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Biogeosciences Discussions

Interactive comment on "Anthropogenic and natural CO_2 exchange through the Strait of Gibraltar" *by* I. E. Huertas et al.

I. E. Huertas et al.

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This reviewer has made a strong effort reporting large amount of details and a long report. Most of the comments are focused in three aspects of the paper that can be summarized in three points:

1. The utilization of the TrOCA approach for CANT computation. 2. The seasonal variation of the studied parameters. 3. The necessity to include the uncertainties associated to the calculations.

With respect to the first point, we agree with the fact that the TrOCA approach is not the most satisfactory method to calculate CANT in marginal seas, such as the Mediterranean. However, it was used in our study in order to compare our results with others that have been previously reported in the Strait of Gibraltar using the same technique

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Interactive Discussion
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and that constituted the first data on anthropogenic carbon in the area. The aim of this paper was not to criticize the TrOCA approach but only to provide a vision of the different results that could be attained depending on the method considered. Following the reviewer s suggestion, we have made clear in the discussion section that the TroCA approach could be revised for the Mediterranean Sea (or for any other marginal sea and coastal analysis) but the objective of this work was not to report possible caveats associated to the TrOCA and the technique was applied as it is formulated.

Regarding the second aspect, our work provides for first time the combination of measurements of the carbon system parameters obtained in situ and values of the water transport recorded concurrently at the Strait of Gibraltar during the entire sampling period. Both variables are submitted to a completely different frequency of variation, which can lead to generate aliasing when considered together at a short temporal scale. From our data, it is evident that the major source of short term variability in the exchanges through the Strait is associated to the changing Mediterranean water transport. Therefore, a powerful examination of the seasonal variability would require to perform a sampling of a higher frequency than that used in our study. In any case, the analysis of the nine campaigns conducted allows to enhance the estimations of carbon fluxes obtained up to date due to two reasons: a) a better assessment of the average water transport and its associated uncertainty is achieved and b) a better determination of the average carbon concentration and its uncertainty is also reached. The analysis of the seasonal cycles, if present in the area, was not the goal of our work since we want to provide annual fluxes that can be considered at a long scale. Finally, we deeply thank the reviewer for having noticed extensively that the uncertainties were not mentioned in the text. They had been included in Table 2 in the original manuscript but now they have been also interchanged throughout the revised paper accordingly.

Specific comments: I8217;ll start with several comments concerning the abstract and the conclusion; both should always present the main results of the work. I then list several issues that should be revised in this manuscript. C1: Abstract: lines 1-3: Authors

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indicate this study aims at describing the anthropogenic and natural inorganic carbon... but the study presents total CT budget (CT,Cant and Cnat) as well as TA budget. As the manuscript also presents budget for TA, which is discussed page 1045-1047 (and I like this part of the paper) this should be include in the abstract (and conclusion). This is a comment also related to the title of the paper (suggestion for title Total carbon and alkalinity exchanges through the Strait of Gibraltar).

Alkalinity results have been added in the abstract and conclusions although we do not consider necessary to alter the title of our work.

Concerning the Cant estimates, it is not clear if the methods used (Troca and C*) can evaluate Cant in the surface Atlantic waters and so authors should explain this more clearly as they derive the Cant net exchange at the strait and not just the Cant outflow from Med-waters.

ADDRESSED ABOVE

C2: Abstract: lines 4-7: A comprehensive sampling program was designed.... to ensure a good spatial and temporal coverage. Authors never say a word about temporal variability. The CT/TA data are used here as an average of all cruises (9). But authors have in hand, for the first time I guess, temporal coverage to estimate the seasonal (interannual) budgets of CT/TA in this region. Does the temporal variations of CT/TA concentrations lead to large (or small ?) differences in CT/TA budget calculations. Previous analysis (Copin-Montegut, 1993; Ait-Ameur and Goyet, 2006) used single cruise, when you have 9 cruises, for which a CT/TA seasonal budget could be derived. It would be very interesting to calculate and discuss the budget for each cruise and demonstrate if this is or not important to take into account the variability of the CT/TA concentrations in both layers (surface and deep). Also from such temporal calculation it is possible to add an uncertainty in the carbon budget of 0.025 PgC/yr (needs an error to this number in the abstract and conclusion).

