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

Anonymous Referee #3

Received and published: 9 July 2012

This study presents a method for isolating the effects of air-sea exchange and NCP from dissolved gas budgets using observations of three gas saturations (N₂O, CO₂, and O₂). N₂O is assumed to be inert, and is used to set a timescale for evolution of gas saturations post-upwelling. NCP is calculated from changes in CO₂ inventory, and the relationship between NCP (O₂)/NCP(CO₂) is used to constrain the air-sea transfer rate using published ranges of stoichiometric O₂:CO₂ quotients for biological productivity. The approach employed here appears sound and represents a valuable contribution to literature on productivity rates in EBUE. As such, I would recommend this manuscript be published after consideration of the comments supplied here and by previous reviewers. As with the other reviewers, my general comments stem from a desire to understand the sensitivity of the calculated NCP to the choices made in the analysis; at present this is the primary weakness of the paper, but this is also something

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that could be easily remedied.

General comments:

1. Regarding MLD – in my experience, standard MLD criterion (temperature- or density- based) do not work so well in coastal regions, particularly in areas of recent upwelling. Often chemoclines or gas profiles provide a better sense of the mixed layer, and as this study utilizes gas saturations, validating MLD estimates with these data would also be particularly relevant for this analysis. Were profiles of O₂ or DIC/TA ever checked against model-predicted MLD? Also, it seems that a constant MLD value was used to calculate inventories of N₂O and CO₂/DIC for nearshore and offshore observations. Was this characteristic of the offshore transects, and would the NCP calculated be sensitive to a calculation that includes changes in MLD from nearshore to offshore?
2. Regarding gas transfer coefficients: similar to the above, was the time variability of air-sea exchange ever considered before being neglected in favor of a mean value? For CO₂, which has a slow exchange timescale, a mean value would be fine, but for something like O₂ which can turn over on a much shorter timescale (i.e. days for shallow mixed layers like those observed here), it strikes me that results might be sensitive to this simplification. As this is the primary data set being used to constrain the appropriate wind speed relationship to use (via comparison of O₂ and CO₂ derived estimates of NCP), this might warrant a bit more explanation.
3. There was some mention of propagation of errors, but yet little discussion other than what I presume is an error reported with NCP in table 3. Can this analysis be described in a bit more detail and the major sources of uncertainty be identified for the reader? Also, can Table 3 caption be updated to make clear how NCP uncertainty was calculated?
4. Were the transect data and associated τ for observations ever compared to mean offshore advection rates (e.g. calculated from upwelling indexes and an Ekman depth)? This would provide an additional check on the method and calculations of NCP.

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Specific (minor) comments:

4858, line 27: Can you give a brief statement as to why turbulent transport can be neglected for the N2O budget? Are gradients below the mixed layer trivial?

4859, line4: ASE – define at first use

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

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