

Interactive
Comment

Interactive comment on “Distributions of the carbonate system properties, anthropogenic CO₂, and acidification during the 2008 BOUM cruise (Mediterranean Sea)” by F. Touratier et al.

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GENERAL COMMENTS

Comment from Referre #2: I found discussion (P 2728 to 2733) a bit personal and a bit aggressive with terms like ‘inappropriately applied’ (P 2728) ‘not scientifically correct’ (P 2730) ‘do not provide any scientific argument’ (P 2729).

Our response: The TrOCA approach, by definition, cannot be applied within the ocean surface layer since the Redfield equation may not be at equilibrium here. This restriction in the application of the method is clearly stated in the paper where the TrOCA approach is detailed (Touratier et al., 2007). Despite this, Rivaro et al. (2010) applied

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the TrOCA approach in the surface layer. We refute any aggressiveness in the term 'inappropriately applied'. We only try to remember to the potential users of the TrOCA approach that results generated by the TrOCA approach for the surface layer may be wrong. Once again, our intention was not to be aggressive and to end with this impression we remove the following terms: '...not scientifically correct.' '...do not provide any scientific argument...'

Comment from Referre #2: In fact the discussion (pros and cons of each CATN method) does not reflect at all the results section that deal with comparison between the 2001-2008 cruises (which does correspond to the title of the paper).

Our response: In the result Section, a full description of the distributions obtained for the carbonate system properties during the 2008 BOUM cruise is given (AT, CT, Cant). This is completed by a detailed description of other properties (S, T, O2) that either influences the distribution of water masses or the computation of Cant. It is our ethical duty to compare these results with those of the year 2001 (METEOR 51 cruise) and this is simply what we are doing. The BOUM 2008 and the METEOR 2001 sections throughout the whole Mediterranean Sea are very comparable and thus provide an excellent opportunity to analyze the evolution of the water mass properties. It must be remembered here that these two cruises represent most of the high quality carbonate system data available for the Mediterranean Sea. The discussion focuses mainly on the distributions of anthropogenic CO2 and acidification. Since these estimates are based on models, there is a real need to justify the selected approaches (MIX and TrOCA) and to compare their advantages/disadvantages. We think that both the result section and the discussion fit well with the Title (and objectives) of the manuscript.

Comment from Referre #2: I know it is not nice when papers show your method differs from others, and question your method and ideas, data. Yet, everyone knows the essence of science is that methods, results, etc... must be questioned. In high quality journals, I believe that tone should always remain polite and objective. This paper could/should have been : (1) a description of the BOUM data (2) a comparison of the

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BOUM data with the Meteor data (2001) (3) a in depth discussion of the different methods based on the comparison of CANT computed from these different methods using both BOUM and Meteor data. As it stands the paper is stuck somewhere in between (2) and (3). The paper does not achieve (3) since it just throws some arguments at each method/paper. The authors should decide if they want to go all the way to (3) or go back to (2). Personally, I would be in favor that the authors go back to (2) removing all of the discussion that could be published as a parallel short communication.

Our response: We always remained polite throughout the manuscript and we did our best to be as objective as possible. We welcome objective critics and we also welcome the opportunity to answer them. We do not fully agree with the point of view of Referee #2 on the organization of the manuscript. For us, points (1) and (2) are fully satisfied. It must be remembered here that this paper is part of a 'Special Issue' that collect many other contributions that use data from the 2008 BOUM cruise. In this paper, we only had in charge to describe the distribution of the carbonate system properties. We then compare the results with those of the year 2001, we estimate the distribution of anthropogenic CO₂ and, finally, we estimate the level of acidification since the pre-industrial era. All these extra were added because we think it was extremely interesting and informative for the readers. Several papers (Coatanoan et al., 2001; Vazquez-Rodriguez et al., 2009) have shown that in depth discussion about the different approaches used to estimate Cant is very difficult or even impossible. For instance, the paper of Vazquez-Rodriguez et al. (2009), which compared five different approaches applied to the Atlantic Ocean, never tried to compare the structures but they only focus on the comparison of their results. Consequently, in the discussion of the present manuscript, we mainly focus on the comparison of the results. We also discuss several hypotheses used by specific approaches in order to understand the divergences among the results. We thus agree with Referee #2 that point (3) is not fully achieved, since this was not one of our objectives. Furthermore, we would like to point out that such issue could probably never "be published as a parallel short communication".