ADDRESSED ABOVE

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C3: Abstract: lines 7-10: As a result of this monitoring....generation of an extensive data set of the carbon system parameters in the area. I don8217;t understand this point in the abstract. The present analysis does not present the extensive data-set; instead the manuscript only presents and discusses the average of the data (CT, TA, O2, Flow, ..) acquired over the period 2005-2007.

As shown partially in Figure 4, the data set of the carbon system parameters has been indeed generated, which can be extended to the water transport records displayed in Figure 2. In addition, the totality of the bulk of data has been used to calculate mean values in each station, as illustrated in the section plots (Figs. 3 and 5).

C4: Abstract: lines 10-11: Data acquired during the development of nine campaigns were analyzed in this work. Again this is not true; authors never describe in the manuscript the temporal variability of the carbon system properties; but I strongly suggest authors to describe the 9 cruises. This would really show new results.

ADDRESSED ABOVE

C5: Abstract: lines 12-13: ... with the concentration of anthropogenic carbon (Cant) being also computed. You should specify in the abstract that you used different methods to derive Cant (and apparently you have selected one, C*, to conclude on Cant budget).

CORRECTED

C6: Abstract: lines 13-17: .....net export of inorganic carbon.....which amount for 0.025 pgC/yr. Need uncertainty attached to this value listed in the abstract (same for conclusion). Again, the TA budget should be also indicated in the abstract.

ADDRESSED ABOVE

C7: Abstract: lines 17-19: In contrast (???), the Atlantic water...resulting in a net flux of Cant towards the Med basin of 4.2 TgC/yr. Need uncertainty attached to this number. This also suggests that you believe the C* method is more appropriate than Troca also

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used in your analysis. This has to be stated clearly in the abstract, discussion and conclusions.

ADRESSED ABOVE

C8: Abstract: lines 21-24: ..... influence of Med basin in the carbon inventories of the North Atlantic. Well, are the results important for the North Atlantic or for the Med Sea (or both). If one is interested in the net total CT budget, authors suggest the net export towards the Atlantic is 0.025 PgC.yr (need uncertainty) and consequently this has implication in the north Atlantic carbon budegt. For the Cant budget authors suggest the Atlantic surface waters import significant Cant in the Med-Sea. If this is true (regarding the difficulty to determine the Cant concentrations in surface waters) the main result of this work would reflect a significant import of Cant in the Med-Sea and consequently a more rapid increase of CT in this basin with potential implication on pH and acidification. This topic (acidification) is presented in the introduction (this is somehow an hot topic), but never commented or discussed later in the manuscript.

We acknowledge the reviewer s analysis, and the paragraph regarding acidification has been deleted from the introduction.

Now I come to the Conclusions: C9 Page 1050, line 21-26: this is like an introduction

We agree with the reviewer that the conclusions may seem reiterative but as BG requires to include such section, a summary of the entire work has been performed within this part and consequently, it must contain a brief introduction, which consists in presenting the area, a synopsis of the results and the main goals achieved, aspects that have been obviously described earlier in the manuscript.

C10 Page 1050 lines 21-22, Conclusion starts: The strait of Gibraltar constitutes the only connection of the Med-Sea with the Atl Ocean. On lines 26-27authors write results... highlight the significance of the strait as a key area for evaluating interactions between both oceanic regions.... this is obvious.

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Once again, we agree with the reviewer s point but this statement may not be so obvious for those who are not familiar with this geographical area and that is why the relevance of the Strait of Gibraltar as a controlling oceanographic spot has been emphasized.

Authors also indicate line 27-28... particularly in the current context of global change. This is ambitious and ambiguous words. Could you explain why the transport of carbon accross Gibraltar is so imporant for global change (or do you mean change in the global Mediterranean Sea ?).

