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Interactive comment on “Anthropogenic and natural CO₂ exchange through the Strait of Gibraltar” by I. E. Huertas et al.

Anonymous Referee #2

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Review of the manuscript Biogeoscience MS-NR: bgd-2008-0217 Version: 1 Title: Anthropogenic and natural CO₂ exchange through the Strait of Gibraltar Author(s): I. Huertas, A. Ríos, J. García-Lafuente, A. Makaoui, S. Rodriguez-Galvez, A. Sanchez-Román, A. Orbi, J. Ruiz, and F. Perez

Decision: This manuscript is acceptable for publication after major revisions

General comment:

This paper presents new data of carbon (CT) and alkalinity (TA) in the Strait of Gibraltar (the boundary between the Atlantic Ocean and Mediterranean sea) based on several seasonal cruises conducted in 2005-2007. To estimate CT and TA net budgets at this boundary, authors use direct observations of the Mediterranean outflow for the

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same period (2005-2007). They also use two diagnostic methods to evaluate the concentrations of anthropogenic CO₂ (Cant) in the Atlantic and Med-waters and derive a net budget of Cant. Authors suggest that Atlantic waters import more Cant than it is exported from the MedSea. This result contradicts previous analysis (Ait-Ameur and Goyet, 2006) that suggested Cant concentrations as high as 111 $\mu\text{mol/kg}$ in Med-waters, which I think would represent the world record for Cant concentrations in the ocean. This is not surprising, as although previous calculations of Cant in this region based on the so-called Troca method were published, this method appears not reliable in this region. It is interesting to show the Troca results derived from the new data but authors should clearly highlight this is a wrong method that has not to be used before it is revised for Mediterranean (or for any other marginal sea and coastal analysis). I suspect the error concerns the Troca-0 definition which was originally evaluated from deep water in the open ocean but not using Mediterranean Sea data. Concerning the CT budget authors conclude this is very sensitive to the choice of the water-balance (Fin, Fout); this is not new. Any carbon (or salt, heat, O₂, nutrients...) balance in a strait strongly depends on the selection of Fin and Fout. For example, Ait-Ameur and Goyet (2006) indicate in their abstract: "The net export of inorganic carbon from the Mediterranean Sea to the Atlantic Ocean, ranges from 0.02 to 0.07 pgC/yr and strongly depends upon the water masses transport". So, what is new in the present manuscript ? To clarify my question, I have calculated the CT budget based on SEMANE 2002 data (Ait-Ameur and Goyet, 2006) using the Fin/Fout from Huertas et al (0.085 Sv in Med Outflow). The result is a net carbon transport of 89 kmol/s, or 0.033 PgC/yr, from the MedSea to the Atlantic. This is about the value presented estimated by Huertas et al, 0.025 PgC/yr... so my conclusion is that authors would have deduced about the same results and discussion using historical CT/TA data but selecting new ADCP data for the water balance (using direct MO outflow measurements as presented in figure 2). The last question in the general comment is: Are the new temporal CT/TA data important to reduce the uncertainties attached to the CT/TA budget at Gibraltar. This is not evaluated and discussed in this manuscript. However, authors have conducted

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an important series of cruises (they recall this many times) and I think they could offer much more results than those presented in the present paper, specifically regarding the temporal variability of CT/TA budgets. The new data obtained in this work represent an important contribution and authors should first present all data and discuss seasonality and interannual variability. Depending this analysis, they should then provide carbon and alkalinity budget like in the present work (taking the average over three years) or estimate and discuss seasonal/interannual budgets. This would be the first time such seasonal budget would be conducted in this region using seasonal data (both concentrations and fluxes). For a reader not familiar with this topic, the manuscript is not very easy to read as the results are often diluted in long sentences, many comparisons and repetitions.

Specific comments:

I will 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 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"). 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.

C2: Abstract: lines 4-7: A comprehensive sampling program was designed....to ensure a good spatial and temporal coverage;. Authors never say

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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).

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 don'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.

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.

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

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.

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

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 budget. 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.

Now I come to the Conclusions:

C9 Page 1050, line 21-26: this is like an introduction

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 26-27 authors write results... highlight the significance of the strait as a key area for evaluating interactions between both oceanic regions;. this is obvious. 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 across Gibraltar is so important for global change (or do you mean change in the global Mediterranean Sea ?).

C11 Page 1051, lines 1-5: The extensive analysis performed using data gath-

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ered 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.

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

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

C14 Page 1051, lines 8-14. Authors conclude on the importance of maintaining GIFT timeseries etc..... However, if they want to justify this, they should demonstrate how it is important to conduct seasonal 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 4 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 recommend 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.

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).

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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).

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

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

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

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.

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.

C22 Page 1037, lines 4-5. Referring 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 calculated from pH,TA for all cruises. You have 9 cruises to show how important is the fCO₂ seasonal variability.

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.

C24 Page 1038, line 2-23, I don'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).

C25 Page 1038 line 17; if you still refer to Copin and Begovic, check names. (Be-

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govic).

C26 Page 1039-1040; Cant concentrations derived from TrOCA are really very high, 81 $\mu\text{mol/kg}$ for your calculation, 111 $\mu\text{mol/kg}$ when quoting Ait-Ameur and Goyet (2006). This is more than 1 $\mu\text{mol/kg/yr}$ 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...)

C27 Page 1042, Authors used a direct approach to estimate the carbon and alkalinity budget. However, other methods (indirect) have been developed 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

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

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

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.

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

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 homogeneous 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).

References cited in this review

Souvermezoglou, E., et al. 1989. Red Sea Budgets of Salinity, Nutrients and Carbon

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van Geen, A. and E.A. Boyle (1990) Variability of trace metal fluxes through the Straits of Gibraltar, Paleoclimatol. Paleogeog. Paleoecol. (Global and Planetary Change) 89: 65-80.

-end of review

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