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# *Interactive comment on* "High resolution regional modeling of natural and anthropogenic radiocarbon in the Mediterranean Sea" *by* Mohamed Ayache et al.

**Anonymous Referee #3**

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General comments: This work is aimed at providing a basin-scale description of radiocarbon (14C) distribution in the Mediterranean Sea through the NEMO-MED12 dynamical model. Overall, the model seems to mimic the main spatial and temporal variability of 14C from the pre-bomb period until 2008, when the simulation is run. However, some regional patterns are not entirely reproduced, which, in my opinion, could be attributed to the presence of mesoescale phenomena that are not resolved by the model. The model is validated with in-situ measurements and proxy-based reconstructions for the Mediterranean Sea. The authors conclude that Atlantic inflow through the Strait of Gibraltar exerts a high influence on the natural 14C distribution. They also state that by following the propagation of the anthropogenic 14C signal, ventilation of deep Mediter-



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Discussion paper

ranean waters can be examined, with special emphasis on the effect of the Eastern Mediterranean Transect. Even though data are undoubtedly relevant and novel, I find the work very descriptive and some of the circulation mechanisms behind 14C patterns are not fully described (or even mentioned). At a certain point, it seems that the study is only focused on showing the robustness of the modelling approach, in other words, how well simulations mimic the available 14C records rather than providing a view of how circulation is responsible of radiocarbon evolution and patterns in the basin before and after the atmospheric bomb tests. I miss, for instance, comparisons with other ocean regions, or a more comprehensive discussion (explanations) on the discrepancies found between the model output and the in situ data. Potential behaviour of radiocarbon under other scenarios in the basin would have been also nice.

Nevertheless, taking into account its novelty, this work deserves publication in Biogeosciences although revisions and modifications should be introduced in the manuscript in order to clarify some of the conclusions drawn by the authors.

Specific comments:

Abstract

The simulation was run until 2010 to give the post-bomb distribution.

I believe the simulation is run until 2008 although model outputs are compared with in situ measurements taken in 2011.

Introduction Page 1, Lines 15-21: The whole paragraph seems to be out of scope. I do not see the relationship between the stresses suffered by the MedSea and the distribution of radiocarbon

Page 2, Lines 6-10. The excess of evaporation versus precipitation does not transform Atlantic waters into Mediterranean waters and leads AW to sink offshore. It is actually the process that drives the entry of Atlantic waters through the Strait of Gibraltar to compensate water loss and keep the mass balance. Water masses formation in the

basin are subsequently the result of other phenomena more related with atmospheric forcing and density gradients. It might be a small detail but conceptually it is important, especially for readers not familiar with the MedSea. I would suggest to re-write the paragraph.

Page 2, Line 21. I would not compare CFC and tritium with 14C, as this one is not entirely passive. I understand what the authors mean by the sentence but radiocarbon is indeed used by the biological community so I would recommend to state that circumstance or simply not to equate the tracers.

Results

Page 8, lines 17-18: According to Figs 2 and 3, the model does not overestimate the radiocarbon concentration in surface everywhere in the basin but it depends on the particular region. Also, plots in Fig. 2 could be manifestly enhanced as it is hard to distinguish in situ observations over the contour in the graphs.

Page 8, Line 20. The careful comparison between vertical profiles of model outputs and seawater observations in Fig. 4 is restricted to the Eastern basin. Why the Western basin is not considered if according to Fig 3 there are also some disagreements? Not enough in situ data to compare? Please clarify

Page 8, Line 27. Any idea why the pre-bomb radiocarbon levels differ so much between the in situ data and the model outputs in the Aegen sub-basin (Table 1)? I guess there must be some circulation patterns not resolved in the model. Plus, I do not understand the sentence the range in the observations is also high.

Page 9, Line 10. At depth, the model tends to underestimate the 14C penetration in the deep Ionian sub-basin, where it fails to reproduce the high 14C levels associated with EMDW formation (Fig. 4b). Where is this disagreement shown in Fig. 4b? Does the plot correspond to that particular sub-basin or to the entire Eastern basin?

Page 9, Line 24. The greater is the mixing layer depth, the weaker is the amplitude and

the peak is delayed. Is this sentence grammatically correct?

Page 9 and 10: To me, Fig. 7 depicts too much disagreement between simulated distributions and in situ data in many regions, not only in deep convection areas. For instance, even though it is hard to see the symbols in Fig. 7a, it seems that in surface waters of the Strait of Gibraltar the model overestimates the radiocarbon concentration by more than 20 ‰Ṗlus, in the discussion section it is stated that there is no time series data of 14C concentration in that area, while in the graph there are at least 4 measurements in the gulf of Cadiz and within the channel of the Strait. Could have they been used to fuel the model? In addition, explanation of data indicated in Figs 7 and 8 is confused, as description of patterns jumps from one to another without a logic sequence.

Page 10: The radiocarbon time evolution spans from 1925 to 2008, why this particular year? In fact, Fig. 7 shows comparison between the model outputs and data of the 2011 Meteor cruise, which included measurements throughout the whole basin. Why the simulated evolution does not run until then? It would be interesting to confirm that evolution follows the pattern indicated in Fig. 7. Also, I would keep the same vertical scale in all plots to facilitate comparisons. The response found in intermediate-deep waters of the gulf of Lions is somehow unexpected, as deep convection events during winter should favor the sink of radiocarbon, particularly in extreme winters, such as that occurring in the area in 2004/2005. In Fig 9d, the intermediate layer of the gulf of Lions exhibit the lowest radiocarbon levels after the bomb episodes and deeper waters are characterized by values even lower than those found in the Tyrrhenian sub-basin. Is there any explanation for that? Moreover, are data in plot 9d integrated values through the whole water column? These results are not explained in the text. Plus, the title is wrong, it should say whole water columns.

Discussion

Page 11, Line 10. The radiocarbon simulations provide independent and additional

constraints on the thermohaline circulation and deep-water ventilation in the Mediterranean Sea. I do not see this in the manuscript. It would be the other way around. Data are interpreted according to the general circulation mechanisms known to proceed in the Med Sea.

Page 12. The comparison between the model outputs and the 14C values from in-situ data reported by Broecker and Gerard (1969), Stuiver et al. (1983) and Tanhua et al. (2013) reveals a good model performance in simulating the bomb/post-bomb radiocarbon distribution (Fig. 4b, Fig. 8). However the representation of the pre-bomb distribution is more contrasted in the simulation (Fig. 4a). I do not understand this paragraph. In fact, those two figures in particular show the largest disagreements, particularly in intermediate-deep waters and for the bomb-produced radiocarbon.

Page 13, Line 7: with higher convection occurring especially during the bomb peak. Where is this shown in the paper?

Conclusions The natural distribution of 14C in the Mediterranean Sea is mainly affected by the inflow of Atlantic water through the Strait of Gibraltar

As far as I understood, the concentration of radiocarbon in the Atlantic inflow did not come from in situ data or available measurements since it was taken from previous modeling approaches (as indicated in different sections of the paper). Therefore, this study does not show per se, the influence of the Atlantic radiocarbon on the distribution of this tracer in the Med Sea, as it is a fixed value used to fuel the model. The paper actually demonstrates that the entry of Atlantic waters is essential for water masses formation and circulation in the Mediterranean, which is a very well known topic and which, in turn, regulates the distribution of radiocarbon. In fact, it would have been interesting to perform the same simulations by changing for instance the values of the water masses transport through the Strait or the radiocarbon concentration associated to the Atlantic jet. To me, such conclusion cannot be drawn from the data. I would omit it here and in the abstract or at least, the sentence should be re-written.





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