

## ***Interactive comment on “Origin and fate of the secondary nitrite maximum in the Arabian Sea” by P. Lam et al.***

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Thank you very much for your constructive comments. We will take them in due consideration in preparing our revised manuscript when we are given the opportunity.

Meanwhile, please find below some detailed point-to-point responses to your comments:

Page 2359 last 2 lines: What we intended to say was that in light of the secondary nitrite maximum (SNM) being coincident with low-oxygen conditions, the finding of SNMs in the oceans have been considered to be associated with the occurrence of denitrification. Hence, most subsequent oceanic water-column denitrification studies in the past decades have been targeted almost exclusively to these nitrite-laden waters of

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the OMZs, and results from these point-measurements of N-loss have been the basis of extrapolation to some global-scaled estimates. We will clarify this in the revised manuscript.

Page 2360: Yes, what we meant by ‘surface biogeochemistry’ was the cycling of carbon and nitrogen, etc. We will use the term ‘surface biogeochemical cycling’ instead for clarification.

Page 2362: The formula for  $N^*$  is essentially the same as originally defined by Gruber and Sarmiento (1997), except that we took into account the concentrations of total inorganic nitrogen instead of nitrate only. The constant  $2.90 \mu\text{mol kg}^{-1}$ , also used by Gruber and Sarmiento (1997), was to bring the global mean of  $N^*$  to zero for global comparison. Since our results were presented in  $\mu\text{mol L}^{-1}$  instead of  $\mu\text{mol kg}^{-1}$  in the original formula, the density term was included in our calculations for unit conversion. The sampling and analysis protocols for  $N_2O$  were detailed in Walter et al. (2006) as referenced. Briefly, samples were transferred from Niskin bottles into 24 ml glass vials without air bubbles, poisoned with  $\text{HgCl}_2$  and sealed. Then a 10-ml helium headspace was created within each vial and the samples were allowed to equilibrate for at least 2 hours at room temperatures. Afterwards, a 9-ml headspace sample was used to flush a 2-ml sample loop after passing through a moisture trap. Gas chromatographic separation was performed at  $190^\circ\text{C}$  on a packed molecular sieve column, and the  $N_2O$  was detected with an electron capture detector. These measurements were done on shipboard. We will add some brief description on  $N_2O$  measurements as requested.

Page 2362 section 2.2: The work published in Jensen et al (2011) examines the pathways and rate distribution of  $N_2$ -production in the Arabian Sea, which includes the results of anammox, denitrification and DNRA, along with the relevant gene expression analyses. In the current paper, we further investigate other co-occurring N-cycling processes in the same set of samples and conducted a number of additional analyses (e.g. additional molecular analyses,  $N_2O$  profiling, flux modelling for various inorganic nitrogen species, comparison with long-term Seawifs and Argos data) to seek a better

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understanding of the nitrite balance and its relationship with N-loss distribution in the Arabian Sea OMZ that was reported in Jensen et al (2011). We will clarify this further in the manuscript revision, as well as with respect to the detection limits and the procedures for oxygen sensitivity experiments.

Page 2362 bottom: Thanks for pointing this out. We will provide more details for our incubation experiments and rate calculation in our revised manuscript. Briefly, water samples collected from Niskin bottles were transferred to 250 ml serum bottles, purged with helium and  $^{15}\text{N}$ -labeled substrates were added ( $^{15}\text{N}$ -ammonium plus  $^{14}\text{N}$ -nitrite for ammonia oxidation, and  $^{15}\text{N}$ -nitrate for nitrate reduction). Samples were then transferred to 12-ml exetainer glass vials, and incubated for approximately 0, 6, 12, 24 and 48 hours in the dark and at in situ temperatures. Incubations were terminated with the addition of saturated mercuric chloride – as described for the investigation of  $\text{N}_2$ -production experiments in Jensen et al. (2011). After  $\text{N}_2$  in the exetainers have been analysed for their isotopic composition, a 5-ml aliquot of each liquid subsamples was analysed for the production of  $^{15}\text{N}$ -nitrite, after acidic azide conversion to  $^{15}\text{N}$ -nitrous oxide and subsequent analyses on a GC-IRMS. This  $^{15}\text{N}$ -production was then plotted against incubation times, and the slope of the linear regression was used for rate calculations, and only slopes significantly different from zero are considered. The subsequent net production rates, as reported, are then further corrected for  $^{15}\text{N}\%$  in the initial substrate pools. More details will be provided in the revised manuscript.

