

General comment

N₂O is one of the most important greenhouse gases, and the ocean plays an important role in the global budget. The production of this gas in the water column will be boosted at low oxygen concentration but consumed when the oxygen concentration drops to certain criteria. Therefore the water with low oxygen concentration is generally an important source for N₂O. Based on the information provided by the authors, the Baltic Sea is a good spot for N₂O biogeochemical cycle study in the low oxygen concentration, and the authors use state-of-the-art techniques such as isotopomer analysis to reveal the possible N₂O production, which will of course promote our understanding of the N₂O dynamics in this study region. Unfortunately, I think there are some major issues in this manuscript, and I think the author should make more effort to revise it before it is acceptable for the Journal. One of my biggest concerns is that the authors divide the water column into two types of water, oxic and suboxic, and the suboxic water is also divided into two groups based on whether or not there is sulfide detectable. However, I can't tell if this is a reasonable way since there is very limited information for the dissolved oxygen distribution patterns in the study section, only a section with a small figure in Figure 2a, and a station profile in Figure 2b. However, from Figure 2 and Figure 3, I suppose that there may be anoxic or oxygen-deficient layers presented in the study region. So, the concentration of oxygen should be clearly displayed, and better categorized, this will provide some very important information for the readers.

- We thank the reviewer for their constructive and helpful comments and suggestions about our paper. Following, we reply to each issue individually, and explain the changes we will make to the revised manuscript to meet the reviewer's criticism. The existing knowledge from the Baltic Sea shows chemolithoautotrophic denitrification to be an important process and the presence of H₂S is critical for this pathway to take place. Looking at the data from the suboxic depths, we observed a distinct pattern between the study sites with and without detectable sulfide levels. This is why, it made sense to group the data in this fashion. To make the dissolved oxygen (DO) profiles more prominent for the readers, we have now added a DO plot for each station in Figure 3 of the revised manuscript. The oxygen concentrations are also clearly displayed.

Secondly, I think the manuscript lacks figures of hydrographic parameters such as temperature and salinity, since the authors mentioned the hydrographic process such as MBI, and it may provide some important information such as the authors mentioned in the manuscript, like, how the oxygen "intrudes" the region. Finally, as a whole, I don't think that the authors give the readers a very clear and solid conclusion, there may be many reasons leading to this situation, including poor preparing of the figure, which in turn leads to an incomplete description of phenomena, and skill of writing, which stops the authors from well describing the

phenomenon, and so on. Hence, I think that the authors should carefully reanalysis the dataset, redraw the figures and reorganized the logic and language of this manuscript. A major revision is needed before the paper is suitable for publication by the journal.

We have added temperature and salinity data for all stations under Supporting Information.

The MBI is an important and unique aspect of the Baltic Sea biogeochemistry. It is an importance source of oxygen to the deeper basins. Many studies have documented the MBI in details . We have modified the text in the following manner: “Occasionally, the North Sea waters flow in over the sills and flush the deeper basins. These inflow events are known as MBIs (major Baltic inflow), and they bring oxygen-rich and saline waters to the deeper basins of the southern and central Baltic Sea. In the recent past, the MBIs have been occurring roughly once in a decade (Gräwe et al., 2015) although this statement has been questioned by Mohrholz (2018) who found a decadal variability of MBIs with a timescale of 25-30 years. The most recent MBI before our sampling campaign, which was also the third largest one in 60 years, occurred in December 2014 (Liblik et al., 2018; Dellwig et al., 2021). Walter et al. (2006) studied the N₂O dynamics during the MBI event of 2003. These studies have visual representations depicting the flow of the North Sea waters into the deeper basins of the Baltic Sea. Our study was during a stagnant period.”

We have modified the Conclusion section. The datasets have been carefully reanalysed, figures have been redrawn and the language has also been restructured.

Specific comment

Line 55, signature of the nitrate or nitrite... I think reference is needed here.

Added.

Bourbonnais, A., Letscher, R., Bange, H., Echevin, V., Larkum, J., Mohn, J., Yoshida, N. and Altabet, M.: N₂O production and consumption from stable isotopic and concentration data in the Peruvian coastal upwelling system, *Global Biogeochem. Cy.*, 31(4), 678-698, 2017.

Line 69 5-15nM..., if this is general range, I suggest a wilder range since the polar ocean may have higher N₂O concentration

We omitted this part of the sentence.

Line 75-81 for the first question, I feel there is inconsistent with last paragraph. In the last paragraph, it seems there are colleagues concluded that nitrification is the predominated N₂O source, whereas the authors want to answer the dominant pathways, any new insight we can obtain in this study?

Walter et al. was published in 2006 and the cruise was conducted immediately preceding a North Sea inflow event. We have mentioned this in Line 81 now. Our data is more reflective of the stagnant conditions that exist for the majority of the time.

Line 82 this Manuscript is not a methodology paper, so I think this question is put forward inappropriate here, if the authors is not confident to use the method here, they should carry out a study to estimate of effectiveness of this method in advance.

We changed the language to “How to interpret N₂O pathways using stable isotopic data, including site preference, as analytical tools?”