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Comment from Referre #2: Anyway, I really would like to see some discussion on why CANT is changing so fast in 7 years in the Mediterranean (maybe some elements are given in intro but the reader has to make the link). This a very intriguing and interesting result and I would like to learn what could be the possible mechanisms behind it.

Our response: P. 2728 of the manuscript we read 'The 2008 CANT and ΔpH distributions globally corroborate those obtained for the year 2001 (published by Touratier and Goyet, 2011)'. Why Referee #2 understands the opposite?

MAJOR COMMENTS

Comment from Referre #2: L 2729 L 9 : Authors state that Troca cannot be used with classical Redfield ratios and that it is required to develop local/regional Redfield ratios. And this can explain the discrepancies between methods. So what ? Why not test this with the present data sets ? Replace RON, ROP, RCO values with classical Redfield ratios and show the difference.

Our response: The determination of the Redfield ratios RON and ROP has been done using specifically the 2008 dataset, which is the best way for the application of the MIX approach. As explained in the ms, these ratios are estimated for two different layers 50-750m and > 750m. The RON and ROP estimates obtained for the deep layer correspond to the classical Redfield ratios while those for the surface/intermediate layer differ significantly.

Comment from Referre #2: L 2730 L 4-6 : If ΔC^* and Troca do not fit in the Atlantic can we really extrapolate this conclusion to the Mediterranean ? Why not check ? Please compute CANT with both standard Troca and ΔC^* methods and show the difference.

Our response: Both the ΔC^* and the TrOCA approaches are designed for world-wide applications. Given that the ΔC^* approach has shown some incoherent results in several region of the world ocean, we think that we can reasonably have some

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doubts on its capacity to estimate Cant in the Mediterranean Sea. Unfortunately, the ΔC^* approach cannot be tested here since CFCs were not measured during the 2008 BOUM cruise.

Comment from Referre #2: I would like to see discussed how the decadal changes described by the same authors in the northwestern Mediterranean (Touratier & Goyet 2009) fit with the present results/interpretation.

Our response: The study of Touratier and Goyet (2009) described the decadal (1993-2004) evolution of the carbonate system properties at the Dyfamed time-series station. To make a link between the 2008 BOUM data and those of the DYFAMED it would be necessary to lengthen the time-series by incorporating the most recent data. Despite this very interesting topic, this is not one of the objectives of the present paper.

MODERATE COMMENTS

Comment from Referre #2: P 2715 L 5 : rather than referring to the figure of a paper why not simply a figure of theta versus S from both cruises ? Same applies later for AT. Why not plot AT versus S from both cruises ?

Our response: We think it could be confusing for the readers to add figures with results from the 2001 Meteor cruise. Concerning the T/S diagrams for the two different cruises, we wrote (p. 2715, lines 5 to 7) that they did not vary significantly. We reached exactly the same conclusion for AT.

Comment from Referre #2: P 2717-2718 : The authors only indicate in the introduction the papers where Troca provides consistent results with other methods. They should at least mention the papers that give CANT values from Troca that are clearly divergent (Huertas et al. 2009; Yool et al. 2010). An introduction should give the overall picture.

Our response: We agree with Referee #2. Consequently, p.2717, line 18, we added the following sentence: 'On the other hand, the papers of Huertas et al. (2009) and Yool et al. (2010) suggest that the TrOCA approach may provide overestimated estimates of

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CANT:

Comment from Referee #2: In P 2719 L 22 : The authors state that classical Redfield ratios RON and ROP cannot be applied in the Mediterranean, but in P 2723 L7 the authors state that the RCO is the classical Redfield ratio. Please clarify as this cannot work both ways.

Our response: As explained in the manuscript and above, the classical Redfield ratios are valid for the deep layer of the Mediterranean Sea while they are significantly different for the surface/intermediate layer. Concerning the RCO ratio, we apply the classical Redfield ratio since neither C nor O are considered as limiting elements of the phytoplankton growth.

