

REVIEW of os-2013-40. REFEREE #1.

The CO₂ system in the Mediterranean Sea: a basin wide perspective by Álvarez et al.

We would like to thank the reviewer for the suggestions and comments on our manuscript. Please find below a detailed answer to some specific comments and questions.

GENERAL COMMENTS

This study presents a description of the CO₂ 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.

We agree that several papers deal with the distribution of CO₂ species mostly at the sub-basin scale. Please see the review in Álvarez (CIESM, 2011). The only works presenting data covering the whole MedSea are Rivaro et al. (2010) and Touratier et al. (Biogeos. Dis., 9, 2709-2753, 2012). The first one corresponds to the TRANSMED cruise (2007) with low spatial resolution (9 full-depths stations in the MedSea) and the second one to the BOUM cruise (2008) with a much higher spatial resolution in the western and eastern MedSea. However, this second manuscript was finally rejected in Biogeosciences, so I think it is a personal choice whether it should be mentioned or not. Even if considering it, Touratier et al. (2012), mainly focuses on the CANT and acidification estimation instead of describing the water masses in the MedSea. In our work we provide a quasi-synoptic picture of three CO₂ variables, pH, DIC and TA, for the whole length and depth of the MedSea in the context of recently observed variability.

In addition, the exchange of carbon between the Mediterranean Sea and the North Atlantic has been assessed by several authors and even the anthropogenic CO₂ 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.

We are aware that the exchange of carbon between the MedSea and the North Atlantic has been assessed before (Álvarez et al., JGR, 2005; Ait-Ameur and Goyet, DSR II, 2006; Huertas et al., BG, 2009). The CANT inventory was assessed in other works (Schneider et al., JGR, 2010; Touratier and Goyet, DSR I, 2010; Touratier et al., BGD, 2012).

However, the aim of our work is just describing the post-EMT water mass distribution and characteristics in the whole MedSea using a full-depth and dense grid of hydrographic stations where three CO₂ parameters were measured. The inventory of CANT and the exchange of inorganic carbon in the Strait of Gibraltar are completely out of the scope of our manuscript.

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.

We are very sorry to read this remark, if this is the take-home message out of the manuscript we really regret it because it was not our intention.

For instance, the second paragraph of the abstract indicates that the analysis 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.

When talking about internal consistency analysis we refer to checking the internal consistency of the CO₂ system, comparing calculated with measured data, the analysis presented in Section 3.2. We are sure this is the first time such analysis was performed using CO₂ data from the MedSea, because this is the first time three CO₂ variables were measured all along the MedSea. Most of the published works present data from two CO₂ variables, pH and TA (PROSOPE cruise, 1999; Millero et al., 1979; Rivaro et al., 2010; in the Adriatic Sea: Luchetta et al., Chem. Ecol., 2010), TA and TIC (Meteor M51/2; BOUM data, DYFAMED data; and Krasakopoulo et al., DSR I, 2011).

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 CO₂ 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).

The accuracy and precision of historical and recent CO₂ data in any ocean basin should be questioned to evaluate the oceanic response to natural and anthropogenic forcing. Assessing the consistency of CO₂ data is always the first step in any CO₂ temporal analysis. In fact checking the internal consistency of CO₂ and ancillary data was the aim of several programs like GLODAP, CARINA, PACIFICA and now the more recent GLODAP 2.0. Although the aim of our work is just describing the water masses and not studying any temporal evolution we think is worthy to explain the quality of the data and refer where it can be easily accessed, concretely in the CDIAC web with the ancillary data well organized. Only the M51/2 (2001) cruise is also easily found in CDIAC, cxv the other cruises are really difficult to get and organize in a readable / treatable format.

Maybe benchmark was improperly used and we will withdraw this term. But any way our data is as novel, original and relevant as the historical ones.

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.

As previously stated benchmark was improperly used and we will withdraw this term from the manuscript. The group of authors belongs to the scientific carbon community, and moreover, T. Tanhua is co-chair of IOCCP and Go-SHIP.

This statement (benchmark) 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.

As commented, the term benchmark will be deleted.

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.

We agree with the referee that this is just a descriptive paper from a high quality data set with a nice spatial resolution, and it is easily accessible to all the oceanographic community no matter the research issue.

The main aim would be describing the post-EMT situation in the whole MedSea with regard to CO₂ variables (three of them). Describing water masses could be boring but we think is the first step before quantifying the changes and the forcing of those changes.

Overall, the manuscript is well written and results are presented in a well structured way. Please see my specific comments below

We thank the referee for these comments.

