Line 1: ‘Presumptive nitrification’ is not clear enough to the audience. I would suggest using ‘nitrite production’.

Changed to “Nitrite regeneration in the oligotrophic Atlantic Ocean”

Lines 419-430: The conclusion is made on the assumption that NO2 - release during assimilative NO3 - reduction is negligible in NO3 - depleted water. i.e., the upper mixed layer of the oligotrophic ocean. However, NO2 - production via NO3 - reduction has been measured above the nitracline. The rate is higher than NH4 + oxidation in both the California Current (Santoro et al., 2013) and the North Pacific Subtropical Gyre (Wan et al., 2021), suggesting a considerable fraction of NO2 - , is contributed by phytoplankton even in the nutrient-depleted water. On the other hand, NH4 + oxidation is frequently to be found at a rate ‘below the detection limit’ using 15NH4 + labelling incubation at the surface ocean (i.e., Horak et al., 2013; Santoro et al., 2013; Shiozaki et al., 2016), demonstrating extreme low activity of marine AOO in the surface layer of the oligotrophic ocean. These results indicate that at least a certain fraction of NO2 - is contributed by phytoplankton in the mixed layer.

*In our defence here the paper by Santoro et al returns only one observation of NO_2 release from NO_3 in fully oligotrophic conditions. Their observation in nutrient replete conditions was reported as equivalent to a filtered control and thus there is some doubt cast over that result. The Wan et al paper was published after the submission of the previous manuscript and we did not have foresight of this data. We have now reconsidered this section and have addressed the reviewers concerns accordingly. (**Now lines 420 - 431**)*

Lines 431-453: Accumulating evidence demonstrates that NH4 + oxidation is the main source of NO2 - at the lower euphotic zone (i.e., the primary mechanism that sustains the PNM). The contribution of NH4 + oxidation to PNM ranged from ~70% to ~90% in different studies (i.e. Buchwald and Casciotti, 2013; Chen et al., 2021; Santoro et al., 2013; Wan et al., 2021). It’s better to review the literature to provide a more comprehensive statement on the contribution of NO3 - reduction to NO2 - at the PNM layer. And again, the NO2 - release during assimilative NO3 - reduction is not negligible.

*We have now addressed the contribution of ammonium oxidation to the PNM and clarified a statement on the contribution of nitrate reduction. We had not stated in this section that the phytoplankton contribution is negligible. (**Now lines 436 - 444**)*

Lines 454-473: I agree that at a depth of 0.1% of PAR, NO2 - production should be predominated by NH4 + oxidation as the growth of phytoplankton is limited by the dim light. However, the statement that ‘The fact that such elevated NO3- concentrations persist at this depth (an average of 8.2±7.1 μmol L-1 was measured) implied that NO3- was not an important N-source for photosynthetic cells.’ is not justified. The high NO3 - concentration at the subsurface water indicates that NO3- supply rate is higher than NO3- assimilation rate due to the light limitation, it cannot tell the nutrient structure (i.e. NO3- vs. NH4+ or DON) by the phytoplankton.

*On reflection we agree with the reviewers argument here, though do not understand the very last point " it cannot tell the nutrient structure (i.e. NO3- vs. NH4+ or DON) by the phytoplankton". This section has been edited in-line with the reviewers comments. (**Now lines 456 - 471**)*

Lines 486-493: Inhibition of marine nitrifiers by light has been well demonstrated in numerous studies, and the rate measured in the present study (1.2±1.9 nmol/d) appears to be lower than the rate collected from 24h incubation (2.9±2.4 nmol/d). I agree with the statement that 'Results presented here may represent a lower limit for RNO2', but not for the idea that 'the exclusion of a dark phase to the incubations used here had no significant impact on average values between studies'.

*This section has amended according to the reviewers comments. (**Now lines 484 - 494**)*