

**Review of “*Marine denitrification rates determined from a global 3-dimensional inverse model*” by DeVries, C., C. Deutsch, P. A. Rafter, and F. Primeau**

The manuscript presents a new estimate of oceanic denitrification in the water column and in the sediments from a coupled 3D ocean circulation-biogeochemistry model that is optimized against physical and biogeochemical observations, and argues for a balanced oceanic nitrogen cycle.

Denitrification obviously plays a major role in the cycling of nutrients and carbon in the ocean. However, estimates of the rates of oceanic denitrification both in the water column and in the sediments are plagued by large uncertainties - to the point that it is not even clear whether the oceanic nitrogen budget is balanced (for example by internal biogeochemical feedbacks) or not. Only recently, estimates obtained with different methods have started converging to a narrower range of values. The uncertainty is particularly acute for benthic denitrification, which, by means of simple considerations on the isotopic composition of oceanic nitrate, should be between 1-4 times larger than water column denitrification. In the literature, sediment denitrification estimates vary 2-3 folds among each other. Such estimates rely on either poorly-constrained benthic models, or on the combination of water column denitrification rates with global isotope data, an equally uncertain method. In this perspective, the new estimate by DeVries and coauthors, which elegantly integrates informations on nitrogen deficit and isotope distributions, is an important contribution that brings additional evidence for a balanced nitrogen cycle.

The Authors find a pelagic denitrification rate of about 60 TgN/year, very much in line with the lower range of recent estimates. By the way, probably the best-constrained of these estimates comes from a previous study by DeVries and co-authors (DeVries et al., 2012). Surprisingly, the estimate for sedimentary denitrification is ~2 times smaller than the lower range of recent estimates, and includes a large contribution from relatively deep ocean sediments. The smaller than previously though sediment-to-water column denitrification ratio (~1.7) seems compatible with the isotopic constraints, once the degree of nitrate consumption in the suboxic volumes is taken into account. However, one could argue that the model resolution in DeVries et al.'s study is too coarse to capture the extent of denitrification taking place in shallow shelf sediments, and the resulting estimate could be too low (the Authors clearly discuss the possibility). So this is not the end of the story, and future data-model syntheses, for example including measurements of N<sub>2</sub> excess from sedimentary denitrification in bottom waters, will help shed light on the issue.

I found the method used by DeVries and coauthors quite sound, as it integrates our knowledge on ocean circulation dynamics (momentum and mass conservation) with observed tracer distribution (temperature, salinity, nutrients and N isotopes) in an optimization framework.

Major model limitations could include: (1) the use of linearized momentum equations, and the assumption of a steady state circulation and tracer distributions; (2) data availability; (3) a simplified representation of biogeochemistry; (4) computational constraints. I am not an expert in data assimilation techniques, but the circulation model used by the Authors (although in different incarnations) has been thoroughly described and validated in previous papers. Similarly, I cannot comment much on the assumption of linearity in the momentum equations, except that it seems acceptable for the type of large-scale processes addressed here (and probably not worse than the common use of coarse-resolution general circulation models for biogeochemical studies). The assumption of a climatological steady state appears adequate for the scope of the study. The biogeochemical model adopted is rather simple, but analogue to widely tested biogeochemical formulations (e.g. OCMIP-type “restoring” models). Obviously, the simplicity of the model allows for a minimal number of parameters that is functional to the optimization approach. That said, I must note that fundamental processes such as non-Redfield organic matter production and nitrogen fixation are explicitly considered by the Authors. The inverse-model approach does not grant that the optimization does not end up compensating for model deficiencies. As an example of this kind of compensations, one could ask whether a reduced shallow denitrification due to unresolved shelves might force the optimization to increase benthic denitrification in the rest of the ocean in order to achieve  $N^*$  and N-isotope consistency with the data. However, this sort of problem is common to any modelling effort. Finally, data availability and computational constraints might be at the base of some of the technical choices of the study, e.g. the 2-step optimization of the circulation/biogeochemistry first and of the nitrogen cycle second, and the subset of parameters chosen for the optimization. More isotope measurements would certainly not hurt, but the major locations where water column denitrification is known to happen, as well as regions representative of larger areas of the ocean basins, are adequately covered in the study.

