

Interactive comment on “Biological productivity in the Mauritanian upwelling estimated with a triple gas approach” by T. Steinhoff et al.

Reply to anonymous Referee #2

We are grateful for your comments to our manuscript. Below are our answers (in italic) to the comments:

This paper describes a clever way of deriving net community production (NCP) using measured mixed layer concentrations of N₂O, O₂ and CO₂. This seems to me to be a basically sound approach and is appropriate for publication in Biogeosciences. In agreement with the exhaustive comments of a previous anonymous reviewer I believe that the major issues to address are those relating to data and calculation uncertainties and I endorse those comments fully. As such there is no point reproducing essentially many similar comments here. Rather, I highlight some additional comments to which the authors might wish to respond, as follows:

The authors assume that nitrification is a negligible N₂O source based on previous evidence for photo-inhibition of nitrification from Horrigan et al. (1981) and indeed C2189 other earlier work supports this. However, the potential for euphotic zone nitrification is long known (e.g. Ward, B.B. (1987) Nitrogen transformations in the Southern California Bight, Deep-Sea Res. 34 (1987), 785–805) and although the conclusion of the authors with regard to nitrification N₂O might well be correct, a little more justification for their conclusion in the light of this would be informative.

-

The authors refer to both “transfer velocity” and “transfer coefficient”. Consistency is needed here.

- *Done.*

I presume that the wind speed data derived from the ship weather stations were automatically corrected for ship motion and corrected to U₁₀, the value 10m above the sea surface but this is not stated. How was the correction applied and was there any correction to neutral stability or not? What was the accuracy and precision of the wind speed estimates?

- The ships weather stations are run by the “German Weather Service” and follow the recommendations of the “World Meteorological Organisation (WMO)”.

Two wind speed parameterisations were used to derive k. Although it is stated that the Tsai and Liu (2003) parameterisation accounts for surfactant, what was the justification for not using some other available parameterisations?

- *The idea was using a classical parameterization and one that accounts for reduced ASE. To demonstrate the huge differences between a classical and adjusted parameterization we could use any other parameterization as the one of Wanninkhof (1992) but this wouldn't change the conclusion of the manuscript.*

How variable was the upwelling index during the cruises? The authors state that they used the mean values for each cruise but this gives no indication of variability.

- *Added standard deviation.*

The term ASE (air-sea exchange) is first used in line 4, p 4859 but it is not previously defined.

- *Changed.*