Response to Reviewers: Shape of the Oceanic Nitracline

Interactive comment on
"Shape of the oceanic
nitracline"
by
M. M. Omand and A. Mahadevan

Please note that our replies to the Referees are included in the document below and the changes we have made to the manuscript text are marked in red.

Anonymous Referee #1
Received and published: 24 December 2014
This paper describes statistical relationships between nitrate and density along vertical profiles in the ocean. In particular nitrate concentrations are regressed onto a linear combination of density to the zeroth, first, and second powers. Two regimes are distinguished: one in which the relationship between nitrate and density is parabolic and one in which it is linear. A 1-d vertical diffusion model is used to rationalize the results in terms of the separation between the euphotic depth and the mixed layer depth. In cases where the euphotic depth extends less than 100m below the mixed layer depth the model produces a nearly linear relationship between nitrate and density. Otherwise the nitrate-density relationship produced by the model has curvature. This result is not surprising. In the absence of sources or sinks we expect a linear relationship between the tracers because they satisfy the same diffusion equation with a common eddy diffusivity. Only when the euphotic depth is significantly greater than the mixed layer depth does the biological-uptake sink of nitrate extend over a large enough region of the water column to produce any appreciable deviations from linearity. However, this results seems artificial because in addition to the biological drawdown of nitrate included in the model there is also a remineralization of nitrate from sinking particulate organic matter.
This important source term is missing in the model considered in the paper but needs to be included because it will almost always extend significantly below the depth of the mixed layer and will potentially break the linear relationship everywhere.
I therefore recommend that the authors consider adding the remineralization of sinking particles, perhaps modelled as a power law F(z) = Jo*(z/zo)ˆ-b. .

Thank you for this suggestion. We have added a remineralization source term to our model, which improves the results. We used an exponential profile for the sinking flux (Fig. 8) instead of the power law (Martin curve), because it makes the model solution more tractable and is also a good a description of the sinking flux profile. The sink and source terms of nitrate have different e-folding depths, but the total nitrate is now conserved in our model. Further, we choose the model parameters (mixed layer depth, euphotic depth) in accordance with the parameters at BATS and HOT. See Section 5 for a description of the revised model. As a result of the remineralization term, the shape of the modeled profile is much closer to the observed shape at BATS and HOT with a subsurface maximum and thereafter, nitrate which stays roughly constant with depth (Fig. 8).

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This paper proposes examining the nitracline using a metric that relates it to density rather than depth. From a dynamic point of view such a metric is interesting as it relates the potential supply of nutrient to the mixed layer to the potential supply of energy from mixing (the energy of mixing is just KvN^2 where Kv is the vertical diffusion coefficient and N is the buoyancy frequency. Thus time periods where the thermocline thins may bring nutrients closer to the surface, but have little impact on the actual supply. The authors fit curves to the nitrate-potential density relationship, and look at the curvature.
They show that these coefficients change over time in some interesting ways at a couple of stations and then try to interpret the meaning of the resulting patterns. In particular, they interpret positive curvature as arising when the euphotic zone depth penetrates into the upper thermocline and causes nutrient drawdown there.
I think this work makes a positive contribution, in particular the station work, which shows really interesting changes in coefficients over time has the potential to reveal dynamically interesting changes. However, I do have a couple of concerns which need to be addressed before this work is ready for final publication.
1. Calculation of the coefficients. In working to reproduce this calculation myself, I found that the answers depended significantly on how the independent variable is calculated. I got quite different results when I used the raw potential density vs. the potential density difference relative to the surface, or the potential density anomaly over the profile. It seems to me that the latter is what you really want. The strong relationship between the a and c coefficients strikes me as problematic, indicating that the mean value is affecting the curvature.

Thank you for pointing this out. It is indeed correct. We have redone the curve fits for the density anomaly, where the uppermost point in the nitrate profile is subtracted from density to obtain the density anomaly. In addition, we found that the fit is quite sensitive to the choice of the upper bound nitrate and density used to delineate the portion of the profile that is fit by a polynomial. Visual examination of the profiles in the World Ocean Atlas revealed that many of the profiles had a lower inflection point in the curve, which is the appropriate point to cut off the profile. We have been able to significantly improve the fits to the WOA profiles by picking the upper bound density and nitrate by eye. Figs 2, 3 and 4 are revised with these new methodological improvements that lead to better “skill”, less noisy results and more coherent patterns.

2. Physical interpretation. The basic mechanism used to explain changes here is production. But in many places remineralization may be just as important. Consider the two profiles in Figure 1, one in the Pacific cold tongue, the other near Hawaii. Both cases have clear curvature. But the bulk of this curvature doesn't arise from the surface, but rather the persistence of nutrient gradients in the low stratification region below 100m. If I look at my (rough) calculations of curvature as well as the calculations in Figure 3 I see what looks like North Pacific Mode Water emanating from the Northwest Pacific. Similarly, one would expect that a'<1 yielding a linear fit would have some spatial relationship to chlorophyll, whereas one tends to find linear fits in the center of the subtropical South Atlantic and Pacific gyres where chlorophyll is low, and higher values along equator where chlorophyll is high. The failure of the model, as currently configured to explain the variation at HOT is also worrisome (although I take the point that the data lies within the envelope, the first time through this looked like a failure of the model to me).

As also stated in our response to another reviewer, the model has now been extended to include a remineralization term, which improves the results. One nice aspect of the inclusion of this term, is that the solution now reproduces the nitrate maximum at intermediate depth (generally 500-1500m) that is a ubiquitous feature throughout the global ocean. We used an exponential profile for the sinking flux (Fig. 8) instead of the power law (Martin curve), because it makes the model solution more tractable and is also a good a description of the sinking flux profile. The sink and source terms of nitrate have different e-folding depths, but the total nitrate is now conserved in our model. Further, we choose the model parameters (mixed layer depth, euphotic depth) in accordance with the parameters at BATS and HOT. See Section 5 for a description of the revised model. As a result of the remineralization term, the shape of the modeled profile is much closer to the observed shape at BATS and HOT (Fig. 8).

Having said this, it remains true that the model provides only a simple one-dimensional framework for explaining the shape of the nitracline. It fails to capture many other kinds of variability (such as the curvature trend seen at HOT in Fig. 6, or any effects of lateral fluxes. In regions where such factors are important, the model does not capture the variability. A paragraph has been added to the mansuscript on pg 12 to discuss this.

3. I would also suggest that the motivation for the paper bring out the "ease of turbulent supply" idea that is now primarily mentioned at the end of the paper.

This has now been moved from the Discussion, into the Introduction of the paper. The other points in the Discussion section are also moved to the Introduction and presented as motivation.

4. Most models of productivity tend to saturate at high light levels, or even to drop slightly. I don't think this would change the results substantially, but it would be worth examining whether a Michaelis-Menten light curve with half saturation at 4 Einsteins

would yield a different result.

We conducted model simulations with the phytoplankton growth rate (alpha gamma Eo) according to a Michaelis-Menten light curve with a half saturation at 4 Einsteins as recommended. In nearly all cases, the saturating part of the growth curves occurs within the mixed layer, where any effects are vertically homogenized. For these cases, the saturating growth curve did not alter the results. Where the mixed layer was extremely shallow (20m and less), the saturating part of the curve did extend deeper than the MLD, but because nitrate was very drawn-down within this portion of the euphotic zone, the resulting profile was indistinguishable from the case with a non-saturating light-dependence. The following sentence has been included in the results (pg 11) *"We also formulated the model with a light-saturating growth curve, but found that the steady-state solution remained unchanged"*.