

Interactive comment on “Nitrogen and oxygen availabilities control water column nitrous oxide production during seasonal anoxia in the Chesapeake Bay” by Qixing Ji et al.

Anonymous Referee #4

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The paper reports an experimental study of rates and pathways of nitrous oxide production in Chesapeake Bay waters. Water was sampled on three occasions (spring, summer, autumn) and incubated with N-15 labelled nitrate or nitrite under anoxic conditions. Additional incubations were made with oxygen added back to investigate the oxygen sensitivity of the processes. Based on the results, the authors draw conclusions about the controls of N₂O emissions from the Bay.

The paper addresses an interesting subject and the experimental work is of good quality. However, the results do not provide strong support for the conclusions because the experimental conditions do not sufficiently reflect the environmental conditions in

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the Bay. Also, although there are few previous experimental studies from comparable environments, the paper largely neglects the large number of previous studies on N₂O dynamics in estuaries, although these do provide some insight to the controls of N₂O emission. Importantly, the literature points to nitrification (ammonium oxidation) as a major N₂O source in estuaries whereas the present study only investigates N₂O production through denitrification. Without data on the rates and controls of N₂O production by ammonia oxidation (i.e. experiments with N-15 labelled ammonia at different oxygen concentrations), no conclusions can be drawn about the controls of N₂O emissions from Chesapeake Bay.

Based on the mismatch between the experiments and the conclusions, I recommend that the paper be rewritten to focus on what the experiments can actually tell us, i.e. how N₂O production during denitrification is affected by oxygen, and how production from nitrate and nitrite seem to function independently, which is novel and interesting. I also warn against trying to translate the results into understanding denitrification as a N₂O source in the Bay as a whole, and the role of anoxia in this, because denitrification is an interface process there, and the anoxic water body might serve as net sink for N₂O, drawing it down from the overlying oxycline. Here, fine scale profiling of N₂O across the interface might be more informative than the experimental approach.

Specific comments 3, 3: Why pilot? – to me this indicates preliminary results

7, 10: The detection limit for H₂S by smell is $\sim 10 \mu\text{M}$. It seems strange if no sulphide was present at all, if all more favourable oxidants were depleted.

9, 5: I would leave out this back-of-the-envelope estimate of denitrification. It does not add new, robust insight to N loss in C. B.

9, 23 and onwards: This is an important finding, which requires elaboration. I suggest calculating the direct contribution from nitrate to N₂O for all the different combinations of nitrate and nitrite concentrations instead of just one example. If rates are assumed to be constant during the incubation, a simple model can describe the concomitant

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production and consumption of nitrite and hence how N-15 should accumulate in the extracellular nitrite pool if the intermediate nitrite were exchanging freely.

10, 9: I don't understand this formula. How can the amount of ^{15}N -nitrite produced depend on either the total nitrate and the total nitrite concentration? Shouldn't it simply be: Rate of NO_2^- production from $\text{NO}_3^- \times$ incubation time \times initial fraction labelled of NO_3^- ?

12, 1: don't understand this. Oxygenation will just shift the zone of denitrification and N_2O consumption to greater depth (until it reaches the sediment). It won't necessarily inhibit it.

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