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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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Touratier et al have laid out in fairly precise terms their objectives in writing this manuscript:

1. Describe the distribution of the carbonate system properties. 2. From (1), calculate the distribution of the anthropogenic CO₂ in the Mediterranean Sea. 3. From (1) & (2), calculate the chemical and spatial extent of "ocean acidification" in the Mediterranean Sea.

All three of these objectives are timely and relevant questions to ask that fall well within the scope of this journal. In order to address these questions, the authors have first

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gone to sea and collected hundreds of samples, analyzed them for the relevant substances and built a sizeable database. The track-line follows a path very similar to an earlier expedition (Meteor 51/2) which allows comparison of their dataset to another work for verification purposes and enables an investigation of the evolution of carbonate system in both spatial and temporal modes.

The analytical methods used in this paper fall into two types: chemical and computational. The chemical methods follow long established protocols used in both the JGOFS and WOCE programs. One of the authors (CMG) was intimately involved in the development of some of these protocols, so there should be no question as to the quality of the data – I fully expect it to be of the highest precision and accuracy. To the extent that their deep water values where little change in concentration is expected over time agrees with the results from the Meteor expedition, this is quite clear. Moreover, the current dataset has more than twice the number of hydrographic stations where the carbonate parameters were measured as the earlier work. This alone represents a significant scientific contribution.

In addition to plotting the spatial distribution of the carbonate species, Touratier et al have placed these measurements within a physical oceanographic context. Using dissolved oxygen, salinity and potential temperature (θ), they have identified the many different water masses within the two major basins in the Med sea and plotted these properties within this context showing how circulation and mixing affects the distribution of the various water masses and their properties.

The computational analyses in this paper are substantial as well. In order to determine the distribution of anthropogenic carbon dioxide in the Mediterranean Sea, the authors have chosen to use two independent calculations, not just one as is typical in most other studies. They are to be commended for this approach. The calculation of the fraction of $C(T)$ that is attributable to anthropogenic sources [$C(ant)$] is neither simple nor straight-forward. The many methods proposed for doing so are ample testimony of this. By making two independent estimates of $C(ant)$, Touratier et al are making it

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possible to immediately evaluate the validity of either method by comparison with the other. This kind of self appraisal and testing is the very heart of the scientific method. Unfortunately it is all too rare in the literature today.

The MIX approach (originally developed by Goyet et al, 1999) uses a geochemical mass balance approach combined with the use of the Redfieldian biology. In order to use these concepts in the Med Sea, the authors calculated "redfield-ratios" specific to each of the two major basins in the Med sea and applied these to each of ten water masses in each basin. In contrast, the TrOCA approach (Touratier and Goyet, 2004), is much simpler in construct and less computationally demanding. As separate estimates of $C(\text{ant})$, they provide a mutual check of the validity of these approaches and an estimate of the uncertainties of the estimates based upon the magnitude of their differences. Simple visual comparison of the two maps of $C(\text{ant})$ reveals a remarkable degree of agreement both in terms of spatial distribution and magnitudes of $C(\text{ant})$ intrusions.

Lastly, the authors have used the difference between the measured CO_2 concentrations and the anthropogenic input to calculate a pre-industrial distribution of pH within the Med sea. Comparison of the two sections (pre-industrial vs 2008) reveals a distinct pattern of ocean acidification and the magnitude of this effect along the section through the middle of the Med sea. These maps show a remarkable consistency with known patterns of circulation and water mass ages lending further credence to the results.

Unfortunately, some of the previous reviewers and commenters of this paper have voiced a concern about Touratier et al championing calculations of their own invention. I find this criticism to be patently absurd. Personally, I would be more concerned if they did not promote their own concepts. In fact, scientists are supposed to promote their own work in discussions with others. To not do so raises questions of why the work was published initially if they have no use for it and whether they are abandoning it now for undisclosed reasons or flaws. That being said, I must admit that initially I was a bit put-off by the amount of direct criticisms of other estimates of $C(\text{ant})$. However, upon

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brief reflection I realized that only when scientists openly and candidly present their work in comparison and contrast with others can the science advance. To not do so is a serious over-sight.

There has also been an objection raised as to whether one can truly speak of "acidification" when the end-point is still on the basic side of neutrality. This objection confuses process with conditions. The addition of massive amounts of CO₂ to the ocean is a process that is acidifying: in all cases the pH is lowered toward the acid end of the scale. It matters little where the start and end-points are when one is describing the process. Similarly there was an objection to the statement that the Med sea is one of the most OA impacted areas of the ocean. Part of the objection was based in the fact that by starting out at a higher pH, a similar addition of CO₂ would cause a greater pH shift. While true, this overlooks the fact that regardless of the starting or end-point pH, the Med sea has absorbed a greater amount of CO₂ per liter or kg than most other bodies of water. It also overlooks the fact that the impact of ocean acidification is seen all the way to the bottom and this impact is a substantial fraction of the surface offset. In my view, both of these objections are mis-placed. Indeed, it is probably due to the fact that the Med sea initially was more alkaline than the open ocean that led to its seawater absorbing more CO₂ per unit volume than other areas. Likewise, the unique circulation of the Med sea has allowed the anthropogenic CO₂ to penetrate deeper. Thus, on a per unit area basis, the Med sea has absorbed more CO₂ than other ocean basins.

Finally, I'd like to say that discussion of any one of their initial objectives would have been sufficient content for a paper. The fact that they have included all three of these issues in a single manuscript tells me that this manuscript is both a significant and substantial scientific contribution to our understanding of the carbonate chemistry of the Mediterranean Sea. With a few minor edits, this manuscript should be fully acceptable for publication in BGD and I see no reason to further delay its acceptance.

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