

Interactive comment on “Marine denitrification rates determined from a global 3-dimensional inverse model” by T. DeVries et al.

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1 Summary

DeVries and coauthors use an inverse approach that combines observations of various tracers and an ocean circulation model to estimate the rate of marine denitrification. They find a global denitrification rate of between 120 to 240 Tg N yr⁻¹, which is at the lower end of all estimates, but in line with the most recent ones. About one third of the total rate is driven by water column denitrification, and the other two-thirds by benthic denitrification. The implied ratio of benthic to water-column denitrification of about 2 is also lower than the original estimates, and implies a relatively modest global efficiency by which the isotopic signature of water column denitrification imprints itself

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on the global NO₃⁻ pool.

2 Evaluation

Each year denitrification removes several hundred Tg N yr⁻¹ from the fixed nitrogen pool in the ocean, stripping the ocean from this essential nutrient. Yet the views on how large this sink actually is still diverge substantially, although most recent estimates tended to cluster on the lower end of the spectrum.

DeVries et al. now add a very powerful and important new estimate to this discussion. They use, for the first time, a global 3-D model, and assimilate a suite of N-related tracers to estimate separately the rates of water column and benthic denitrification. They thereby demonstrate the importance of the previously identified dilution effect in determining the global ratio between water column and benthic denitrification. I particularly like the insightful discussion of the factors that control this dilution effect and hence the benthic to water column denitrification ratio, and also the discussion of how denitrification impacts the determination of the N:P remineralization rates from observed NO₃⁻ and PO₄³⁻ data.

The study was carefully and insightfully designed, the paper is well written and illustrated, and the results are clearly novel, interesting and important. This manuscript is therefore very well suited for publication in Biogeosciences. I have many comments, but none of them is of a fundamental nature. They are rather intended to make an already excellent study (hopefully) better.

I list here the major comments, while I discuss the minor (general and specific) ones below. All of the major comments deal with various error sources whose potential contribution to uncertainty could be better discussed. At the moment, the uncertainty section considers essentially just the "internal" uncertainties, i.e., those emerging from the assimilation system, and pays limited attention to the "external" errors, i.e., biases,

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particularly those that are of structural origin.

- (i) *Circulation model error*: The results critically hinge on the model's ability to correctly capture the circulation of the oxygen minimum zones, as it is the relative consumption of nitrate in these regions as well as the "efficiency" by which this signal is mixed out into the rest of the ocean that is critical for determining the dilution effect. I therefore consider it important to learn more about how well this model is able to capture the circulation of these regions. Global coarse resolution models have notorious problems in these regions, and it is not clear that a data constrained model will necessarily do better. In this regard, I was a bit surprised to read that the authors used here a version of the circulation model that was not optimized with CFCs, but "only" with T, S, and radiocarbon. While the latter is certainly a very good constraint for the deep ocean, it is not that well suited for constraining thermocline rates. There is some indication of the thermocline circulation potentially being a problem in that the optimized profiles of N^* differ substantially from the observed one in the thermocline across the Indo-Pacific. It thus seems to me that this aspect deserves a deeper discussion.
- (ii) *Data error*: The other main ingredient of any data assimilation system are the data. My understanding is that the authors are using the objectively mapped N^* data of the World Ocean Atlas. These data underestimate the extent and magnitude of the low N^* in the oxygen minimum zones (see e.g., Eugster and Gruber, (2012)), likely due to the strong smoothing that was applied when this data product was produced. Presuming that a substantial fraction of the results are driven by the model trying to match the low N^* data in the oxygen minimum zones, any errors in the data have a direct effect on the results. I also wonder why the N_2/Ar data used in deVries et al. (2012) were not included here as an additional constraint.
- (iii) *Shallow seas*: With much of benthic denitrification occurring in shallow seas

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that are poorly represented in the relatively coarse resolution model, it is unclear how this structural error imprints itself onto the final results. Our experience using a structurally much simpler model, but essentially the same data constraints (Eugster and Gruber, 2012) leads me to believe that this may turn out to be rather unimportant source of error, as the global results are strongly driven by two numbers, i.e., the water column denitrification rate, and the global mean $\delta^{15}N$. But it would be useful to know more about this than the somewhat "ad hoc" argument that the likely underestimate of benthic denitrification may be compensated by the lack of consideration of the riverine input of N.

- (iv) *Atmospheric deposition and Riverine input*: These are two important sources of fixed N to the ocean, perhaps as large in magnitude as water column denitrification. In addition, these sources might have undergone a substantial change over the anthropocene, with some studies suggesting a doubling of the overall input. This raises two questions: First, in what way will the lack of consideration of these two fluxes impact the results? Second, how will the large transient in these fluxes interfere with the essentially steady-state assumption that underlies this inverse modeling system?

