

Response to Referee #2 Comments on Gemayel, E., Hassoun, A.E.R., Benallal, M.A., Goyet, C., Rivaro, P., Abboud-Abi Saab, M., Krasakopoulou, E., Touratier, F., Ziveri, P., 2015: Climatological variations of total alkalinity and total inorganic carbon in the Mediterranean Sea surface waters. Earth Syst. Dynam. Discuss. 6 (2), 1499-1533. 10.5194/esdd-6-1499-2015

P.S: Original referee comments are in normal font; our replies are in italics. Intended changes to text are shown in bold font.

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 dissolved 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 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.

We would like to thank the referee for their thorough comments, suggestions and criticisms. The points raised by them helped us to improve our manuscript.

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.

We added figure 6, showing the SSS and SST climatological fields, 7 years averages of the WOA 2013.

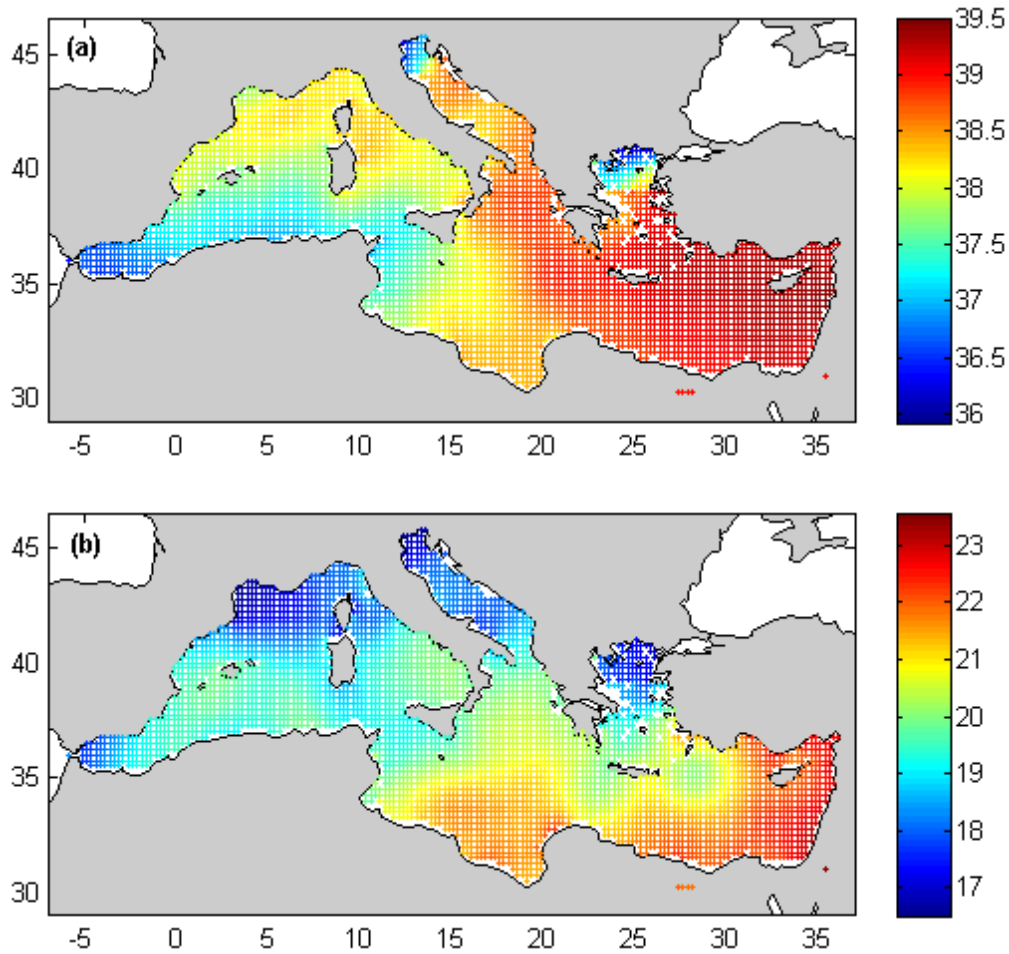


Figure 1. The seven years averages (2005-2012) of (a) SSS and (b) SST climatological fields of the WOA13 (Locarnini et al., 2013; Zweng et al., 2013)

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.

Statement (iii) was removed from the manuscript

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).

This was corrected accordingly

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

The sentence was corrected as follows:

The A_T surface fit yielded a root mean square error (RMSE) of $\pm 10.6 \mu\text{mol.kg}^{-1}$, and where salinity and temperature contribute to 96% of the variability

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.

Done

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

This was added in the abstract according to:

The identified empirical equations were applied on the quarter degree climatologies of temperature and salinity, available from the World Ocean Atlas 2013.

Line 17, “repartition” do you mean distribution?

Yes. The term repartition was replaced with ‘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.

This statement was deleted

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

Originally we wanted to use also nutrients data. But because these measurements are very scarce we did not opt for this option. In all cases this sentence was deleted

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

The testing dataset is from cruises where A_T was measured without accompanying C_T , as well as the testing subset of the 10th fold

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.

The number of data points was revised and corrected. We have 490 and 400 data points for A_T and C_T , respectively. For the training dataset we choose 360 data points where both parameters were measured. Hence for A_T and C_T , 40 data points remain for the validation. Furthermore we add for A_T data points where the latter was measured without accompanying C_T , yielding 90 data points. We rewrote this section as follows:

The dataset consists of 490 and 400 data points for A_T and C_T , respectively (Table 1). To ensure the same spatial and temporal coverage of the polynomial fits, the same training dataset was retained for both A_T and C_T . This was performed by selecting stations where both parameters were simultaneously measured; yielding 360 data points (Figure 1). To validate the general use of the proposed parameterizations we tested the algorithms with measurements which are not included in the fits (Validation dataset). For A_T , the validation dataset consists of 130 data points which are formed from the testing subset of the 10th fold (40 data points), and from cruises where A_T was measured without accompanying C_T (90 data points). For C_T , the validation dataset is the same as the testing subset of the 10th fold (40 data points).

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

We couldn't find 'global' on page 1506, line 6. We found 'global' on page 1506, line 15 and replaced it with 'general'. We also found 'global' on page 1507, line 4 and replaced it with 'representative'

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

Done

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

This was replaced by:

In fact, estimating C_T in the mixed layer adds a high uncertainty due to the seasonal variability

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”).

This statement was deleted

Page 1509, line 11-15. Both pCO_2 and CT are mentioned. Please be consequent, and comment only CT variations. Remember pCO_2 can change even under constant CT!

All the discussion concerning pCO_2 was removed

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

We argue for high A_T values in the Adriatic and Aegean sub-basins. The sentence was rephrased as follows:

Hence Eastern marginal seas, such as the Adriatic and Aegean sub-basins have high A_T concentrations due to the freshwater inputs

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.

Done

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. Climatological mean and decadal change in surface oceanp CO₂, and net sea–air CO₂ flux over the global oceans. *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).

Thank you for your suggestions. We consulted again these articles