effect for nitrate assimilation derived from these relationships and how it compares to previous field studies (e.g., Altabet et al. (2001)?

Lines 415-416: Why isn't the correlation showed for $\delta^{18}O$-NO_3^-? Could $\delta^{18}O$ of NO_3^- also be used as a complementary tool to trace these different water masses? This aspect should be better discussed.

Lines 427-429: The authors should explain why no change in water-column $\delta^{15}N$-NO_3^- is expected during sedimentary denitrification (i.e., discuss the suppressed net "community" isotope effect for sedimentary denitrification due to diffusion limitation and complete NO_3^- consumption in the sediments).

Line 468: Could a similar equation be derived for $\delta^{18}O$ of NO_3^- as well? However, $\delta^{18}O$ of NO_3^- would not be useful for food-web or paleoceanographic studies as the O atom is not conserved during N incorporation into organic material.

Lines 471-478: This argument needs to be discussed better since as for N*, it is not possible to disentangle different co-occurring processes (nitrification, denitrification, N_2 fixation) using $\delta^{15}N$ of NO_3^- data solely. These co-occurring processes were disentangled in the BBW because of the additional insights from $\delta^{18}O$ of NO_3^- data.

Line 483: This section title is vague. I would rename it "Using our $\delta^{15}N$-NO_3^- relationship to establish a baseline $\delta^{15}N$ for food-web and paleoceanographic studies." This section could also be merged with sections 3.7 and 3.8.

Line 565: Change to "fraction of Pacific water"

Table 1. Add number of samples analyzed for each water masses (n).
Indicating a range of depths for each water masses would be better than showing the average (given the large standard deviation).

Figure 3. Are $\delta^{18}O$ of NO_3^- values shown at about 200 m depth (177) and 500 m depth (ROV5) outliers? It is unclear why there is no corresponding increase in the $\delta^{15}N$ of NO_3^- at these stations/depths. Were these samples measured in duplicate?

Figure 5. I think it would make sense to separate the symbols based on depths for this figure (e.g., as in Figure 2: surface waters impacted by nitrate assimilation (open symbols) versus deeper waters (filled symbols)).

Figure 7. The R^2 as well as p-values should be added.

Additional references:

Altabet, M. A. (2001). Nitrogen isotopic evidence for micronutrient control of fractional NO3− utilization in the equatorial Pacific. *Limnology and oceanography*, *46*(2), 368-380.

Kobayashi, K., Fukushima, K., Onishi, Y., Nishina, K., Makabe, A., Yano, M., ... & Okabe, S. (2021). Influence of δ18O of water on measurements of δ18O of nitrite and nitrate. *Rapid Communications in Mass Spectrometry*, *35*(2), e8979.