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Discussions

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

Anonymous Referee #1

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A model is used to study the nitrogen budget in the suboxic zone of the Arabian Sea, using an idealised configuration of a typical transact through the area. As yet, we do not have a full quantitative understanding of the relative roles of different processes in the suboxic zone, including remineralisation, denitrification and anammox, and so modelling studies that address this are to be welcomed. At first sight, the simulation results presented in this ms appear to be a good match with observations (figure 6 versus figure 2). When extrapolated to the entire suboxic zone of the Arabian Sea, however, results yield a N loss of only 1.15 Tg N yr-1, approximately 20x lower than previous estimates. Perhaps the authors have discovered something radically new here, but I am far from convinced. As written, I have significant reservations regarding several aspects of the model and am therefore deeply sceptical about this result. The authors will have to justifiy their model far more rigorously, and provide extra supporting

information in terms of results, if I am to be convinced that this manuscript is worthy of publication.

In more detail:

The single biggest issue that must be addressed is the first paragraph on p. 13,604: "An extrapolation of the model's nitrogen losses over the entire suboxic zone of the Arabian Sea ...yields a nitrogen loss of roughly 1.15 TgNyr−1. The majority of previous estimates range from 30 to 41 TgNyr−1 (Bange et al., 2000; Codispoti et al., 2001; Devol et al., 2006; Naqvi, 2008; Bulow et al., 2010). We argue that inadequate assumptions in the previously applied methods can explain this large discrepancy. First, pointwise loss rates should not be taken as representative for the entire suboxic zone. Second, the N-deficit method should not be applied with a ventilation time scale that may be adequate for the oxicline layer, but not for the layers below. The relevant system response time is closer to 100 yr than to the frequently used 1–10 yr, as proposed by, e.g. Bange et al. (2000).We therefore conclude that the nitrogen loss for the Central Arabian Sea has often been significantly overestimated."

I find it almost breathtaking how, with almost no supporting evidence, the authors have dismissed both the nitrogen deficit methods of estimating denitrification in the Arabian Sea, as well as the N2:Ar method used by Devol et al. I simply do not buy it. The authors need to provide a supporting reference to make their case. Otherwise, they need to provide much more detail and analysis to support their claim that these previous estimates are in effect flawed. Furthermore, the authors appear to have ignored the fact that Anderson et al (2007; A07 hereafter) predicted denitrification of 26.2 TgNyr−1 (they are clearly aware of this study because they cite it, and indeed comment on this model). Where are the flaws in that study if that estimate of denitrification is also to be dismissed?

One way to get at this problem is to examine model predictions for primary production and export flux and compare these with estimates from JGOFS data and the model results of A07. Perhaps I missed it, but the authors do not appear to say what their predicted primary production is in the surface mixed layer. Data indicate ~100 mmolC m-2 d-1, and A07 predictions were close to that. Export flux in A07 was 0.73 mmolN m-2 d-1 = 266 mmolN m-2 yr-1, of which only 14% was predicted to reach 650m. Predicted explored of detritus in the current model appears to be only 103 mmolN m-2 yr-1 (figure 8), which seems a very low given the (well known) level of primary production for the Arabian Sea. Furthermore, 28% of that reaches 1500m. That seems unduly high. So, it looks like there is too little detritus entering the oxicline, and then too little turns over there.

What, then, of the ecosystem model? It is very simple in structure. For starters, it is not even an NPZD construction because it does not include zooplankton. Next, looking at the biogeochemical model parameters (table 1), the maximum phytoplankton growth rate seems rather low (~0.5 d-1). The Arabian Sea is warm and nutrient rich, so why so low? Where does this value come from? My biggest concern over the ecosystem parameterisation is with the detritus. It is split between "labile" and "refractory" fractions. I can understand the need to split between slow-and fast-sinking, but why should the latter be refractory? Fast sinking material may, for example, be large phytoplankton aggregates that are readily amenable to microbial use. I do not see any justification for having a fast sinking detritus remineralisation rate of 0.005 d-1 (table 1); again, where does this value come from? Assumptions of this kind may explain why the predicted N loss in the suboxic zone is so low, so they need careful justification.

In general, the work and approach needs better justification and explanation. I'm not against using an idealised model of a transect, as is the case, as the difficulties of a full 3-D approach are considerable. Nevertheless, the authors should provide a detailed rationale, rather than just paying lip service to Occam's Razor (p. 13,587). The biogeochemical model also. The steady-state assumption needs justifying, particularly as it took 3000 years for the model to reach steady-state. The surface ocean is strongly seasonal, for example, in this area.

Overall, there may somehow be some exciting science here but, as written, I am far from convinced of the authors' approach and results.

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