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Biogeosciences Discussions

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

T. Steinhoff et al.

tsteinhoff@ifm-geomar.de

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We are very grateful for your comments to our manuscript. Attached are our answers (in italic) to the comments:

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 1.5 umol/L (compared with measurements) comparable with that of previous studies, often with simpler regression models.

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- A paragraph will be added to the discussion section where the differences between our equation and the results of other studies will be explained. We think that the presented MLR is easy to use to predict nitrate in the study region.

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.

- We believe that you are referring to the north-western part of the study region. Here we think the predictions fail because of the influence of the Labrador Current. Its extension and variability cannot possibly be covered through our predicting variables (lat, SST and MLD). This points at the general problem such approaches in frontal regions.

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).

- A paragraph will be added to the discussion section where we will discuss the application of the nitrate estimations more thoroughly. Especially the influence of the uncertainty of the estimations will be discussed.

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

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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.).

- The discussion section will be changed to explicitly point out the role of MLD as a predictor. More details about the stepwise regression will be shown (e.g. showing the refinement of the algorithm step by step). This will also help to understand the significance of MLD. We think it is a general problem in oceanography that most parameters are correlated. But with the given data set and the idea to find an algorithm that is easy to use (e.g. input parameter that are easily accessed) it was acceptable to choose input parameter that are not totally uncorrelated.

We have our doubts about the inclusion of chl-a which are based on the poor predictive capacity of chl-a for pCO2 in the study region (Lüger et al. 2008). The role of its inclusion is only discussed with respect to pCO2. Here we will add a paragraph that explains our reasons in detail.

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.

- Good advice; we will apply the Lorbacher criterion to all ARGO profiles and do the comparison.

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

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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.

- Will be added to the MS: We will add a figure that shows nitrate predictions based on an algorithm that uses only lat and time and predictors (i.e. climatology). A comparison of the measured nitrate values with this results and the results obtained with our algorithm will show the influence of SST and MLD.

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.

- We don't think that a simple approach as MLR (or even neural network) can be used to completely explain nitrate variability in the ocean. This is particularly true for small-scale variability (patchiness). However, if there are predictors such as sea surface height that improves the algorithm significantly they should be used. We will check this and discuss it more detailed.

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

- Will be done together with Fig 4. (see also comment to referee 1).

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)?

- Will be stated more clearly in the revised MS:

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

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- Yes, it is speculative but shows a possible implication of nitrate estimations. In addition it could explain observed features in the carbon system (Corbiere et al., Tellus, 2007).

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