**Biogeosciences
Discussions**

# *Interactive comment on* "Estimating the monthly $pCO_2$ distribution in the North Atlantic using a self-organizing neural network" *by* M. Telszewski et al.

**Anonymous Referee #3**

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This manuscript presents monthly North Atlantic pCO2 maps for the years 2004-2006 using a neural-network based combination of VOS-line observations and SST, Chl and MLD data derived from satellite observations. The topic presented by the manuscript is within the scope of Biogeosciences and the method as well as the results are of interest to a wide range of readers. This paper is an important step towards gaining reliable estimates of surface ocean pCO2 by combining sparse underway pCO2 measurements with satellite-based basin-wide data. The work is timely and will have an immediate and sustainable impact in biogeoscience community. The paper is fairly well written and the description of the method is thorough and clear. I recommend this manuscript for publication after some revisions.

My two major points of criticism are:

1. Coming mainly from the same project (CarboOcean) this paper can be regarded as the practical part following the methodological study published 3 month ago by Friedrich and Oschlies in JGR (F&O in the following). I assume that the presented manuscript was already with the numerous co-authors for approval when F&O was published. However, in the revised version of the manuscript the findings of F&O with respect to the basin-wide uncertainties of the pCO2 estimates need to be discussed. The text describing the method's uncertainty in estimating pCO2 is very confusing and the RMS-error of 11.55 uatm given in the abstract is really misleading. The pCO2 values memorized by the SOM are averages of the VOS-line pCO2 data. Thus, the given overall RMS-error represents a validation against a data-set that is at best semi-independent. F&O pointed out that this way of validation is not representative of the basin-wide error (see their Figure 9). In fact they found the basin-wide error to be about 3 times higher (including water depths < 500m and the Mediterranean and Labrador Sea where there were no data available). The comparison with the MV Santa Maria data remains unclear to me. What is meant by "absolute value of mean monthly residuals". Why not calculate the error as in equation 4 for having a validation against a truly independent data set that can be used for comparison with the results of F&O in order to get at least some first order estimate for the basin-wide accuracy? For a more indicative uncertainty estimate I highly recommend to take 2 of the 3 years for labeling the SOM and to validate it against the remaining year and repeating this for all 3 permutations. One main focus of the presented study should be to present an uncertainty estimate as reliable and as representative as possible rather than attracting attention with a low RMS-error that may not be realistic.

2. As I pointed out above I believe that this manuscript will be of great benefit for the biogeoscience community. Only, this benefit should be clarified to a broader scope of readers. Probably most readers are familiar with the necessity to better constrain the marine carbon uptake. So, what are the metrics of success for a basin-wide pCO2

mapping in the North Atlantic? How large is the uncertainty of the presented method with respect to CO2 uptake and it's interannual variability? Can we detect the anthropogenic impact on oceanic pCO2 with this method and this VOS-line coverage? Also it should be mentioned what the additional benefit of this study is compared to e.g. Lefevre et al. [2005] and Jamet et al. [2007]. At first sight their approaches result in similar uncertainties, although Lefevre et al. [2005] were not able to use Chl or MLD.

Specific comments:

1. Figure 3.

The figure is somehow deceptive as it shows the cumulative coverage instead of what is available monthly/seasonally. The great challenge the authors are confronted with is (besides the large pCO2 variability) the lack of coverage for pCO2 observations. This should be illustrated by the figure. e.g. Similar to Figure 6: (4 Seasons) x (3 years) (Also the black lines on the blue background are hardly recognizable.)

2. Figure 5

The density of the scattered points is not clear. The way it is shown as a contour plot in Figure 2 is much better.

3. The description of the methodology is very long. Maybe it would be enough to refer to Kohonen and Lefevre et al. [2005] and focus on the different labeling scheme used here.

4. Friedrich and Oschlies [2009] pointed out that depending on the mapping procedure (daily, monthly) there might by a considerable impact of remote sensing errors on the pCO2 estimates. MLD products are still subject to unknown (and probably high) uncertainties. How would a MLD-error of 5%, 10%, 25%, ... affect the pCO2-error?

5. Page 3398, line 21: Reference for Jamet et al. [2007] gives the wrong pages.

6. Page 3394, lines 4-19: I am not sure I understand the argument presented consid-

ering the impact of MLD on pCO2 in the Subtropics. How is the entrainment of DIC-rich water by a deeper MLD in the year 2006 balanced by a lower SST if the SST is virtually the same for all 3 years? In Olsen et al. [2008] (their Figure 9), I see a large impact of changes in MLD on pCO2 for the considered depth range right at the bottom of the euphotic zone. Also Jamet et al. [2007] find a positive coefficient for MLD for Winter in their multiple linear regression (their Table 2, last row)

7. Page 3378, equation 1: Lefevre et al. [2005] and Friedrich & Oschlies [2008] successfully used Latitude, Longitude and Time as additional input parameters for their SOM-based mapping. The latter ones reported that neglecting position leads to larger (about 5-10 uatm) RMS-errors (their Figure 8 + paragraph 32). Since Latitude, Longitude and Time are available 'for free' and have been shown to improve mapping accuracy why doesn't this study utilize them? Is it the different labeling scheme applied in this study that impedes the use of Latitude, Longitude and Time?

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