Comment from Referee #2: P 2726 L 2 : I'm surprised that throughout text authors provide values of CANT at $0.1 \mu\text{mol/kg}$ level, when the actual measurement error of DIC is close to $1 \mu\text{mol/kg}$ (on the good days). I suggest to round all values to unit, in agreement with the stated accuracy of CANT ($6 \mu\text{mol/kg}$, P 2728).

Our response: We agree with Referee #2. We made the correction in the manuscript.

Comment from Referee #2: P 2727 L 16 : please quantify 'accurately' by computing the error on calculated pH by propagation errors on DIC and AT (include this at end of material and methods).

Our response: The pH is derived from CT and AT using the CO2SYS model of Lewis and Wallace (1998). Using the error propagation technique to estimate the uncertainty on the calculated pH from this model is extremely difficult.

Comment from Referee #2: P 2727 L 24 : regarding term 'acidified'. I'm not sure that this term is adequate. pH might have dropped faster than elsewhere but in surface waters pH remains within acceptable levels ($\text{pH} > 8.05$, Fig. 12).

Our response: The term 'acidified' simply refers to a decrease of the pH.

Comment from Referre #2: P 2728 L 7-9 : Here the authors compute the pH change from changes in CANT from 2001-2008 cruises. I would like to see a comparison with the pH change computed from difference of the 2001-2008 but from pH computed directly from AT and DIC field data (without the troca and mix modeling).

Our response: This would be possible if the sampling grids used during the 2001 Meteor and the 2008 BOUM cruises were exactly the same. An alternative approach would be to interpolate the CT and AT data on a common regular grid, but given the low number of available measurements, the errors generated by the interpolation procedure would be important.

Comment from Referre #2: P 2730 L 20-25 : I find this comment a bit unfair. Validation of a 3-D OGCM with sparse DIC and AT cruise data is a very tricky business. But anyway, a model will always be an unperfected tool that allows to check a working hypothesis by a series of tests. This is what Yool et al. (2010) did, although I understand that the authors dislike their conclusion.

Our response: The conclusion of Yool et al. (2010) suggesting that the 'TrOCA results systematically overestimates by more than 50% the Cant inventories' is erroneous. All Cant inventories that have been computed with TrOCA are very similar to those computed with other approaches (see the papers of Touratier and Goyet 2004; Vazquez-Rodriguez et al., 2009). It must be remembered here that the inventories published in these papers were computed using measurements of several properties (like O₂, CT, AT, S, Σt , etc.) Concerning the availability of the carbonate system properties (AT, CT, and pH) that could be used to validate their 3D simulation, we indicate that huge databanks like GLODAP, CARINA, or SOCAT exist and they are easily accessible.

MINOR COMMENTS

Comment from Referre #2: P 2712 L 20 : might want to refer a general introductory paper of the BOUM experiment if it exists ?

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Our response: To our knowledge, such a paper is not yet published.

Comment from Referre #2: P 2712 L 22 : spell out INSU/CNRS

Our response: This is done

Comment from Referre #2: P2713L9: spellout CTD

Our response: This is done

Comment from Referre #2: P 2716 L 9 : why an approximation of 100 km on distance from Marseille ? Why not exact distance ?

Our response: The distance is 1400-500 km and it corresponds to distance between Marseille and Station A (Fig. 1). This distance is useful to visualize station A in Fig. 5.

Comment from Referre #2: P 2716 L 16, L22 : use AT abbreviation

Our response: This is done

Comment from Referre #2: P 2718 L 17 : I not sure 'persuasive' is the adequate term here.

Our response: We replace the term 'very persuasive' by 'efficient'

Comment from Referre #2: P 2724 : include here (Material and methods) the estimated errors on CANT values from Troca and MIX. This only appears much later in the text (start of discussion, P 2728), so that reader only 'discovers' error estimates after reading all of the results.

Our response: The uncertainties associated to these approaches are now indicated when they are presented.

Comment from Referre #2: P 2727 L 24-25 : provide the time frame for these pH decreases (100 yrs ? 50 yrs ?) Our response: Since the pre-industrial era, the time is approximately 138 years. This information is added P 2727 L 24-25 Comment from Referre #2: P 2728 L 9-10 : please add 'if we assume a linear trend'. Our response:

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This is done Comment from Referre #2: P2729L13: who=>why

Our response: This is done

Comment from Referre #2: P 2729 L 23 : Learn => Compute

Our response: This is done

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