The necessity to study the transport of carbon across Gibraltar was recognized by the international scientific community when it was included in the European integrated project CARBOOCEAN as a hot spot to be evaluated in the context of the carbon uptake and release at European regional scale and its influence in the carbon inventories of the North Atlantic. We kindly refer to the reviewer to the official web site of the project (www.carboocean.org).

C11 Page 1051, lines 1-5: The extensive analysis performed using data gathered during the development of nine cruises..... provides a comprehensive view of the carbon exchange in this area. Well, again, authors did not present the nine cruises and they consider that the average as presented here could provide a comprehensive view of the carbon exchange in this area. The study should first present seasonal/interannual analysis of the nine cruises before reaching these conclusions.

ADDRESSED ABOVE. Besides, following the reviewer s suggestion, references to the nine cruises have been reduced in the text.

C12 Page 1051, lines 5-6 authors say they derive a net export of inoganic carbon equivalent to 0.025 PgC/yr. Need uncertainty on this number.

DONE

C13 Page 1051, lines 6-7 authors say an annual flux of 4.2 TgC/yr Cant towards the

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Medsea can be detected; again you need to add an uncertainty with this number and specify in the conclusion which method (TroCA, C*) you used to derive this conclusion.

DONE

C14 Page 1051, lines 8-14. Authors conclude on the importance of maintening GIFT timeseries etc..... However, if they want to justify this, they should demonstrate how it is important to conduct sesaonal cruises over several years. In the context of GIFT experiment and in order to justify the reoccupation of this time-series in the future, this would be very new and attractive result to discuss: is it or not important to conduct cruise each year, or would it be better to conduct only two (one ?) cruise with better coverage ? I think authors have an incredible data-set to investigate this question. This is however not presented in this analysis and I really would recommand to discuss this point. If not authors need to justify in the paper why they are not documenting the AT/CT temporal variability and thus why it is not important to conduct seasonal/interannual cruises in the future.

Such analysis is being performed currently in the frame of other works that consider more variables other than the ones shown in this study, which are being the subject of a thoroughly temporal examination at the moment. The need to keep the sampling of the GIFT timeseries is indicated in the conclusion section as we believe it can be used as a monitoring tool to assess potential changes in the long term at a large temporal scale.

Other comments C15 Introduction is very long. After a global CO2 context, the paper could start with the uncertainties associated to carbon inventories in marginal seas etc... (Borges et al 2006).

Introduction has been shortened.

C16 Page 1024 lines 1-7: references listed are not all based on back-calculation technique (e.g. Siegenthaler and Sarmiento 1993; Takahashi et al. 2002).

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Interactive Comment

CORRECTED AS FOLLOWS:

"Gruber et al. (1996) improved later this method by adding the quasi-conservative tracer C*, derived from the estimate of the water mass age from tracers such as CFC-11, CFC-12, tritium (3H) and helium (3He). A recent comprehensive analysis based on this method (ΔC* hereafter) has evidenced that the oceanic sink has accounted for 48

C17 Page 1031, line 10. What is the accuracy of TA measurements.

ADDRESSED ABOVE

C18 Page 1033. Please explain how you derive the Cant in surface waters.

EXPLAINED IN THE MATERIAL AND METHODS SECTION

C19 Page 1034. Could you explain why you are using the new TrOCA when Ait-Ameur and Goyet used the former TroCA for Cant extimates in Med outflow waters (and not in surface because this method as any back calculation cannot be applied in surface waters).

When the study of Ait-Ameur and Goyet was released back in 2006, the new TrOCA had not been proposed yet. In the later study by Touratier et al, new elements were added to the tracer and recommended to be used thereafter by the authors.

C20 Page 1034. Could you explain why you use TroCA0 as derived from deep global ocean data (Touratier et al 2007) and not fitted here with regional and Med data.

ADDRESSED ABOVE

C21 Page 1034. Could you specify why you are using different Redfield ratio for C* and TroCA; this could lead to the large differences as observed in Cant concentrations.

ADDRESSED ABOVE. Unfortunately, each method has its own Redfield ratio. Sorry, this manuscript is not the place to assess this issue.

