

Interactive  
Comment

## ***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.***

**S.E. Mikaloff Fletcher (Referee)**

s.mikalofffletcher@niwa.co.nz

Received and published: 11 February 2013

The manuscript “A novel method for diagnosing seasonal to inter-annual surface ocean carbon dynamics from bottle data using neural networks” by Sasse et al. presents a novel approach to estimate mixed layer concentrations of total carbon and alkalinity using more widely collected observations of hydrographic data and nutrients. They used neural networks to develop a self organised map of waters with similar characteristics, and then use linear optimization to determine the best fit to the data for those waters. The approach is elegant and has been well evaluated using their global independence test and timeseries from fixed stations BATS and ALOHA. Therefore, I recommend

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



publication of this manuscript after minor revisions, as described below.

General comments:

Overall, I found this to be an excellent statistical treatment of an important global issue: improving our understanding of the spatial and temporal distribution carbon species in the surface ocean. However, while the paper was rich in explorations of the technique, it's errors, and potential biases, I found it quite thin on discussion of the end result. For example, two natural figures to include would be a map of the predicted DIC and Alkalinity from this approach next to maps of these values from the GLODAP gridded data set, and then a Takahashi pCO<sub>2</sub> map next to a map of pCO<sub>2</sub> calculated from this approach. This could have been accompanied by discussion of what can be learned from the higher resolution information about these tracers. Discussion of the predicted seasonal cycle would have also been exciting.

In the Introduction, the authors argue that their approach will help to address questions about inter-annual variability in the carbon cycle. This occurs in a few places, but especially P15333, paragraph 2. I agree that you will gain a wealth of information about spatial and seasonal variability from applying the algorithms developed here to World Ocean Atlas data. However, to the best of my knowledge, the World Ocean Atlas does not include information about inter annual variability. Please discuss what data would be used to look at this question or de-emphasize this point in the introduction.

There is a limited discussion of the complimentary efforts underway to use satellite and pCO<sub>2</sub> data to map sea surface pCO<sub>2</sub> (P15334, top). This should be expanded to discuss in more detail what has been done in this area, what the limitations of these techniques are, and what the SOMLO method contributes, and whether there is any scope for the two methods to compliment one another. This could be discussed partly in the introduction and partly in the discussion/conclusions.

While it can be quite beneficial for some of the more technical aspects of the paper to be included in the supplementary material, I felt that in this case some of the material that

**BGD**

9, C8107–C8111, 2013

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

was quite central to the paper was also included in supplements. In particular, I would recommend including all or at least part of Supplement E in section 4. The authors have included a short sentence meant to summarize Supplement E in section 4.1, but I have to admit that I could not understand that sentence until after reading the supplement. Likewise, a description of the geographic training parameters in Supplement I would have been helpful on page 15345.

The authors developed a Global Independence Test (GIT), which is a quite clever way of evaluating the predictive capability of the dataset without using the same data that was already used to constrain the problem but also without sacrificing a large amount of the available data for the final fit. While I quite liked the approach, it wasn't quite clear to me from the equation provided in section 3 exactly how the RSE was calculated. From the description, I have the impression that each cruise is, in turn, excluded from the optimization and used to evaluate the fit. Is an RSE calculated for each cruise? If so, how do the authors combine the RSE's from each cruise to get one number? Or is the numerator in Equation 1 summed over all of the cruises?

The authors use timeseries observations from two tropical stations, the Bermuda Atlantic Timeseries and the Hawaiian Ocean Timeseries (BATS and HOT) to constrain and evaluate their approach. It would be interesting to also test these results against the Munida timeseries (1998 to present), which samples Subantarctic Surface Water, just South East of New Zealand (Currie et al., 2009; Brix et al., 2013). This region is likely to experience quite different ocean biogeochemistry than the other sites that were tested and the timeseries includes all the required measurements.

Specific comments:

P.15330, line 11, No comma after ML\$.

P. 15330, line 19, comma after network.

P. 15330, line 26, no comma after complex, but comma after Pacific on the following

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

line.

P15332, line 12, replace has with have

P. 15333, line 7, no comma after Southern Ocean.

P. 15335, line 13, be a bit more explicit about the quality consistency (e.g. the quality consistency needed to directly combine data measured from different labs.)

P15335, line 17, no parenthesis around Tanhua et al., 2010.

P. 15335, line 19, please be more explicit about the methods used to remove questionable measurements.

P. 15335, line 3, comma after (Fig. 1b).

P. 15337, line 15, no comma after data

P. 15337, line 21, algorithms should be algorithm's.

P. 15339, line 18, gives should be give

P. 15341, line 16, comma needed after SOM

Pl. 15343, line 10, comma after GIT. Also, GIT has been previously defined and does not need to be re-defined again.

P15345, line 28, no comma after "zone"

I have to admit, I ran out of steam proofreading when I got to the end of the paper and the supplement, but an additional careful grammar check is needed here as well.

References:

Brix, H., K.I. Currie, and S.E. Mikaloff Fletcher (2013). Seasonal Variability of the Carbon Cycle in Subantarctic Surface Water in the South West Pacific, Global Biogeochemical Cycles, DOI: 10.1002/gbc.20023.

**BGD**

9, C8107–C8111, 2013

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Currie, K.I., M.R. Reid, and K.A. Hunter (2009). Interannual variability of carbon dioxide drawdown by subantarctic surface water near New Zealand. *Biogeochemistry* Volume 104, Numbers 1-3, 23-34. doi: 10.1007/s10533-009-9355-3.

---

Interactive comment on *Biogeosciences Discuss.*, 9, 15329, 2012.

**BGD**

9, C8107–C8111, 2013

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C8111

