

Interactive comment on “A novel method for diagnosing seasonal to inter-annual surface ocean carbon dynamics from bottle data using neural networks” by T. P. Sasse et al.

Anonymous Referee #1

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In this important manuscript, the authors present a neural network based approach to constrain the non-linear inter-correlations in marine carbon system from standard hydrographic parameters. They estimate the seasonal and interannual variability in total dissolved carbon dioxide (CT) and total alkalinity (AT) fields in the open ocean mixed layer globally and validate their encouraging results against a number of independent in-situ data sets. The manuscript was a pleasure to read, with sound analysis, the right degree of detail and appropriate balance between the background information and new material. One formal criticism should be easy to fix. I think that this work will be of interest to readers of Biogeosciences and I recommend its publication after looking into my formal criticism and a few minor comments.

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GENERAL COMMENTS

The Self Organizing Map neural network (SOM) was used several times over the past 8 years to help us constrain the distribution of carbon parameters (predominantly partial pressure of carbon dioxide, $p\text{CO}_2$) in the ocean (all work duly cited in this manuscript). Similar to previous reports, this manuscript confirms that the SOM based estimates of the marine carbon parameters agree better with the values measured in-situ, than do the estimates based on multiple linear regression (MLR) based approaches.

However the real value of this work lies in three great improvements over previous attempts:

1) SOM and MLR combined

The first is the fact that the SOM and MLR are combined into one modelling system (called SOMLO by the authors). Such procedure is shown to improve the method's predicting capability over what was proposed to date and is described in the manuscript as SOMM. Using what authors call a local multiple-linear optimizer (MLR in this case) in addition to the global SOM optimizer improves the model's ability to constrain local distributions of analysed parameters at a global scale. It's specifically visible in regions of high complexity or high intensity, which usually happen to be important regions for marine carbon cycle. SOMLO preserves the large-scale (basin-wide) robustness reported previously by authors using the SOM approach, but it reproduces local minima and maxima with much better accuracy.

2) Global SOM

The second improvement over previous work is the global scale of this analysis. As far as I know this is the first attempt to estimate carbon parameters beyond regional or basin-wide scale using SOM. In this context it's interesting to see how adding the geographical information to training parameters (in a way that decreases the effect of latitude/longitude discontinuity) improves the estimates. It was argued before (eg.

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Telszewski et al 2009) that for basin-wide estimates such addition causes artificial divisions and introduces some rigidity to the SOM results. However some level of regional division seems to be beneficial for global estimates, where great in situ variability of inter-correlations between biogeochemical water properties is combined with sheer number of regions considered.

3) Validation

Finally, the results presented in this manuscript are thoroughly validated against a series of independent in-situ data sets. These validations reveal the method's potential for improving our estimates of seasonal and, what's probably more important, inter-annual variability in distributions of other carbon parameters in the mixed layer. This method also seems appropriate to take on a more challenging task and try to estimate the distribution of carbon parameters below the ocean's mixed layer.

CRITICISM

I have only one general criticism. It is of formal rather than merit-based nature. Some information contained in supplements (14!) is critical to understanding of various aspects of the presented analysis. As much as I understand the authors' effort to streamline the manuscript by providing the more technical paragraphs in a form of a supplement, it seems to me that in this case the opposite effect is achieved. I had to switch between the two documents (main manuscript and the supplement) constantly, which definitely did not help the "flow" of the information. I leave it to authors and the editor to decide which elements that are currently supplements could be incorporated into the manuscript but I would strongly recommend looking into this issue for reader's benefit. In many cases it is simple cut and paste because specific supplement corresponds 1 to 1 with sections of the manuscript.

SPECIFIC COMMENTS

Page 15330, line 3:

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sparse – change to still insufficient. In general calling the current coverage of carbonate system parameters sparse seems inappropriate. It's greatest ever, achieved with enormous effort and still increasing. Yes, it's still insufficient for several purposes. I would look at this wording throughout the manuscript.

Page 15346, line 25 Please change the sentence to: For AT, the benefits of using SOMLO are much weaker, with deterioration of system's detection in some regions.

Table 5b Would recommend specifying negative improvement for Eq. Pac and North Atl.

In both cases above the authors should name appropriately what they show.

Page 15347, line 14 The word anomalies should be replaced by influences in my opinion. Authors exclude all coastal influences from their analysis and not only the anomalous coastal data.

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