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Biogeosciences Discussions

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

Anonymous Referee #1

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General Comments The authors present a novel approach for calculating net community production (NCP) in eastern boundary upwelling systems using data from the Mauritanian Upwelling as a case study. Their calculations rely on a triple gas approach (N2O; CO2 and O2), though O2 is primarily used to validate the results derived for CO2. Briefly, N2O is used as a quasi-inert tracer of recently upwelled water to calculate the time since upwelling (τ). This is used to assign τi values to concurrent DIC data. The latter are corrected for sea-air gas exchange over τi+1-τi and the resulting value is subtracted from the observed DIC for τi+1 to derive NCP. The authors show that NCP from three cruises is strongly correlated with the upwelling index (UI) derived from model predictions. This could be a particularly attractive and cost-effective way of quantifying NCP under different upwelling conditions. The paper addresses pertinent scientific questions and is within the scope of BG. To my knowledge, this is a novel

and, in my opinion, elegant approach, which leads to important findings. The paper is clearly presented with appropriate equations, tables and figures. However, the authors have not discussed or propagated critical uncertainties in their calculations. These uncertainties could have a large impact on the magnitude of NCP and bring the predictive utility of the NCP-UI relationship into question. Nevertheless, I like the concept and would like to see the paper in BG, once the authors have addressed my concerns. My main concern is that the authors have not attempted to propagate uncertainty from their various data sources. Although calculated NCP carries error bars, the source of this uncertainty is not given. Firstly, the authors do not discuss uncertainty over the value of the mixed layer depth (MLD) which is derived from a model rather than in situ data (see also comments 3 and 14 below). A reference to the model skill is given (Steinhoff et al., 2010), but this publication did not examine model skill in highly dynamic upwelling systems where it may not perform as well. Steinhoff et al. (2010) found a difference of 24 m between model MLD and observations which is of the same magnitude or greater than the MLD calculated for the Mauritanian upwelling. Furthermore, the model itself has a resolution of 1/6 o which may be insufficient for dynamic upwelling systems. Secondly, a potential source of uncertainty is the assignment of τ values to DIC samples where there are no concurrent N2O data (see also comment 10 below). The authors use a fit for DIC against τ for this purpose. This appears to be some exponential asymptotic decrease, though the function is not given. According to this fit a DIC sample around 2150 μmol/kg would have a τ value around 8 days. Yet the data in Figure 3B show that τ for such DIC could be anything between 5 and 14 days. The situation is worse as one approaches the limit of the function. What proportion of DIC samples did not have concurrent N2O? Did the authors use any criteria other than the fit to assign τ to DIC? Thirdly, I am concerned about the use of average TA for the calculation of DIC (see also comment 6 below). TA variability translates into DIC uncertainty of $\pm$9 μmol/kg. Yet the threshold for the biological drawdown of DIC is set at 2 μmol/kg, i.e. at undetectable DIC change. I'd like the authors to clarify these uncertainties and propagate them through to their NCP calculation. Specific comments

follow below.

Specific Comments 1. P4856; Line 15-16: '..accuracy...' for what? I presume the authors mean the CO2 concentration of their standards. Please specify. 2. P4857; Line 10: Please give original reference for nitrate analysis: Brewer, P.G., Riley, J.P., 1965. The automatic determination of nitrate in sea water. Deep-Sea Research 12, 765-772. 3. P4857; Line 18-22: Steinhoff et al. (2010) did not validate the Mercator model in upwelling systems. The 1/6 o resolution may be too coarse for highly dynamic systems such as upwellings. pCO2 and O2 in Figure 2 shows variability at much shorter length scales than 1/6 o. Is this critical? Why not use in situ data to validate the Mercator model and estimate its uncertainty for the Mauritanian upwelling? 4. P4859; Line 1-3: Please also refer to Rees, A.P., Brown, I.J., Clark, D.R., Torres, R., 2011. The Lagrangian progression of nitrous oxide within filaments formed in the Mauritanian upwelling. Geophysical Research Letters 38, L21606, 21610.21029/22011gl049322. 5. P4859; Line 4: Please define the abbreviation ASE. I presume this refers to Air Sea Exchange (?). 6. P4859; Line 19: TA variability from Table 2 ($\pm$11 μmol/kg) translates into DIC variability around $\pm$9 μmol/kg for DIC in CO2SYS (pCO2=450ppm;S=35,T=15). This uncertainty over DIC is $\times$4.5 larger than the DICbio threshold of 2 μmol/kg used to detect the end of upwelling mediated productivity (P4862; line 1). This implies that the authors can't actually detect this threshold with any certainty. 7. P4860; Line 9: Please specify where the 'red marker' is. I'm guessing Figure 3. 8. P4860; Line 9-10: How was the location of highest N2Osw confirmed to be in core upwelling? Remote sensing? Climatology? SST? Geographic location? 9. P4860; Line 22-23: What type of function was fitted to the data? Please give equation in brackets. 10. P4861; Line 3-4: How did τ from the fit match up with τ from concurrent N2O? From Figure 3B, it looks like a sample with DIC around 2150 μmol/kg could have a value of τ between 5 and 14 days measurements and 8 days from the fit. At 2125 μmol/kg, τ from the fit could be anything between 12 and 36 days! Did the authors use any other criteria to assign τ to DIC samples where there were no concurrent N2O measurements? For example, manual inspection - proximity to nearest N2O, interpolation

between neighbouring DIC values. I think it's necessary to quantify and propagate these errors through to NCP. 11. P4861; Line 6-8: I presume the inventories I refer to the mixed layer. Please specify. 12. P4862; Line 1: Why did the authors use a threshold of 2 μmol/kg? What does it represent? Analytical uncertainty is much greater (see comment 6 above). 13. P4862; Line 9-10: What does the NCP uncertainty in Table 3 represent? How was it calculated? 14. P4862; Line 12: Perhaps the calculations do depend critically on the parameterization of k, but there are other potential errors which have not been propagated or discussed (see comments 3, 6 and 10 above). For example, the Lohrbacher-Mercator model mismatch of -24 m in Steinhoff et al. (2010) is of the same magnitude or larger than average MLD in Table 2 for the 3 cruises. I would guess that this will affect the calculations much more than the uncertainty over k.

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