Response to Reviewer

Italic: reviewer's comments
Underline: Authors' response

The authors refute my major concerns about the paper. They have, however, made changes in response to the other reviewers' comments that partly resolve these issues.

My major concern was that while the study only examines one pathway of N2O production, denitrification, and pathways associated to nitrification are ignored, the text in several places seems to deal N2O production in general, and not just one of the pathways (e.g., abstract l. 11, 12, 16, 18, 20; l. 273). The authors in their response argue that nitrification is not important during anoxia, which is obvious, but it is not obvious to the reader of the abstract that only (or mainly) anoxic experiments were conducted and that only denitrifying pathways were quantified. This must be made clear from the beginning, e.g., l. 11 should be changed to "…were used to investigate the geochemical factors controlling N2O production FROM DENITRIFICATION in the Chesapeake Bay", and so on for the other places mentioned above.

We made revisions in line 8 – 9, 10 – 12 in the abstract, and line 274 – 275 in discussion section, plus the statement in line 59 – 61 to emphasize that the main focus of this pilot study is quantifying the N_2O production rates from denitrification and associated geochemical factors. We acknowledged in line 313 – 320 that, nitrification is also another important production pathway that awaits further research effort.

The authors also refute my comment about the detection for H2S by smell being ~10 µM. They do so, however, by referring to a naïve calculation, which has little to do with the context in which (I guess) the olfactory assay was conducted (on deck, in turbulent air, placing a nose above a bottle or stream of water, and not by equilibrating a large volume of water and inhaling the equilibrated air purely – if I'm wrong this needs to be specified). Thresholds in the low µM range can be found as a rule of thumb in the scientific literature and on the www – I leave the search to the authors. The point here is, however, that the conclusion that H2S was absent is not justified.

We revised the statement in line 169 – 170 to adopt the reviewer's suggestion: "The water samples were free of any hydrogen sulphide odor, so we conclude that sulphide was either absent or was present at very low level (< 1 µmol L^{-1})."

The first version contained a very rough calculation of nitrogen removal by denitrification based on the N2O production rates. In their response, the authors say that it will be removed. Now I find it in the conclusions (l. 300-9). This is confusing.

We consider this evaluation useful in that N_2O production via denitrification could indicate the effectiveness of nitrogen removal by the sediment-water system. Thus nitrogen removal in estuaries comes with a small cost: emitting N_2O as a by-product. This also points out an interesting research characterizing a potential negative feedback: "increase nitrogen loading → deoxygenation → nitrogen removal → decrease nitrogen loading". We revised the statement in line 310 – 312 to preserve our intent.