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Interactive comment on "Nitrogen cycling in the Central Arabian Sea: a model study" *by* A. Beckmann and I. Hense

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We thank the reviewer for his/her rapid response and welcome the opportunity to clarify several aspects related to our study. We have identified three main groups of criticisms in the review:

1. *missing supporting evidence for the low N-losses found in our model; dismissal of the N-deficit method and the N_2:Ar-method*

   First of all, we did not dismiss the N-deficit method. The conclusion section (where the cited paragraph is from) presents only a brief summary of our findings. However, in section 4.5 "Nitrogen losses", we have discussed in detail the nitrogen-loss calculations based on the N-deficit method (using the theta–NO-

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relationship), differences compared to our results and the consequences thereof. As pointed out in the manuscript, our N-deficit is even higher than estimates based on observations. The main difference lies with the assumed time scale (1-10 years in previous studies compared to 50-100 years from our model). We have not discussed the phosphorus based N-deficit method by Codispoti et al. (2001), because the method is conceptually very similar to the theta–NO-deficit method, as it also relies on a ventilation time scale.

We did not mention the N_2:Ar method from Devol et al. (2006), because no vertically integrated value for the N-deficit has been provided (this is not surprising, as the focus of this particular part of their study was the comparison of vertically resolved N-deficit from different methods). Since all measurements have been compiled into one graph, it is difficult to disentangle individual stations for comparison. We note, however, that visual inspection leads to similar values of N-deficit for the N_2:Ar and phosphorus based N-deficit method of Codispoti et al. (2001). We found this merely qualitative result too vague to include it in our manuscript.

It may be important to point out again, that the reason for low N-loss is **not** a smaller N-deficit but a longer time scale (see 4.4 “Time scales of the system”).

2. *ignorance of results from the study by Anderson et al. (2007); too low export production*

   The modelling study in question (hereafter A07) is indeed mentioned in the introduction: We point out that due to the relatively strong restoring to observed nutrients and oxygen (i.e. forcing the model to be close to observations) the model is less predictive (in the sense that it provides an independent estimate of the N-loss) than it should be, and that the results concerning N-loss may or may not be meaningful. We are not convinced that they are, but the authors would certainly argue otherwise. In our view, there are several problematic aspects in A07:

First and foremost the above mentioned restoring. Unfortunately, A07 do not provide the total flux of N due to the restoring, which could be substantial [as an example, a relatively small nitrate deviation of 1 mmol m^{-3} from climatology combined with a two month restoring time scale results in an integrated flux of 100 mmol N m^{-2} yr^{-1} when applied over a 600 m thick layer] and is not even necessarily a source of N (as one might implicitly assume) but maybe even a sink.

Second, the biogeochemical model is run for only 30 years, much too short to capture the dynamics of the 50-100 year time scale that we think is more adequate.

Third, the phenomenology of nitrite profiles in A07 does not reflect the typical shapes found in the Arabian Sea. With the exception of the N7 profiles (which in our terminology is the "core" region), none of the nitrite profiles from the model matches the observed shallow peak.

Finally, yes the export production of A07 is significantly higher (about a factor of 2.5) than ours, but – in contrast to what the reviewer claims – it is also significantly higher than observations (see Figure 15 lower right in A07). Since our simulated export production fits well with the observed values for the northeastern Arabian Sea (see Sarma et al., 2003), we see no need to compare our model results with those from another model that performs worse in this respect.

In conclusion, we believe there is ample reason to be skeptical about the model simulation of A07. We have not included these points into the manuscript, as our focus is not to criticize earlier work, but to use the best available observations for comparison.

3. *too low complexity of the ecosystem/biogeochemical model; steady state/no seasonal cycle*

In general, the complexity of a model should be in sync with the questions to be addressed. We have chosen to put more emphasis on the subsurface N cycling than on the surface ecosystem dynamics, as it is more important to include DNRN, DNRA and anammox explicitly, if one is interested in the N cycling in the oxygen minimum zone. To our knowledge, the complexity of the nitrogen cycle (number of prognostic variables and individual aerobic and anaerobic nitrogen transformation processes in the water column) in our model is unprecedented. The main function of the surface layer ecosystem is to generate a reasonable export flux. Again, we like to point out that this export flux is in good agreement with the observed values for the northeastern Arabian Sea. As a consequence, there is no need to explicitly consider zooplankton (which is parameterized in our model by a comparatively large mortality rate).

Concerning the maximum growth rate, we have chosen a relatively low value, because we do not consider the day-night and seasonal cycles in light. This requires an adjustment of the growth rate. We agree that we should have mentioned this aspect in the manuscript (and will do so in the revised version). However, we would also like to note that the steady state model results are not very sensitive to the maximum growth rate, because the production is limited by the upward nutrient flux through the barrier layer and not by the maximum growth rate.

The criticism concerning the detritus parametrization points to a problem for almost all marine biogeochemical models. The spectrum of properties of sinking detritus ranges from highly labile to almost refractory and from fast sinking to almost neutrally buoyant species. Clearly, dead organic matter cannot be successfully modelled by using just one compartment with constant sinking and remineralization rates. As detailed knowledge about fractionation, remineralization rates and sinking velocities is not available, it is common to classify detritus based on the *remineralization length scale* (sinking rate divided by remineralization rate) instead. In agreement with observations (e.g., Lutz et al., 2002), we have chosen one remineralization length scale about 100 m and another one that is significantly longer, i.e., 3000 m. A more detailed discussion can be found in Beckmann and Hense (2009) and the references listed therein.

The steady state assumption is a well-established modelling method, which does not imply that the real system does not change but which allows us to investigate the system in equilibrium. Concerning the missing seasonal cycle, we are not aware of any study claiming that the subsurface nitrite signal changes significantly with season. Given the system response time scale of 50 years and more, the seasonal variability is clearly of secondary importance. Interannual variability or even trends over a few decades may be more relevant, but that is beyond the scope of our study.

In conclusion, we like to point out that we are well aware of the fact that our results may be seen as a contradiction to the majority of previous estimates of N-loss in the Arabian Sea (and have said so at the end of the manuscript). But in our view the manuscript provides both evidence and arguments for the validity of our results: (1) the modelled process rates are in the range of observed values, (2) the phenomenology nicely matches the observed fields and (3) we have an explanation for the previous overestimates (the assumption about ventilation time scales, which is appropriate for the "oxicline nitrate tongue" but not for the oxygen minimum zone below).

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