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## ***Interactive comment on “The CO<sub>2</sub> system in the Mediterranean Sea: a basin wide perspective” by M. Álvarez et al.***

### **Anonymous Referee #1**

Received and published: 24 October 2013

GENERAL COMMENTS This study presents a description of the CO<sub>2</sub> system variables along the Mediterranean Sea based on data collected during the development of two cruises in 2011. The distribution of the carbonate system properties in Mediterranean waters has been previously examined in a number of papers, either at a basin scale or in its sub-basins. In addition, the exchange of carbon between the Mediterranean Sea and the North Atlantic has been assessed by several authors and even the anthropogenic CO<sub>2</sub> inventory at a basin scale has been calculated. Therefore, the point of this work and its main objectives have been already considered in the past. In this sense, the originality of the paper and its expected impact may be seen as moderate. Also, in my humble opinion, some of the comments stated by the authors are not entirely true, particularly those regarding the aim of the paper, which I find a bit prepotent. For instance, the second paragraph of the abstract indicates that the anal-

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ysis performed is the first internal consistency analysis for the Mediterranean, which is somehow disrespectful to the previous and pioneers studies carried out by different groups. The following sentence is even inappropriate or at least incorrect, depending on what the authors mean by benchmark: 1. If benchmark is used in the sense of being a reference point for future observations and surveys, we would be unfair to early studies that provided novel, original and relevant data on the CO<sub>2</sub> system in different regions of the Med (e.g. at the DYFAMED site, Strait of Gibraltar etc). Plus, such historical data (whose accuracy and precision should not be questioned) are still especially useful to detect temporal trends and evaluate the response of the Mediterranean to the anthropogenic forcing (e.g. ocean acidification patterns). 2. If, on the other hand, benchmark refers to a standard by which following works should be measured or judged, that is something that should be jointly agreed by a representative number of the scientific carbon community.

This statement is also repeated in Pages 1463 and 1474. Either case, as the nature of this work is essentially descriptive, I doubt it could be defined as a paradigm breaker or starting point for future research in the Mediterranean. It rather provides high quality data at a nice spatial resolution that can be used for comparisons, modeling exercises etc. As it stands, the aim of the paper seems to be more ambitious than the actual information one can draw from its reading. Overall, the manuscript is well written and results are presented in a well structured way. Please see my specific comments below SPECIFIC COMMENTS Abstract This section seems to be a mission statement rather than a summary of a work. No results or conclusions are given and actually the message that one can take from the abstract is the high quality of the work performed and the need to be used as a reference for future studies in the Med. Introduction This section seems a bit short and it could be well extended. Although the focus of the study is properly presented, recent works dealing with this topic are missing. In fact, some of those studies are mentioned thereafter in subsequent sections whereas others are simply ignored. Among those, I would recommend to include at least, Huertas et al., (2009), Krasakopoulou et al., (2011) and Touratier et al., (2012). If a basin perspective

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is meant to be given, a brief summary of what has been done at a basin scale or in the sub-basins separately should be mentioned here along with the novelty or innovation that this work represents. Data and Methods Why was TA measured in a different way in both cruises? Please explain. Did you perform any sort of inter-calibration or inter-comparison between the two methods to ensure consistency?

Consistency of CO<sub>2</sub> analysis The thorough analysis performed by the authors is greatly acknowledged. The last paragraph is particularly interesting, as it points towards a new direction on carbon system research in the MedSea. Vertical Distributions Although results are explained in details, I miss comparisons or references to other works that have previously provided carbon data in the Mediterranean sub-basins. If the new results do not coincide with those reported previously, the authors should explain the reason behind such discrepancies (different measurement protocols, global change impact, effect of local processes on carbon dynamics etc). If, on the contrary, these recent observations are in agreement with previous results, then the authors would have more rationale and tools to confirm their data. It is somehow, surprising the fact that not a single comparison to early carbon measurements is made throughout this entire section.

Pag. 1466 line 14. This reviewer can provide several papers that show chemical characterization with regard to CO<sub>2</sub> variables in some MedSea sub-basins, particularly at the Strait of Gibraltar, Aegean Sea and Ionian waters. Regarding the effect of EMT on physical and biogeochemical distributions in the different MedSea sub-basins, does the author's analysis coincide (or differ) to the ones conducted by Klein et al., (2003) Gasparini et al., (2005), Touratier and Goyet (2011) among others?

Technical corrections Pag. 1449 Line 11: Bergamasco and Malanotte-Rizoli reference publication year is not correct. Pag. 1452 Line 7: Hernandez-Ayon et al., 1999 does not appear in references chapter. Pag. 1453 Line 11: Uppström, 1979 reference publication year is not correct. Pag. 1456 Line 12: Will be a more clear view if you name the water masses in the figures Pag. 1456 Line 19: Why AW pH shows an

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opposite trend? It increases as well AT and DIC. Line 25: Revise reference Crise et al., 2001 in references. Pag. 1458 Line 8: Hainbuncher et al., 2013 does not appear in references. Line 9: delete in the at the end of the line. Line 13: Revise reference Stöven, 2012 in references chapter. Pag. 1466 Line 27: Reference Roether et al., 1996 does not appear in bibliography. Pag. 1483 Line 21: Revise publication year of Roy et al., 1973.

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Interactive comment on Ocean Sci. Discuss., 10, 1447, 2013.

**OSD**

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