The manuscript is well organized and written, and the assumptions, the model limitations, and the sources of uncertainty are generally clearly addressed. I consider this study an important and useful contribution - both in terms of the results and of the methodology - and I recommend its publication in *Biogeosciences*. Overall, I think that a few minor points could benefit from clarifications.

- Model formulation. What is the value of the depth attenuation coefficient for particles (b) used in the biogeochemical model? This is perhaps the most important parameter for remineralization as it controls the proportion of particulate organic matter that is remineralized in the water column and the fraction that reaches the bottom. As such, the choice of this parameter has a first impact on denitrification by controlling the particle flux that fuels sedimentary denitrification. Furthermore, we know that the attenuation of the particle flux

within suboxic regions is substantially lower than in the open ocean (approximately 0.3-0.4 vs. 0.8-1.0, e.g. Martin et al., 1987; Devol and Hartnett, 2001, Van Mooy et al., 2002). This implies that less export is remineralized inside suboxic waters. In fact, a recent study by Bianchi et al. (2012) showed that both water column and sediment denitrification are quite sensitive to the value of  $b$  inside and outside suboxic waters. Thus, I'm a little surprised that (1) a uniform value of  $b$  is used for oxygenated and suboxic waters; and (2)  $b$  is not included in the set of parameters optimized against  $N^*$  and N-isotope measurements. I suspect that the lack of oxygen-dependence for  $b$ , and the lack of optimization against nitrogen tracers, might be due to data limitation (do nutrient measurements allow to solve for an oxygen-dependent  $b$ , when so many other factors control particle remineralization?), as well as limitations in the optimization procedure (that is, first using  $PO_4$  to solve for the optimal circulation, second solve for the optimal nitrogen-cycle model parameters) that require the same  $b$  value for particulate organic phosphorous and nitrogen flux attenuation (otherwise the N:P stoichiometry of remineralization would vary with depth). However, given that  $b$  controls the model partitioning of remineralization between water column and sediments, I feel that a discussion of the choices of keeping  $b$  constant, and not including it as part of the nitrogen-cycle optimization, is needed.

- Figure 3.b: the model overestimates the magnitude of  $N^*$  in the thermocline and deep ocean - with a bias of up to  $\sim -1$   $\mu\text{mol}$ . This misfit is described in section 2.3. Whereas the overall denitrification signal is about  $\sim 5$  times larger, the implications of this bias could be further discussed. For example, does it imply that the model overestimates the strength of denitrification in the thermocline and in the abyssal ocean by up to  $\sim 20\%$  at those depths over the whole ocean? This seems at odds with the low denitrification rates found in the water column and in particular in the sediments (relative to existing estimates).
- I found the discussion in section 4.1 useful, but rather technical. I suggest that the Authors introduce the paragraphs addressing the effects of denitrification on  $N^*$  and N:P ratios with a couple of sentences that briefly introduce why these effects are important to a broader audience.
- Sedimentary denitrification model. While I have no reason to prefer a model formulation over another, given the empirical nature of the sedimentary model itself, and its subsequent optimization against data, the authors could be more generous in describing the assumptions behind it. In particular, the only reference given is the study by Middleburg et al., 1996, which provides quite different empirical relationships. For example Middleburg's relationships show a non-linear dependence between denitrification and particle fluxes, while DeVries and coauthor assume linearity (equation A11). Also, why a hyperbolic dependence is used for  $NO_3$ , and a hyperbolic tangent for  $O_2$  (equations A13-14)? Again, these are technical choices but more discussion could be useful.

**Technical comments:**

- page 14015, line 21. The delta notation should express the ratio between  $^{15}\text{NO}_3/^{14}\text{NO}_3$  and a standard (e.g. atmospheric  $\text{N}_2$ ).
- equation A6. What is  $T_{\text{max}}$ , and what value is used?