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.

The abstract has been improved in the sense of highlighting the results from our cruises with regard to the description of the CO₂ system in the water masses of the MedSea.

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 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 works represents.

As has been previously commented our work is not about CANT / acidification or the transport of inorganic carbon in the Strait of Gibraltar. We will extend the introduction and give some more info about the works dealing with the description of the CO₂ variables in the MedSea sub-basins.

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?

TA was measured slightly different in both cruises but with a potentiometric technique. We did not perform any intercalibration exercise between IEO and ISMAR. But this is the same for any other property, Temp, salinity, inorganic nutrients and so on. This is the reason behind the crossover stations between EF11 and M84/3 and the results from this analysis are commented in the manuscript. In fact, the repetition of the same stations from the two cruises was done within a times frame of less than one month, so they are practically synoptic, this is the best intercomparison to be made.

Consistency of CO₂ 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.

Thank you.

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.

As commented previously we are not assessing the temporal changes in any physical or biogeochemical property in the MedSea. The temporal changes in CO₂ variables will be the aim of a second manuscript part of H. San León's PhD. In order to do so, the first step is gathering all the published data, this is an extremely difficult task because the historical data are not archived and for the recent ones, only M51/2 is easily accessible.

In section 4.1.1.1 Meteor M84/3 cruise data, we perform a coarse comparison between the 2011 and other cruises. But it is true that this is not the aim of the manuscript and we will not expand this discussion.

Pag. 1466 line 14. This reviewer can provide several papers that show chemical characterization with regard to CO₂ variables in some MedSea sub-basins, particularly at the Strait of Gibraltar, Aegean Sea and Ionian waters.

Thank you but we are aware of this literature and some of them is now included.

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?

As previously said this is not the aim of the manuscript and we will not discuss this point further than already done at the end of section 4.1.1.1.

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 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.

All the former comments have been corrected or addressed in the new version of the manuscript.

Thank you.

REVIEW of os-2013-40. REFEREE #2.

The CO₂ system in the Mediterranean Sea: a basin wide perspective by Álvarez et al.

We would like to thank the reviewer for the suggestions and comments on our manuscript. Please find below a detailed answer to some specific comments and questions.

GENERAL COMMENTS:

This paper presents results from two cruises carried out in the Mediterranean Sea which provide a basin-wide dataset for the study of CO₂ parameters in the Mediterranean. The data treatment is sound and exhaustive and the authors perform an internal consistency analysis for the CO₂ system in the Mediterranean that can be useful for future studies. The data presented is of high quality and of interest to the science community. The paper provides a nice description of "this is what is observed".

We thank the reviewer for the compliments.

However, it would greatly improve if the authors also added some information explaining why and how those values are observed. Furthermore, if it is to be used as a staple reference for future studies it must include a comparison to previous studies, to put it in perspective.

In this manuscript, which in fact is intended to be the first one of a series of them based on the 2011 CO₂ data taken in the M84/3 cruise, we preferred to present just the chemical CO₂ characterization of the different water masses in the MedSea because there was no previous exhaustive and basin-wide description. The focus of previous manuscripts was mainly the CANT inventory / acidification (Schneider et al., JGR 2010; Touratier and Goyet, DSR I, 2010; Touratier et al., BGD, 2012).

We are not assessing the temporal changes in any physical or biogeochemical property in the MedSea because that would be the aim of a second manuscript part of H. San León's PhD. In order to do so, the first step is gathering all the published data, this is an extremely difficult task because the historical data are not archived and for the recent ones, only M51/2 is easily accessible. We are currently contacting all the PIs and gathering all the CO₂ and ancillary data in a common format. Then we will proceed with the quality control in the way of the CARINA group and finally the quantification of the changes. In section 4.1.1.1 Meteor M84/3 cruise data, we perform a coarse comparison between the 2011 and other cruises. But it is true that this is not the aim of the manuscript and we will not expand this discussion.

SPECIFIC COMMENTS:

My main concerns with this work are the following:

1) This is mostly a descriptive paper, it does little more than present the values measured in the cruise. The manuscript would greatly improve with some interpretation of the results.

We admit this is a descriptive manuscript; the literature is full of them about water masses in several oceans. We wanted to stress the peculiar CO₂ chemistry in the MedSea (internal consistency analysis) and its sensitivity to natural and anthropogenic changes (buffer factors section), and present the East to West evolution of the main water masses, Atlantic, Levantine and the deep and bottom waters. Particularly, deep and bottom waters show EMT and post-EMT differential characteristics that are discerned just with the vertical profiles.