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Biogeosciences Discussions

Interactive comment on "Estimating mixed layer nitrate in the North Atlantic Ocean" *by* T. Steinhoff et al.

Anonymous Referee #2

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I have reviewed the paper "Estimating mixed layer nitrate in the North Atlantic Ocean" by T. Steinhoff et al. Unfortunately I cannot recommend the paper for publication without major revisions for the following reasons:

The authors propose model based on sea surface temperature (SST), mixed layer depth (MLD), time of year and latitude for a relatively limited domain (40N-52N, 10W-60W). They achieve a predictive accuracy of $\sim$1.5 umol/L (compared with measurements) comparable with that of previous studies, often with simpler regression models.

Because measurements are generally confined to a sub-domain of the study area, the authors use model simulations to assess possible limitations to their predictions due to the under-representativity of their observation sample set. This is a nice approach, but unfortunately underlines further shortcomings of the regression models, as nitrate

predictions are markedly poorer in regions where there are few or no observations.

It is unclear what the intended application of the model predictions is, and there is no discussion of whether predictive accuracy acheived is adequate to meet that end. This is a major shortcoming which makes it very hard to judge the value of the paper (particularly as without this information one tends to think that 1-2 umol/L prediction errors would compromise most likely applications).

The novelty of the proposed method resides in the attempt to introduce MLD as a regression predictor (to represent seasonal and interannual variability in sub-surface nitrate supply). Unfortunately there is no discussion of the benefit (reduction of variance) on introduction of this predictor. Moreover, several of the predictors, in particular SST and MLD, will be correlated. One suspects much of the variance associated with the seasonal cycle would be just as adequately represented with a smaller number of predictors, so a more complete presentation of the stepwise regression results would be useful to justify the full model. No discussion is given to as to why chlorophyll was not used as a predictor: this has been shown to give a significant error reduction in other regression models (e.g. Goes et al, Sherlock et al.).

In section 3.1 the authors claim it is possible to find a robust estimate of MLD with good spatial and temporal resolution, but this appears to be based on analysis of just 31 ARGO profiles. This analysis established the Lorbacher methodology gave a good retrieval of visual estimates of the MLD. It is not clear to me why the authors did not extend their preliminary analysis to apply the Lorbacher algorithm to all ARGOS float data in the study region, and compare these results to predictions from the Mercator model. This would have provided a much stronger validation and justification for the Mercator MLD in the regression model.

If aim of the model development is to accurately predict interannual variations from the climatological seasonal cycle of nitrate (f(lat,time)) in the region, and assuming both SST and MLD predictors can be measured or modelled with sufficient accuracy, then I think one would need to demonstrate -i- an unbiased model of climatological seasonal cycle in the region -ii- that anomalies with respect to this climatology show a significant correlation with SST and/or MLD anomalies i.e. that a significant fraction of residual variance about the seasonal climatology can be accounted for by these predictors Comments in the text about 'patchiness' suggest that a significant component of variance in observed nitrate is not in fact correlated with either SST or MLD, in which case another observable predictor (sea surface height? Chl-A?) would probably be needed for useful predictive skill.

Other comments:

Figure 6 would be vastly improved by a panel showing the difference between predicted and measured nitrate.

The references to 'time dependent terms' in paragraph 2 of section 3.2 is not clear: do the authors mean the terms in cos(t) and sin(t), or these terms and the MLD and SST terms (which are implicitly time varying)?

The discussion of pCO2 is speculative, and could perhaps be limited to one paragraph in the discussion.

Interactive comment on Biogeosciences Discuss., 6, 8851, 2009.