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C22 Page 1037, lines 4-5. Refering to previous studies, authors indicate that seasonal variability of the fCO2 in surface water may contribute to the variations registered in the surface layer. Why not showing your data, e.g. fCO2 caculated from pH,TA for all cruises. You have 9 cruises to show how important is the fCO2 seasonal variability.

The sentence has been deleted.

C23 Page 1037-1037. Authors compare their data (TA/CT) with previous studies. In these lines authors seem to justify their measurements because they are consistent with previous analysis. However, I would expect they obtain different results, because they sampled different years and seasons. If not, it may be not so important to conduct seasonal cruises to reduce the uncertainty in the carbon and TA budgets.

ADDRESSED ABOVE.

C24 Page 1038, line 2-23, I don8217;t understand these lines; they should be deleted. The text would be clearer (moving from a description of TA data and presenting the TA/S relation).

This paragraph tries to underline the importance of the participation of the biology in the concentration of carbon in the surface layer as compared to the deeper layer. Although the contribution of carbon consumption by phytoplankton in the photic zone was not assessed in this study, it is supposed to play a role in the withdrawal of carbon in the upper layer (Macías et al 2009)*, which would be also a factor that reduces the levels of carbon with respect to the Mediterranean layer in which this mechanism is not expected to occur. *Macías D, Navarro G, Echevarría F, Bartual A, Huertas IE (2009) Primary production in the Strait of Gibraltar: carbon fixation rates in relation to hydrodynamic and phytoplankton dynamics. Estuarine Coastal and Shelf Sciences (in press)

C25 Page 1038 line 17; if you still refere to Copin and Begovic, check names. (Begovic).

CORRECTED

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C26 Page 1039-1040; Cant concentrations derived from TroCA are really very high, 81 umol/kg for your calculation, 111 umol/kg when quoting Ait-Ameur and Goyet (2006). This is more than 1 umol/kg/yr since 100 years ! Do you really believe in these very high concentrations. If not, you should indicate this calculation is not correct and explain why (e.g. TA0, method not applicable in shallow waters etc...)

The rate of CANT uptake is not constant and has been increasing from 0 to the current 0.8 μmol/kg/yr from the preindustrial era. In any case, this manuscript is not the place to assess issues related with the basis of the CANT methods.

C27 Page 1042, Authors used a direct appoeach to estimate the carbon and alkalinity budget. However, other methods (indirect) have been developped to estimate heat, salt, metal or carbon budget in straits, and some taking into account the seasonal variability (e.g. Souvermezoglou et al 1989; Van Geen and Boyle, 1990). Authors have everything in hand to apply such indirect method on both seasonal and interannual scales The first paper is devoted to the Red Sea, which shows a more complicated water exchange with the open ocean and the second paper does not focus on seasonal variations. Neither does ours.

C28 Page 1043, lines 7-9, please add a reference of the numerical model.

The numerical model is contained in the paper by García-Lafuente et al. (2007) and it has been specified in the text.

C29 Page 1044, lines 6-14, to delete (very general), not usefull for the discussion.

DELETED

C30 Page 1044, lines 19, your estimate of 2.11 TmolC/yr is not in the range of previous estimates, but on the low side. Need to add uncertainty to this value.

ADDED

C31: Page 1060, table 1, need uncertainty attached to TA/CT average values.

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ADDED

C32: Page 1065 Figure 5. As TrOCA seems to lead to ambiguous results for Cant, I think you can delete Figure 5A (Figure 6 clearly show the strange TrOCA profiles with a maximum of Cant in deep layer). Instead, and because water column distribution of Cant (using C*) is homonegeous along the section (Figure 5B), it would be nice to show the Cant distribution (from C*) for all cruises as a function of time (e.g. using average of station 1 to 5 for each period).

With respect to the first point, Figure 5A is useful to compare the data in the section plots and provides the reader with a good picture of the different situation that can be attained depending on the approach considered for CANT computation.

Interactive comment on Biogeosciences Discuss., 6, 1021, 2009.