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

Reply to comments from Referee #4 (Jan Kaiser)

We are grateful for your comments to our manuscript. As the comments and the proposed calculation scheme has substantially changed and improved the manuscript, Jan Kaiser became a co-author of the revised manuscript.

Below follow our author’s comments to Kaiser's suggestions:

Comment to the part “Direct calculation method in an upwelling system”

We implemented Kaiser’s approach of the direct calculation of NCP. We opted not to use the combined equation 3, but used the equations 1 and 2. The advantage was that equation 2 could be used in the same form for the change of dissolved inorganic carbon (DIC) and oxygen with time. This enabled us to directly compare the NCP estimates based on both parameters, oxygen and DIC. It looks like the knowledge of mixed layer depth (MLD) is not needed using equation 3 of Kaiser, but since time information is needed anyway, MLD is needed in equation 4 or 8.
We also renamed some variables for better understanding.
Furthermore we added a “Supplementary Material” where all data and calculations for all cruises are shown. Fig. 2 in the manuscript is only for illustration of the found concentration patterns.

Comment to the part “Temporal versus spatial mapping”

With the new approach we could calculate NCP values for each data point. Thus a regional average is not used anymore. For a reliable regional estimate more data points are needed.

Comment to the part “Gas Exchange parameterization”

We agree with the argumentation of Kaiser. Fig. 4 of the revised manuscript shows the oxygen increase during the first days that was used to establish a relation (DO_2/dt). We discussed the influence of diel cycles on the estimation, what makes it more difficult to use the oxygen data. Diel cycles and measurement uncertainties determine the robustness of these fits. For the cruises M68-3 and P399 the higher uncertainty and diel cycles combine to scattered data that can’t be seriously fitted.
We clearly discussed that surfactants MAY be the reason for the reduced air-sea gas exchange (ASE) and that this is still speculative. However, surfactants stay stable and can suppress ASE up to wind speeds of 11 m/s (Cunliffe et al. (2013) and references herein). Only 1.5% of the dataset has wind speed higher than 11 m/s.
The parameterization of the transfer coefficient *k* was done following Wanninkhof (1992) as one of the classical parameterizations contrasting the parameterization of (Tsai & Liu, 2003). It was not the scope of this manuscript to discuss the different parameterizations. We only wanted to show the difference between a classical parameterization and one that takes into account surfactants.
Furthermore discussing the Schmidt numbers is far beyond the scope of this manuscript.

We used an oxygen saturation concentration of 102%. In the revised manuscript this number is stated.

References

Cunliffe, M., Engel, A., Frka, S., Gašparović, B., Guitart, C., Murrell, J. C., Salter, M., et al. (2013). Sea surface microlayers: A unified physicochemical and biological perspective of the air–ocean interface. *Progress in Oceanography*, *109*, 104–116. doi:10.1016/j.pocean.2012.08.004

Tsai, W., & Liu, K. (2003). An assessment of the effect of sea surface surfactant on global atmosphere-ocean CO2 flux. *J. Geophys. Res.*, *108*(C4), 1–16. doi:10.1029/2000JC000740

Wanninkhof, R. (1992). Relationship between wind speed and gas exchange over the ocean. *J. Geophys. Res.*, *97*(C5), 7373–7382.