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Biogeosciences Discussions

Interactive comment on "Role of regression model selection and station distribution on the estimation of oceanic anthropogenic carbon change by eMLR" *by* Y. Plancherel et al.

Anonymous Referee #4

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Monitoring the ocean carbon sink requires accurate estimates of changes in spatial and temporal patterns of anthropogenic carbon uptake by the ocean. The extended Multiple Linear Regression (eMLR) approach is commonly used to estimate anthropogenic carbon uptake from hydrographic datasets. Application of the eMLR approach requires the user to select a suite of physical and biogeochemical parameters to use as predictor variables. However, there has been no standardization as to which variables are used. The authors provide a much needed comprehensive evaluation of eMLR regression models. Using a global biogeochemical model as a synthetic dataset, they evaluate the impact of MLR parameter selection, sampling densities, and seasonal and vertical variability on estimating anthropogenic carbon accumulation rates.

I feel this paper contributes significantly to the field and should be published. However, there are a few aspects where the analyses and conclusions of the paper could be strengthened. This would broaden the audience of the paper and make the findings more applicable to field studies.

1. While this paper provides an in-depth analysis of different regression models and the impact of models with different numbers of parameters, I feel that the discussion and conclusion are in need of some stronger statements as to the findings of these analyses. Specifically, how can field studies use these findings to determine optimal eMLR regression models? The authors allude to this on page 14613 and in the discussion but the reader comes away unsure how the findings from this study could be directly applied to field data where the 'true uptake' rate is unknown and so a rigorous evaluation of regression models is difficult.
    (a) Is there a relationship between the MLR residuals (or AIC values) and the delta Inventory relative error? This analysis would be simple to add as the data already are presented in Figures 4 and 6.
    (b) The authors find that regression models with 7 parameters are often selected. However, for many observational datasets this large number of parameters is not available. Could the authors expand on the analyses presented on page 14605 and 14606 to show (perhaps with an additional figure or table) which parameters are most commonly selected for and so should be a high priority for programs such as CLIVAR? Similarly, the authors mention that some parameters often replace each other such that it would not be necessary to include both in a regression model. Could this information be expanded upon so that all of these related groups are presented to the reader instead of just a select few?
2. The authors present and analyze two eMLR strategies in addition to a combined

strategy but do not clearly state which strategy they find evidence for being the preferred strategy. Similar to my comment #1, this leaves the reader unsure how these findings could best be applied to other studies. The comprehensive analysis done by the authors should make a set of guidelines for 'strategy selection' easy to provide to the reader.

(a) Goodkin *et al* 2012 describe the theory behind the eMLR approach, specifically that by applying a regression model at both t_1 and t_2 the MLR residuals cancel each other out. It would be interesting to see whether this assumption holds for strategy 1 where two different regression models with different variables are used. One would assume that different regression models would produce different residual patterns. This could explain some of the differences in the ΔC_{anthro} estimate.

(b) It is not clear why the results from strategy 1 are displayed in a different format (Figure 5) from the results from strategy 2 (Figure 6). It would be helpful to have directly comparable figures, if only in the supplemental material.

3. The authors do not comment on the possibility of model drift explaining some of the observed patterns especially in the control run (page 14602). The spin-up period for these model simulations was relatively short and so model drift, particularly in the deep ocean, would not be unexpected. This should have a relatively small impact on the primary analyses of this paper however it is an important limitation of the dataset that the authors used and should be mentioned.

Minor comments:

Abstract: I felt the abstract does not do this paper justice. I would suggest shortening the sentences to make it easier to read, providing a motivating first sentence, and providing more specific conclusions.

Pg 14592 line 11: "proposed to compare empirical" is not grammatically correct. Perhaps 'proposed the comparison of"

Pg 14592 line 18: "In preparation to an application of the eMLR approach to global data sets" needs to be reworded

Pg 14594 line 7: The following wording is difficult to understand: "predictions obtained from a model obtained from one data set"

Pg 14601 line 26: There is significant evidence from multiple studies that mode waters take up anthropogenic carbon in the subtropics (added to the list provided by the authors is an observational based analysis by Bates *Biogeosciences*, 2012 and a modeling analysis by Levine et al *Global Biogeochemical Cycles*, 2011). It seems that the lack of uptake by mode waters in this model represents a significant limitation that should be pointed out.

Pg 14603 line 15: "Observations" or "field data" might be more appropriate terms than "real data".

Pg 14621 line 5 "regressio" should be "regression"

Pg 14622 line 6: "the variance is associated to a seasonal cycle" should be "the variance is associated with a seasonal cycle"

Figure 2 & 3: I find these figures very difficult to read. I would recommend removing the background colors and displaying them as a colorbar on top of the figure as done in Figure 6. Could the authors provide an appendix table which lists which parameters were used for each model number to allow for further interpretation of these figures?

Figure 4 caption: The magenta bars are almost impossible to see.

Figure 6: Can the authors indicate the 'best fit' model for each model group?

Interactive comment on Biogeosciences Discuss., 9, 14589, 2012.