

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

M. Trimmer

m.trimmer@qmul.ac.uk

Received and published: 2 May 2011

This is an interesting paper which finishes up posing some interesting questions about the ‘residence’ and processing time of nitrite in the oxygen poor waters of the Arabian Sea. My comments are mainly minor and refer mostly to a lack of detail in the methods and the structuring of the results section, which I found confusing in parts. This confusion comes mainly from the authors refereeing to a previous paper by Jensen et al 2011 and the distinction between the current and previous study is not always clear. In addition, they need to refer more explicitly to other work in the central Arabian Sea, some of which came to very similar conclusions about some of the aspects of the work reported here.

Page 2359. Introduction. The last two lines of this paragraph, can you put this material

C773

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in the context of the ocean?

Page 2360. Minor point but what is the difference between surface biogeochemistry and surface biological production. Cycling of carbon and nitrogen?

Page 2362. The formula for calculating the N deficit looks different to previous publications, please clarify. Also you say nothing about how you sampled for gases? Or how you measured oxygen or even very briefly how you measured N₂O. As you mention how oxygen was measured at the start of Results and Discussion, put something in the methods.

Page 2362. 2.2 I think it is very important that you clarify the links to previous Jensen paper as lines 10-15 are not clear. Also, I would say below the limit of detection rather than non-detectable and concentration rather than levels. Lines 17 – 20 give an example rather than various amounts. It is all sounding a bit descriptive rather than scientifically factual.

Page 2362 bottom. All rates presented were obtained in the linear production phase of our incubation experiments, and only those with significant increase without an initial lag-phase are considered (slope significantly different from zero, t-tests with $p < 0.05$). These are net production rates that have been corrected for the mole fractions of ¹⁵N-substrate additions, but not for any simultaneous consumption or dilution via potentially coupling processes. It is difficult for the reader who wasn't there to know what you mean by this. So far you haven't said anything about timed incubations! What corrections? How does this work? I'm sure what you have done is right but you need to explain it more clearly in my mind, this is not just isotope pairing as you introduce it.

Page 2364. Assuming steady states and a lack of significant horizontal advection in the Central-NE Arabian Sea over the time-spans of our experiments. You can't really say this in my mind. The Arabian Sea is a dynamic system with horizontal advection, mixing etc, besides the time span of your experiments has nothing to do with the real 'age' of the profiles in the water column, as I think you are saying at the bottom of page

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



2372. That said, the model produces reasonable representations of the water column chemistry.

Page 2365 line 5. Can you explain the term isoneutral, this is not a pure oceanography journal. Line 22. Nicholls et al came to similar conclusions, and should you not contrast this with Ward 2009 as well, as you contrast it with your own work? In addition, this section is confusing in my mind I am not sure what comes from the previous Jensen paper and what you are reporting here, either as new or supplementary to that in the previous Jensen paper. This section has the heading N-loss activities but further on in section 3.3.3 you report anammox, denitrification and DNRA which are the N loss activities, what's the difference. Please clarify. Lines 26-27. I know there are many interpretations of the various degrees of oxygen saturation but can you should define what you mean by suboxic. To me it means anoxic but with a positive redox, not just a little oxygen. Where you say 'a region previously considered insufficiently suboxic to permit N-loss. In fact, apparent anoxic conditions ($2 \mu\text{M}$) were detected below 110m depth' Please just clarify your terms.

Page 2366 3.2.1. Nitrate reduction. What you are saying is fine but Nicholls et al 2007 also reported nitrate reduction as a 'stand alone process' precisely explaining the nitrite pool and its subsequent reduction.

Page 2367 3.2.2. Ammonia oxidation. Again Nicholls et al came to similar conclusions about ammonia oxidation as a minor source of nitrite.

Page 2368 3.3.1. Why is this material different to that in 3.1? I would just report the profiles in 3.1 and put all the activities together here. Also, make it clearer again what is new here and what you are referring to from Jensen's previous work. I also think you should tone down the section 'Although another recent study reported some moderate denitrification rates at three stations in this region (Ward et al., 2009), detailed time-course data revealed considerable initial time-lags in the incubations for at least the representative depth shown (Bulow et al., 2010). Such results were not considered as

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

in situ activities in our current study.’ It could be interpreted as a little overly negative. The truth more likely is that denitrification comes and goes (as you suggest at the bottom of 2373) and that there are also other unconfirmed pathways confounding the story; as you go on to suggest on line 9-10 on page 2370 (Nicholls et al (2007)).

Page 2368 3.3.2. Ok it’s looking like a personal complaint now but why are you not acknowledging other peoples work on this precise material in the central Arabian Sea?

Page 2372. ‘Relative to the 10-year record, surface chl-a concentrations at the time of our sampling seemed rather representative for the central-NE basin, implying that our measurements were likely not far from the typical N-cycling activities in these waters (Fig. A8)’. Confusing way of saying your data seem typical?

Minor edits and lines that could be rewritten. Page 2358. The last line of the abstract is a bit of a tongue twister. Page 2359. Line 16 Micromolar concentrations rather than levels Lines 24-29 need simplifying/clarifying. Page 2360. lines 12 – 19 are not clear Line 25 vs versus. Page 2365 Line 11. Have you mentioned chlorophyll up to this point? Line 12. near the Omani Shelf. Page 2366 and both processes were detected in the Arabian Sea OMZ. Page 2370 line 12. ...sum of the measured NO₃- reduction and the modeled net change in NO₃-. Page 2371 line 1 do you mean nitrite reduction? Line 3 small net production of nitrite Line 21 suitability rather than aptness? But it is a good point! Line 25 be more explicit? ...whereas the concentration of nitrite present in the water column was... Page 2372 bottom. This is a good point and I agree with 2 and 3. Page 2373...’slow net production, which is, in the long run, balanced by slow exchange with water outside the OMZ, where NO₂- is eventually oxidized back to NO₃-.’

Interactive comment on Biogeosciences Discuss., 8, 2357, 2011.

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