3 Recommendation

I recommend acceptance of this manuscript after a minor to moderate revision. I particularly recommend that the authors extend the discussion of the potential biases in their estimates emanating from structural errors in the assimilation system.

4 General (minor) comments

Anammox is not mentioned anywhere in the whole manuscript. With some authors arguing that this process represents a large sink for fixed nitrogen in the ocean, it behooves the authors well to discuss this process and what it means for the interpretation of their results.

The authors should clarify better the similarities and differences of this study with the recently published deVries et al. (2012) paper in Nature Geoscience. Although this is partially done, it would be helpful for the non-expert reader to be provided with a succinct summary.

I admit that this is self-serving, but the recently published article by Eugster and Gruber (2012) in Global Biogeochemical Cycles addresses many similar issues (e.g., global rates, dilution effect, benthic to water column denitrification ratio) on the basis of a fundamentally similar approach. Thus it would make a lot of sense to discuss these results in the light of the findings presented here. I found it very intriguing that the global rates turn out to be rather similar and also the benthic to water column denitrification ratio is remarkably close. Is this a sign of robustness in these findings, given the very different nature of the underlying circulation models (3D versus box model), or is this just coincidence?

5 Specific (minor) comments

section 2: inverse nitrogen model. I am wondering how the variations in the N:P uptake ratios in the Southern Ocean are dealt with? I presume that the simultaneous restoring of N and P toward observations takes care of this, and that the particular N-fixation parameterization (linked to atm. Fe deposition) avoids the diagnosis of elevated N-fixation in this region. As Deutsch and Weber investigated this issue in other

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publications, it would be good to know in a more explicit manner how this is considered. This may be especially important for determining the preformed N^* in the thermocline of the Southern hemisphere.

section 2.1: If I am not mistaken, this is an annual mean model. This should be mentioned here explicitly. I don't think that this is extremely critical here, but in the real ocean, seasonal variations in physical supply and nutrient drawdown are correlated, leading to co-variances that are not captured by an annual mean model.

section 2.2: Optimization and appendix B: It would be useful to know how the authors ensure that their optimization method is not falling into a local minimum. With this being a highly non-linear problem with likely a large number of local minima in the cost function, this can easily happen.

section 2.2: Cost function: I also think that it would be useful to be more specific and explicit about the formulation of the cost function and add also some details already in the main text. I presume that the authors do not include a regularization (or Bayesian) term (e.g., by penalizing deviations from the initial guess), but I wasn't sure from reading the text.

section 2.3, lines 7-8: "good fit". I would love to see more sensitive measures of model data misfit than a plot of observed vs measured N^* . In particular, one wonders about the regional distribution of the residuals. Some of this is shown in Figure 3, but in a highly aggregated manner.

section 2.3, lines 13-14, "relative nitrate consumption". This may not be that relevant in this section, but it will be later in the discussion (p14026, lines 1). So I raise it here already. The equation $f_c = 1 - NO_3 / (16 \cdot PO_4)$ is not really correct for estimating the degree of nitrate consumption in the oxygen minimum zones. This equation works only if preformed N^* is 0. Otherwise, the relative degree of nitrate consumption needs to consider the preformed value of NO_3 and PO_4 explicitly. Based on our own calculations in Eugster and Gruber (2012), we found the preformed N^* value to be important for the

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calculation of the relative N consumption inside the oxygen minimum zones.

section 4.2: I commend the authors for this very useful discussion.

section 4.2, p14026, lines 1ff: See above comment on the definition of f_c . Despite my concern, the approximation seems to work quite well, but I would love to see this calculation repeated with a more exact definition of the relative nitrate removal. One of the reasons for raising this issue is that we found in Eugster and Gruber (2012) only a moderate relationship between relative nitrate removal and the magnitude of the inversely estimated benthic to water column denitrification rates across the 2500 circulation configurations we considered.

Appendix B, lines 19-29. "cost function". See also comment above. In addition, it wasn't entirely clear to me how the different constraints were weighted relative to each other. In addition, the data are highly non-randomly distributed (especially the $\delta^{15}\text{N}$ data, so that some adjustment might have been necessary. Finally, the authors write "This choice is made so that the primary factor controlling the final value of the parameters is constraints provided by the [...] observations". This is unclear to me.

6 References

Eugster, O., and N. Gruber (2012), A probabilistic estimate of global marine N-fixation and denitrification, *Global Biogeochem. Cycles*, 26, GB4013, doi:10.1029/2012GB004300.

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