

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

### **Anonymous Referee #2**

Received and published: 29 June 2012

This paper describes a clever way of deriving net community production (NCP) using measured mixed layer concentrations of N<sub>2</sub>O, O<sub>2</sub> and CO<sub>2</sub>. 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 N<sub>2</sub>O source based on previous evidence for photo-inhibition of nitrification from Horrigan et al. (1981) and indeed

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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 N<sub>2</sub>O 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.

I presume that the wind speed data derived from the ship weather stations were automatically corrected for ship motion and corrected to U<sub>10</sub>, 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?

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?

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.

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

---

Interactive comment on *Biogeosciences Discuss.*, 9, 4853, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)