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Ocean Science Discussions

Interactive comment on "Detecting changes in Labrador Sea Water through a water mass analysis of BATS data" *by* A. Henry-Edwards and M. Tomczak

Anonymous Referee #1

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1. General comments

The manuscript *Detecting changes in the Labrador ....* by A. Henry-Edwards and M. Tomczak discuss the application of a water mass mixing analysis (TROMP analysis) using time series data from the BATS site (Sargasso Sea) to derive changes in source water properties at a remote place, the Labrador Sea. Based on a system of mixing equations but with several unknown the problem is nonlinear and underdetermined. To find a set of solutions the task is to set meaningful constrains on the variables which makes the problem a 'nonlinear constrained problem'. A nonlinear constrained problem can be solved e.g. using a sequential quadratic programming method (Matlab based). The authors consider only variability in T and S and constrain through ob-

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served changes in the Labrador Sea T/S. The authors know that this is a bit from the track, as typically one would like to derive the unknown changes rather then using the observed changes as constrains. The final solution shows similarities with the T/S evolution in the LabSea while they found that the nitrate source water for the LabSea needed adjustment as well.

Main conclusions and my concerns are:

(i) Nitrate (but not other nutrients and oxygen) underwent substantial changes in the LabSea (TS as well but it was used as a constrain) during the decade under investigation.

Concern: From direct observations (time series) in other convection areas we know that mixed layer nitrate indeed increases as a result of entrainment of water from below the mixed layer during the deepening phase. Therefore it appears to me plausible to assume that different intensity in mixed layer deepening (essentially convection activity) produces different source water types. However, the authors found this variability for nitrate only while one would expect other nutrients and oxygen to behave similar (it has been shown by Mick Fellows and colleagues, MIT, in a recent GRL note that oxygen is particularly undersaturated in deep mixed layers) as the signal for all comes from below (the water underneath the seasonal mixed layer). Maybe the heavy weighting of nitrate in comparison to the other nutrients is the reason - this needs to be shown.

(ii) T/S source water derived from TROMP analysis suggest that this water is colder and more saline than water observed at the periphery (station BRAVO).

Concern: As no 'quality control' on the TROMP results is presented (something like a convergence parameter of the algorithm) I doubt that optimization is able to detect such marginal differences in source water types.

In summary I do not feel that the material presented really allows to draw this conclusions. Again, my concerns are based on: the system of mixing equations used (with or without biogeochemical cycling included), the weighting matrix (inhomogenous weights on nutrients and oxygen, in particular the 8 times higher weight on nitrate than phosphate prefers nitrate in the analysis), and the unknown 'quality' of the results obtained through the sequential programming code. I recommend not to reject the paper but send it back with major revisions required.

2. Specific comments

2.1. Technique/Method

Most technical concern I made on my review on the methodical paper (*OSD: Remote detection of water property changes from a time series of oceanographic data* by A. Henry-Edwards and M. Tomczak). I came through one other point which I missed in the technical paper. The reader is told that a TROMP analysis technique is based on a 'sequential quadratic programming method' (see e.g. abstract). Being not a specialist in the algorithm I wonder if this is really the 'technique' or just a kind of code to solve the problem. I found that the nonlinear problem is solved through defining 'constrains' which makes it a nonlinear constrained optimization problem and which is maybe a 'technique' to find a solution for the nonlinear system of equations. The sequential quadratic programming just one way to solve a nonlinear constrained optimization problem (I learned other solution algorithms might be sequential linear programming, feasible directions, interior and exterior penalty functions, ...).

2.2. Source water types

I do not understand why the authors use the upper Central Water (uWNACW) as one of their source water types although it does not contribute to the depth ranges considered (page 412, row 11-15). As it turned out the the TROMP analysis can only be applied to the data in 1100m and below. Here no uWNACW can be expected (only lWNACW) from the analysis. Removing uWNACW from the source water matrix would reduced

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the number of source waters to be determined and would better constraint the rest of the parameters.
Omitting uWNACW the diagonal type weighting matrix (see below) would change as the variance in source water types would be much smaller.

2.3. Weight

I have already discussed the weight in the review of the other paper (*Remote detection of water property changes from a time series of oceanographic data*).
However one other point: from personal experience I do not agree on page 421, row 21-22: OMP analysis, ... insensitive to the choice of parameter weights ... . It is true that the magnitude of the weights doesn't matter, say weights of 1 2 1 3 or 100 200 100 300 have the same effect. But weighting nitrate (here: 32) higher than oxygen (here: 30) and 8 time higher than phosphate (here: 4) will give nitrate a much more prominent role in the analysis and consequently the system is increasingly sensitive to changes in nitrate instead of phosphate. Although nitrate and phosphate are connected via the 'Redfield ratio' (as long as denitrification does not play much of a role) a different weighting in the OMP or TROMP analysis force them to behave 'non-Redfieldian', as observed here.

2.4. Nitrate changes and Redfield ratio and discussion

*) p422, row 25 and following (including discussion): The Schneider et al. 2005 paper shows exactly what the authors did is not appropriate to derive Redfield ratios (applying a linear regression or reduced mayor axis fit). Schneider et al had model data available which was based on constant Redfield stoichiometry. They applied different techniques to derive Redfield ratios and compared to the 'model

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truth’ (the model was based on). They used linear regression (reduced major axis), multiple-linear regression, mixing triangle, and OMP analysis (with biogeochemical cycling included). They found that, although the Redfield ratio was constant by definition of the model, the different methods derived (even apparently systemtic) non-constant ratios. The only explanation was that mixing of different source water types alters the ratio apparently. Based on these findings I do not see that the temporal trend the authors present us (Fig. 5) are real but rather a consequence of not or inadequate consideration of mixing (which is probably also true for the work cited by the authors in the discussion section, p425, line 22-26). Note, a high correlation between parameters is not an indicator of a valuable Redfield ratio estimate.

*) The authors are aware of the fact that the results of the method are only valuable if new oceanographic knowledge is gained. They know that this is not the *verification* of certain trends in observed source waters. First of all new insight is suspected from the trends that they found in nitrate source water. As outlined above (in particular weights and equations used) I do not see that the trends can be excepted to be real.
The differences between BRAVO and inner LabSea characteristics are small and I would expect that some sensitivities or error inherent in the methods are presented to believe them. There is a considerable amount of lateral mixing along the core of the LSW when it spreads towards Saregasso Sea from its formation region. Thus the different types of LabSea water will not reach BATS in successive pulse but as a kind of LSW ’melange’. Therefore one individual type of LabSea water mixes not only with uWNACW and ISOW but with different blends of itself - again this is not considered in the way the mixing equations are set up.

Interactive comment on Ocean Science Discussions, 2, 417, 2005.