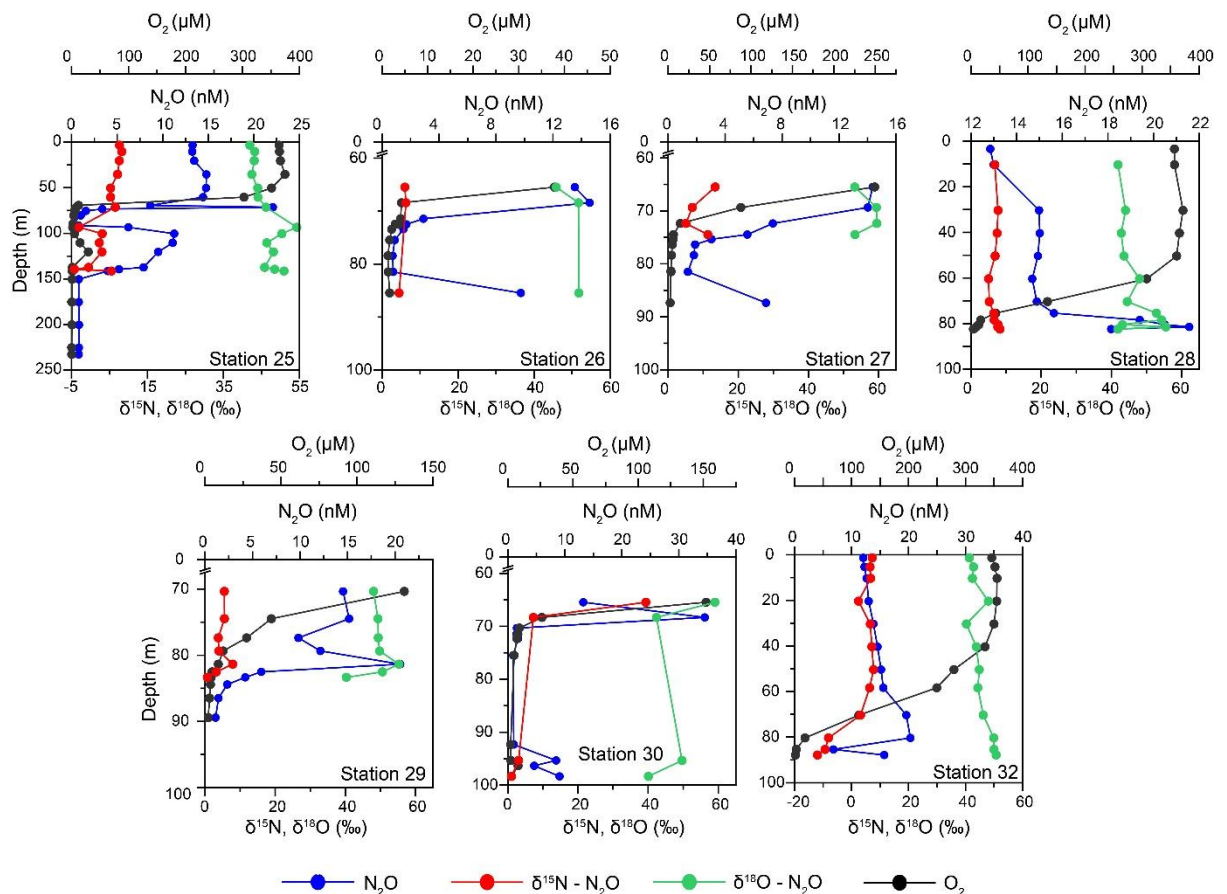
Line 130 Is this means that the sample is bubble free before capping, generally, when butyl stoppers and aluminium crimps was capped, it will easily introduce bubble, so i guess bubble free should be make sure after capping?

We ensured each vial was bubble free after capping by closing each vial under running water from the CTD.

Figure 2. as mentioned before, hydrographic figures should be provided here, and the oxygen profile of each stations should be added to figure 3

We have added temperature and salinity data of all stations under supplementary information.

Oxygen profiles of each stations have been added to Figure 3.



Line 174 O_2 between 9-2umol? Typo?

Yes. Corrected to 2-9 μM

Figure 3, the scale of x y axis of each figure should agree with each other, so it is easier for the reader to compare them.

Corrected.

Line 229 “Significant” is a word used only after statistic data analysis performed, it generally should followed with a criteria such as p value

Removed.

Line 264 I think the first sentence should be rewritten

Changed to “..the $\delta^{15}N_{bulk}$ - and $\delta^{18}O$ of N_2O in the oxic surface waters closely resembled those of trophospheric N_2O ”

Line 396 the conclusion should be rewritten, there is no solid conclusion in current format, for example, the sentence “our results demonstrated the spatial variability...” what kind of variability? Moreover, half of the paragraph is about future work, generally, it will only be one or two sentence for future work

We are referring to the variability between Gotland and Bornholm basins that we discussed.

Section 5 has been renamed to “Concluding remarks and future scope” as we think these knowledge gaps are important information.