Page 2364: This is actually the model assumption, a prerequisite condition for the reaction-diffusion model to be applied. We will be more explicit on this in the revised version. We agree that there has to be some degrees of advection, as we also mentioned in the Results & Discussion. However, as you have kindly pointed out and we have also mentioned later in the section 3.4, the fact that the modelled results agree well with our experimental data also strongly suggest that horizontal advective fluxes were not very large in the central-NE Arabian Sea OMZ, at least at the time of our sampling. Besides, the fact that we could hardly measure N-loss rates in the central-NE

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OMZ during the cruise, would only fit scenarios 2 and 3 suggested in section 3.4 in explaining the strong N-deficits (page 2372) – i.e. episodically high or generally low N-loss activity in a water mass of long residence time – as you also agree in your later comments. Nevertheless, this water would have to be ventilated eventually in a long run. This model was not applicable to stations near the Oman Shelf, however, exactly because of the apparently strong advective fluxes towards the western boundary of the basin – e.g. upwelling, lateral intrusion of Persian Gulf Water at mid-depths.

Page 2365 line 5: 'Isonutrals' refer to surfaces of equal neutral density. Water parcels can move along a neutral surface without doing work against gravity and without being statically unstable with respect to the surrounding water at its new position. In other words, water masses preferentially move and mix along neutral surfaces. Iso-surfaces of potential density (the so-called 'isopycnals') are commonly used. However, isopycnals have to be calculated from a reference pressure ( $\sigma_\theta$ ,  $\sigma_2$ ,  $\sigma_3$ ,... are referenced to 0 dbar, 2000 dbar, 3000 dbar) and thermobaricity introduces an error when the pressures of the water parcels deviate from the reference pressures. In the Indian Ocean, differences between the vertical location of isopycnals and isoneutrals can be substantial (You and McDougall, 1990, JGR, 95 (C8): 13,235-13,261). Hence, we prefer the use of isoneutrals as these surfaces make better physical sense. We will add some brief explanations in the revised manuscript.

Page 2365 lines 22: Because N-loss pathways and rate distribution are the subjects of study by Jensen et al. (2011), we are not repeating the same comparison and discussions here. We only state the magnitude and distribution of N-loss observed during the same cruise in the current paper, as a basis for discussion on its relationship to nitrite balance.

Page 2365 lines 26-27: As you have pointed out, the definition of 'suboxic' is so spurious amongst researchers that instead of defining it, we are considering to rephrase this sentence to "... a region generally considered unimportant for N-loss due to the previous detection of oxygen".

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Page 2366 3.2.1 and 3.2.2: Nitrate reduction and ammonia oxidation. Thank you for pointing this out. Indeed more comparison could be made between our findings and the observations made by Nicholls et al (2007), in which rates of the net loss of nitrate, as well as that of net nitrite accumulation, were determined by changes in concentrations. The latter would also include some effects of ammonia oxidation in the same samples, though ammonia oxidation rates were not directly assessed. In our study, we used two different but parallel sets of 15N-incubation experiments to directly and individually determine nitrate reduction rates and ammonia oxidation rates. While our measured nitrate reduction rates were somewhat lower than what you determined by concentration differences, the relative contributions of the two processes to nitrite production appear similar. There are also more similar conclusions drawn, which we will take in consideration in the revised manuscript.

Page 2368 3.3.1: We originally intended to briefly describe the N-loss distribution in 3.1, and emphasize on the nitrite reduction aspects here, but we also notice now that it might be repetitive. We will change it as suggested. Regarding our comments on the study by Bulow et al (2010), we are only stating what could be observed objectively, but we also do not eliminate the possibility of the occurrence of denitrification at other times, as we have also stated later in section 3.4 and further discussed in Jensen et al. (2011) which focussed on N<sub>2</sub>-production pathways.

Page 2368 3.3.2: Indeed more comparison could be made regarding N<sub>2</sub>O production with Nicholls et al (2007), in which you measured the production from different 15N-labeled substrates. We performed similar experiments, but unfortunately most samples were lost. Consistent with your findings, we detected active expression of nitric oxide reductase genes (which encodes the enzyme responsible for N<sub>2</sub>O production) where you detected N<sub>2</sub>O production, and rates estimated from our reactive-diffusive models were within the same order of magnitude at those depths. We will discuss these in greater details in the revised manuscript.

Page 2372: What we intended to point out is that the surface productivity at the central-C1101

NE Arabian Sea seems to be typical at the time of our sampling based on those 10-year records. If N-cycling processes in the underlying OMZ are indeed directly related to surface production as we postulate, then it would imply that our data on N-cycling processes would not be far from the norm. However, this implication only applies if our assumption is correct, so we would rather not to directly claim our data set to be 'typical'.

For the remaining suggestions of minor edits, we would change them accordingly. Thank you very much also for the positive comments.

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