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

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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 $K_v N^2$ where K_v 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.

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).

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.

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.

Interactive comment on Biogeosciences Discuss., 11, 14729, 2014.

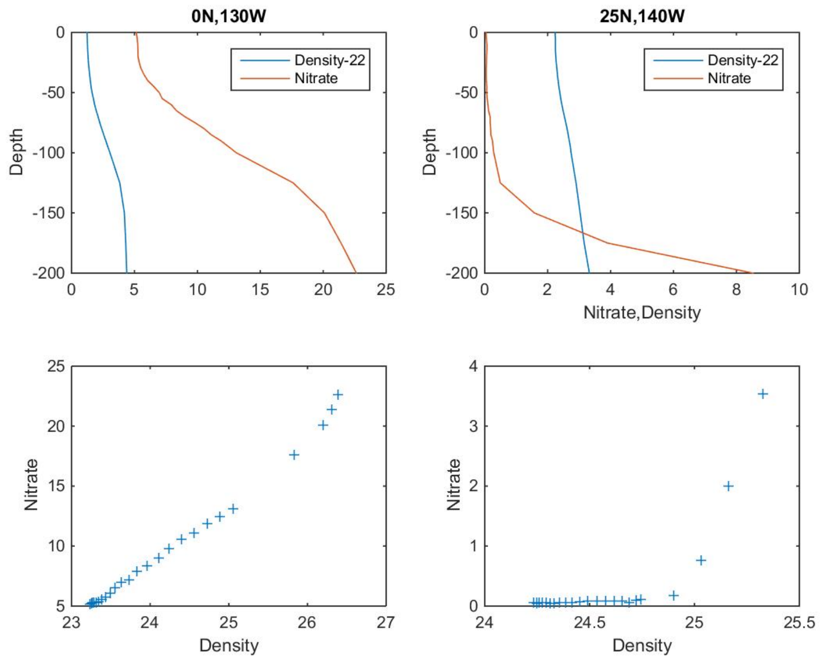

Fig. 1. Nitrate and density relationships at two points. Left-hand column along the equator at 130W. Right-hand column is 25N, 140W. Data taken from World Ocean Atlas 2013 dataset.