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Interactive comment on "A global monthly climatology of oceanic total dissolved inorganic carbon: a neural network approach" *by* Daniel Broullón et al.

Anonymous Referee #3

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The authors provide a nice description of global TCO2 distribution based on monthly climatology from a neural network approach. The addition input variable "year" is reasonable and important for TCO2. It is great to see that the neural network outputs match very well with the measured TCO2 for the independent time series locations. This manuscript is well organized and easy to follow. I would like to see the publication of this work.

Below are specific comments.

Lines 71-74, include the influence of mixing on TCO2 variability. For example, upwelling-induced increase in TCO2, which is described in lines 83-86.

Lines 75-76, the temperature and salinity influence on TCO2 through the modification of the solubility of CO2 affects the seawater pCO2 (which is almost instantaneous) and thus the air-sea CO2 flux, which eventually drives the change in TCO2 over time.

Line 131, delete de in "minimize de errors"

Lines 142-143, move the full list of the input variables to line 153 to make it ahead of the sentence, "In addition to the target variable. . ."

Line 147, specify which generalization method was used to prevent overfitting.

Line 258, provide how many hidden layers are used in the neural network

Line 263, the errors cannot be avoided in any case. It is likely that a much smaller weight is assigned to a variable that contains a large error.

Line 276, provide how depth-weighted RMSE is calculated

Lines 349-351, the error may come from the bias in total alkalinity. I agree that the error involved in the calculation of pCO2 from AT and TCO2 can be large, but the monthly averaged error is another case. In addition, the choice of K1 and K2 affects the calculated pCO2. Lueker et al. (2000) is a better choice in the case of calculating pCO2. Provide the bias information for total alkalinity in each region in Table 4.

Line 420-430, Fig. 9, the differences in the surface water at BATS in May, June, July, and August seem large as compared to the values in other months. Provide an explanation.

Line 445-448, the bias for pCO2, considering it is the averaged value over space and time, seems large as compared to the bias in Landschutzer et al. (2017) as shown in Table 4. Therefore, I don't agree that climatologies of other seawater CO2 system variables can be confidently computed. Furthermore, this statement compromises the value of this study if TCO2 can be confidently computed from pCO2 and AT.

Fig. 2, color-coded depth instead of the log10 frequency would provide more informa-

tion regarding the distribution of errors. Fig. S4, add another panel with a similar generated from measured bottle TCO2 data. Furthermore, add one more panel to show the differences between the derived TCO2 and the bottle TCO2. The differences are partially shown in Table 2 with the average differences by region. An additional panel regarding the differences would help to determine the reliability of this new dataset on a smaller scale (e.g., 1 degree by 1 degree).

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