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# *Interactive comment on* "Estimating temporal and spatial variation of ocean surface $p\mathrm{CO}_2$ in the North Pacific using a Self Organizing Map neural network technique" *by* S. Nakaoka et al.

**S. Nakaoka et al.**

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Comment:Nakaoka and co-authors present a well-written and thorough overview of surface water pCO2 patterns in the North Pacific using a self-organizing map (SOM) neural network technique. This is the first time this approach is used for the North Pacific and it takes advantage of remotely sensed (SST & Chl) and modeled (SSS & MLD) parameters for the training step. They then use the extensive and high-quality dataset pCO2 from NIES to perform the labeling phase. Finally they use the SST, SSS, MLD and Chl to determine the temporal and spatial changes of surface water pCO2 over 6 years. The results are compared with the Takahashi climatology and SOCAT



database. The SOM shows very good correspondence but with higher fidelity and the approach has the ability to determine finer scale structures than seen in the climatology. The paper is clear and well-structured. It does a good job describing the general principles of the technique and it does a very thorough comparison of the results with other approaches. It ends with some comments on future efforts and possible improvements. It would be of interest if the authors provided some suggestions on the amount of pCO2 data that is necessary for labeling and how the uncertainty is related to the amount of labeling data. The quantification of the sources of uncertainty would be of interest although this might be a paper in itself.

Reply:

Dear Dr. Wanninkhof,

We thank you for your very useful comments. The sensitivity study required to provide a quantification of the relationship between the amount of the in-situ data and the method's uncertainty estimate is planned along the global mapping exercise which is our next goal. In this study we focused on relative comparison to other existing estimates in the region as indirect validation method. However, we added a paragraph in the summary of the revised manuscript hinting on this subject by describing the relative amount of empty neurons in this study and their meaning in terms of in-situ data coverage.

Comment:Page 4577 line 24: replace "current systems" with "current regimes".

Reply: Done.

Comment:Page 4578 line 14: place space between "high" and "latitude". Reply: Done

Comment:Page 4580 line 7 and elsewhere: in Takahahsi et al. 2006 they point out regions in the N Pac. where the ocean CO2 increases are slower than atmospheric increases.

Reply: We revised the manuscript pointing out the geographical variability of the in-

crease rate.

Comment:Page 4582 line 1: There needs to be some discussion on how well the assimilated MLD and SSS used for the training data match observations. I expect that SSS is measured along with all the pCO2 observations such that a large comparison set is available. Assuring that there are no biases in the training data is important.

Reply: We now provide a quantitative assessment of uncertainty related to the training data sets for SST, CHL and SSS in section 2.2. However we can't discuss the same for MLD because we do not measure this parameter in-situ and related uncertainty has not been yet reported.

Comment:Page 4584 line 1: the difference between fCO2 and pCO2 needs to be described as this will lead to confusion for some readers. Perhaps the values should be converted to a single unit (either pCO2 or fCO2) (although the difference is very small).

Reply: We agree. We changed/recalculated from fCO2sea to pCO2sea throughout the manuscript and corrected the description. The pCO2sea variations shown in figure 9 is also corrected.

Comment:Page 4585 line 9: Emphasize here that the MLD is expressed in log form in training and that log 200 and log 400 m are close in magnitude.

Reply: We added the sentence to emphasize it in the last paragraph.

Comment:Page 4585 line 26: give the magnitude of "negligibly small".

Reply: We quantified the difference.

Comment:Page 4586 line 12: it is of note that the RMS of 18 uatm is fairly large for some applications (e.g. determining regional sea-air CO2 fluxes)

Reply: We pointed to this fact in the revised manuscript

Comment:Page 4587 line 19: Is 37 N, 140 W a region or location- If region, specify



approximate size.

Reply: We specified the region size.

Comment:Page 4588, line 7: see comment above.

Reply: We specified the region size.

Comment:Page 4580 line 7. Page 4589, line 5 and beyond: spell out acronymns in text the first time they are used e.g. KE, WST, VOS

Reply: Done.

Comment:Page 4590, line 19: Is this a universal conclusion or does it apply to certain regions only? It would appear that a mechanistic understanding of controlling processes would be important to back this up.

Reply: By now we think that this conclusion applies globally. We precised our statement by pointing out the fact that training procedure is the key here and this is thoroughly described in this manuscript as well as in Telszewski et al 2009.

Comment:Table 1. Perhaps mention in caption that these data are used for labeling.

Reply: We agree. We added the explanation as you pointed.

Comment:Figure 4. Perhaps list the total number of data points in this figure.

Reply: We added the number of data points in the figure.

Comment:Figure 6 and 8 (and also in text): Why is 2-sigma used as an indication of spatial variability? I've generally seen 1 or 3 sigma.

Reply: We changed from 2-sigma to 3-sigma as you indicated.

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Interactive comment on Biogeosciences Discuss., 10, 4575, 2013.