We thank reviewer #3 for her/his constructive comments on the manuscript. We have carefully considered all questions and concerns raised. The structure of our reply is as follows; each comment from the anonymous reviewer is recalled in blue, and our reply in black.

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.

The ^{14}C simulation was done in a computationally efficient off-line mode, which allows us to run simulation of different tracers (e.g. Palmieri et al 2015; Ayache et al. 2015), in pre-computed transport fields instead of re-computing them, which is very costly.
The dynamical fields are available until 2011 from Beuvier et al. (2012). Starting from the end of the pre-industrial equilibrium run, the model was integrated from 1765 to 2011 (as mentioned page 7 line 13) covering the Suess effect (SUESS, 1955), the entire radiocarbon transient generated by the atmospheric nuclear weapon tests performed in the 1950s and early 1960s as well as the anthropogenic CO_2 increase.

Corrected in the revised manuscript
[See changes p 1, line 4 in the revised manuscript.]

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

In addition to providing constraints on radiocarbon distribution in the Mediterranean, our simulations provide information on the ventilation of the deep Mediterranean waters which is useful for assessing NEMO-MED12 performance.
This study is part of the work carried out to assess the robustness of the NEMO-MED12 model, which will be used to study the evolution of the climate and its effect on the biogeochemical cycles in the Mediterranean Sea, and to improve our ability to predict the future evolution of the Mediterranean Sea under the increasing anthropogenic pressure (e.g. Drobinski et al., 2012; Beuvier et al., 2010; Herrmann et al., 2010; Somot et al., 2006).

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.

We thank the referee for this suggestion, this paragraph is rephrased in the revised version according to reviewer's comments.

[See changes p 2, line 7-10 in the revised manuscript.]

Page 2, Line 21. I would not compare CFC and tritium with ^{14}C, 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.

This point has been also raised by reviewer #2 and it has been addressed in the revised version of the manuscript.

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.

The number of in-situ data for the pre-industrial period is very limited in the Mediterranean Sea. Figure 2 shows that the east-west gradient was satisfactorily captured by the model, with a slight overestimation of ^{14}C concentrations in the surface water of western basin. However results on figure 3 are reservoir ages and as discussed in the manuscript they are influenced by other sources (e.g. coastal input of "old carbon") so that only the spatial structure is discussed (east-west gradient).
More paleo-data from the pre-industrial period would help improving the knowledge of the natural distribution of ^{14}C in the Mediterranean.

Fig.2 was improved as suggested by the reviewer.

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

The careful comparison between vertical profiles of model outputs and in-situ data in Fig.4 is restricted to the EMed because there is no data available for the WMed for the bomb situation from Stuiver et al. (1983).
Fig. 3 presents only the surface values from paleo-reconstruction, and there is no data for the deep waters.

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.

The simulation of natural radiocarbon is particularly difficult because the average dynamical circulation used in the present study does not produce enough convection to pull-up the old carbon accumulated in the deep water. In addition, our simulation does not take into account the potential impact of old carbon in the coastal area, which could be the case in the Aegean sub-basin.

The sentence ‘the range in the observations is also high’ means that the range of uncertainty is higher in the observations.
Clarified in revised version.
[See changes p 8, line 30 in the revised manuscript.]

Page 9, Line 10. At depth, the model tends to underestimate the ^{14}C penetration in the deep Ionian sub-basin, where it fails to reproduce the high ^{14}C 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?

The vertical profile potted in Fig.4b represents the model result in the Ionian sub-basin together with in-situ data measured by Stuiver et al. 1983 at 18 °E.

As mentioned in the Introduction, in this study we used the NEMO-Med12 dynamical model that was already tested and evaluated with other tracers, such as tritium (Ayache et al., 2015a), helium (Ayache et al., 2015b) and CFC (Palmieri et al., 2015). Those tracers have highlighted that the model simulates a too-weak formation of Adriatic Deep Water (AdDW), followed by a weak contribution to the Eastern Mediterranean Deep Water (EMDW) in the Ionian sub-basin. The EMDW formed in the Adriatic basin is propagating to the entire deep eastern basin so that the consequences of weak formation of this water mass in the model are observed in the whole sub-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?

We thank the referee for this suggestion, this sentence has been changed for the sake of clarity.
[See changes p 9, line 28-29 in the revised manuscript.]

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 ‰. Plus, 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.

Although we partly agree with the reviewer, we think that the disagreement between simulated distribution and in situ data is particularly evident in deep convection areas as represented in Fig. 7.

In this simulation we used the same parametrization for the whole basin with same boundary conditions at the surface (first level), with ^{14}C and the atmospheric CO_2 values extracted from Orr et al. (2001). The radiocarbon values in the buffer zone are prescribed from a global simulation of radiocarbon by Mouchet et al (2016), and the ocean ^{14}C is initially set to a constant value of 0.85 ($\Delta^{14}C$ = -150 ‰, appropriate for the deep ocean; (Key et al., 2004)).

