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Interactive comment on "Climatological variations of total alkalinity and total inorganic carbon in the Mediterranean Sea surface waters" *by* E. Gemayel et al.

Anonymous Referee #2

Received and published: 25 September 2015

Review of: Climatological variations of total alkalinity and total inorganic carbon in the Mediterranean Sea surface waters

By: E. Gemayel, A. E. R. Hassoun, M. A. Benallal, C. Goyet, P. Rivaro, M. Abboud-Abi Saab, E. Krasakopoulou, F. Touratier, and P. Ziveri.

General comments The authors have compiled CO2 system measurements from 14 cruises in the Mediterranean Sea surface waters. These were then used to constrain basin wide, improved empirical algorithms for both alkalinity (AT) and dissolve inorganic carbon (CT) using salinity and temperature as the independent variables. The newly identified relationships were then applied to WOA climatology to evaluate the spatial

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and seasonal variability of the carbon system in the Mediterranean Sea surface waters. Thus, the authors contribute with an improved way to utilize the more abundant data of salinity and temperature, for instance, for estimating the exchange of CO2 across the air-sea interface or for the validation of model results etc.

The manuscript is well structured and adequately written (for suggested improvements see “specific comments” below) and I find only few minor issues. I recommend publication after minor-moderate revision according to the following comments.

The authors mention their use of both sea surface temperature (SST) and sea surface salinity (SSS) as regression parameters improves the statistics of the estimated CT and AT values, and that SST and SSS explain most of the variability in AT (96%) and CT (90%). This indicates differences in the processes driving SSS and SST compared to AT and CT. Thus, readers may wonder how similar (or dissimilar) are the SST and SSS distributions compared to those presented for CT and AT? Therefore, the authors should consider presenting SSS and SST distributions as well.

The authors use CT data that has been measured over a period of fifteen years (1998-2013), but they do not account for any systemic CT trend. The reason for this is, they argue, (i) the anthropogenic signal is concealed by measurement uncertainties and seasonal variations, (ii) including the small observed CT trend results in an insignificant change in their results, and (iii) in regions above 30N latitude the outcropping of deep isopycnal surfaces dilutes anthropogenic CO2. The last point represents an outdated view. Firstly, surface CT trends do not need to arise only from local uptake of anthropogenic CO2, but transport of both natural and anthropogenic carbon can also produce trends (e.g. Perez et al 2013). Secondly, several recent studies have actually shown significant anthropogenic CT concentration (e.g. Waugh et al 2004; Sabine et al 2004) as well as pCO2 increase (Takahashi et al 2009) in the surface in areas north of the 30N. Furthermore, I think statement (iii) above is not really essential for the manuscript and, thus, I would suggest removing it altogether.

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Specific comments Abstract: Line 2 (and throughout the manuscript), “total inorganic carbon (CT)” should be “total dissolved inorganic carbon (CT)” in accordance with Best Practices for CO2 measurements (Dickson et al 2007).

Line 6 - 7: “The AT surface fit showed an improved root mean square error (RMSE) of. . ..” Improved compared to what?

Line 13 - 14, the word “surface” should be deleted since the whole study is treating only surface data. Actually, throughout in the manuscript “surface” should be used only if necessary because emphasizing this word can give the false impression that there are subsurface data included in the study.

Line 11-14, please mention that the climatology were mapped using the identified empirical equations.

Line 17, “repartition” do you mean distribution?

Line 17-19, “.. primarily due to the deepening of the mixed layer and upwelling of dense waters”. I do not find any evidence supporting this statement in the manuscript. Please substantiate or otherwise provide references.

Methods: Page 1504, line 6-7: “However, the number of the nutrients concentrations was very limited.” why is this relevant here?

Line 26, “Hence for the AT, 375 and 115 data points are used for the training and testing” I understand the testing dataset is from the cruises where AT was measured without accompanying CT, right? If no, then the necessity of holding out some data for validation purposes should be discussed. In either case a clarification is needed.

Page 1504, line 1-2 “. . . and the validation dataset is the same as the testing subset of the 10th fold (45 data points).” I thought the 10th fold procedure means that you divide your dataset randomly into 10 equal parts. But 45 is not exactly one tenth of 381 or 426! Can you please clarify this point.

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Page 1506, line 6 “global” should be replaced by more appropriate word like “general”, “representative” etc.

Results and discussion: Page 1507, line 22 “contribute to” should be replace with “explain”

Line, 26-27 “In fact, the interpolation of CT in the mixed layer..” what interpolation?

Page 1508, line 21-24 The general comment about dilution of anthropogenic carbon in the surface water in areas north of the 30 latitude is unnecessary and somewhat misleading (see “general comments”).

Page 1509, line 11-15. Both pCO2 and CT are mentioned. Please be consequent, and comment only CT variations. Remember pCO2 can change even under constant CT!

Page 1511, line 11-20. I’m not sure if the authors argue for low AT or high AT values in the Adriatic and Aegean sub-basins. Please clarify.

Tables & Figures:

Table 1, please consider including number of data points and area. Figure 1: please consider to indicate the locations of important geographical features named in the text.

Literature referred to in my comments: Waugh, D. W., T. W. N. Haine, and T. M. Hall (2004), Transport times and anthropogenic carbon in the subpolar North Atlantic, Deep Sea Res., Part I, 51, 1475– 1491.

Sabine, C. L., Feely, R. A., Gruber, N., Key, R. M., Lee, K., Bullister, J. L., Wanninkhof, R., Wong, C. S., Wallace, D. W. R., Tilbrook, B., Millero, F. J., Peng, T.-H., Kozyr, A., Ono, T., and Rios, A. F.: The oceanic sink for anthropogenic CO2, Science, 305, 367–371, 2004.

Takahashi et al 2009. ClimatologicalmeananddecadalchangeinsurfaceoceanpCO2, and net sea–air CO2 flux overtheglobaloceans. Deep Sea Res II, 56, Pages 554–577.

Perez et al. Atlantic Ocean CO2 uptake reduced by weakening of the meridional overturning circulation. Nature Geoscience 6, 146–152 (2013).

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1499, 2015.

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