Hence we did not use any in-situ measurements to fuel the model for a specific region, because here we aimed to develop and optimize a ^{14}C modelling method for the whole Mediterranean Sea basin. Moreover no time series exists close to Gibraltar exist to force the model in the duration of the simulation. Data in the gulf of Cadiz represent a single date. As we used in-situ data to evaluate our results they cannot be employed to force the simulation.

Figures 7 and 8 present the same in-situ data of METEOR M84/3. Fig.7 provides a descriptive overview of the global horizontal distribution of ^{14}C, where the vertical profile in Fig.8 permits to quantify the difference between the model and in-situ data at different levels. Hence the description of these figures was mad at the same time in the text.

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.

The model was integrated from 1765 to 2011 as mentioned in section 2.3 page 7 line 14. The ^{14}C evolution was plotted from 1925 to 2008 in Fig.9 just to zoom on the period affected by the Suess effect (SUESS, 1955), and the entire radiocarbon transient generated by the atmospheric nuclear weapon tests performed in the 1950s and early 1960s as well as the anthropogenic CO_2 increase.

The intermediate-deep waters of the Gulf of Lions, characterized by ^{14}C values that are lower compared to the other sub-basins, are the result of a mixture of local water masses with Levantine Intermediate Water that is formed in the Levantine sub-basin, i.e. this water mass is isolated from the bomb signal in the atmosphere until arrived to the Gulf of Lion (deep convection area), hence the peak-bomb appears delayed in this area.

The data in plot 9d integrate values through the whole water column and present the same pattern as in Fig.9c for the deep water where the content of radiocarbon in the deep water control the distribution of $\Delta^{14}C$ in the whole water column.

This has been clarified in the revised text, and the title has been corrected as suggested by the reviewer. However we didn't use the same scale, because in the vertical section $\Delta^{14}C$ value up to -70 (due to the AdDW waters shortcomings, as mentioned provisory), and if we use the same scale for the horizontal maps, many information will be not clear as well (i.e. the gradient between the different basins).

[See changes p 11, line 3-4 in the revised manuscript.]

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.

Unlike the other tracers (e.g. CFC and Tritium), radiocarbon simulation provide additional constraints on the thermohaline circulation from the seasonal cycle to decadal and centennial timescales (e.g. Naegler, 2009; Muller et al., 2006; Rodgers et al., 1997; Guilderson et al., 1998).

In this study we have implemented the ^{14}C module in high resolution regional model, and we work mainly on the validation on this ^{14}C modelling method in the Mediterranean Sea basin, this work will allow many other applications especially in paleo-context e.g. Sapropel events. However direct comparison with in-situ ^{14}C data is a new constraint for the model and it has revealed or confirmed some shortcomings such the weak EMDW formation. This will be clarified in the conclusion of the paper.

Clarified in the revised manuscript
[See changes p 3, line 2-7 in the revised manuscript.]

Page 12. The comparison between the model outputs and the 14C values from insitu 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.

The reviewer is correct, there is a quite large disagreement between the model and in-situ data in some regions at intermediate-deep water depths for the bomb produced radiocarbon. However the mentioned sentence refers to Fig.6 where an important disagreement (~ 15 ‰) is observed between the model outputs and the sea-surface ^{14}C record obtained from a 50-year-old shallow-water coral from Tisnérat-Laborde et al., (2013) for the natural pre-bomb signal. On the other hand the model nicealy represents the bomb signal with the good timing and the amplitude of the peak in the near-surface water compared to in-situ data (Fig.6).

For the sake of clarity, this sentence has been modified in the revised version.
[See changes p 12, line 6-9 in the revised manuscript.]

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

We agree with the reviewer that the sentence is not clear. There is no higher convection during the period of bomb peak, but the surface water masses undergo transfer or convection with different intensity in the different sectors of the Mediterranean Basin. This argument has been clarified in the manuscript

[See changes p 13, line 16-17 in the revised manuscript.]

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.

Unfortunately, there is no time series data of ^{14}C concentration close to the Strait of Gibraltar. Hence simulated ^{14}C levels in the model's Atlantic water (AW) are determined from global model estimates. As mentioned in the paper, we have performed sensitivity tests on the imposed value (as presented in Fig.1, below).

We have performed two simulations with different boundary conditions at Gibraltar (red and blue boxes and lines); the first time series (red box) gives very low level of radiocarbon in the Mediterranean Sea (as represented in Fig.2, below). In the second simulation we used a larger box (blue in Fig.1), where results are more realistic compared to some data from the North Atlantic (Tisnérat-Laborde et al., 2013; Tisnerat-Laborde, personal communication). The radiocarbon simulation greatly improves when using the larger box as boundary conditions, hence this was used to simulate ^{14}C in the Mediterranean Sea.

This point has been also raised by reviewer #1 and it has been addressed in the revised version of the manuscript.

[See changes p 7, line 18-25 in the revised manuscript.]

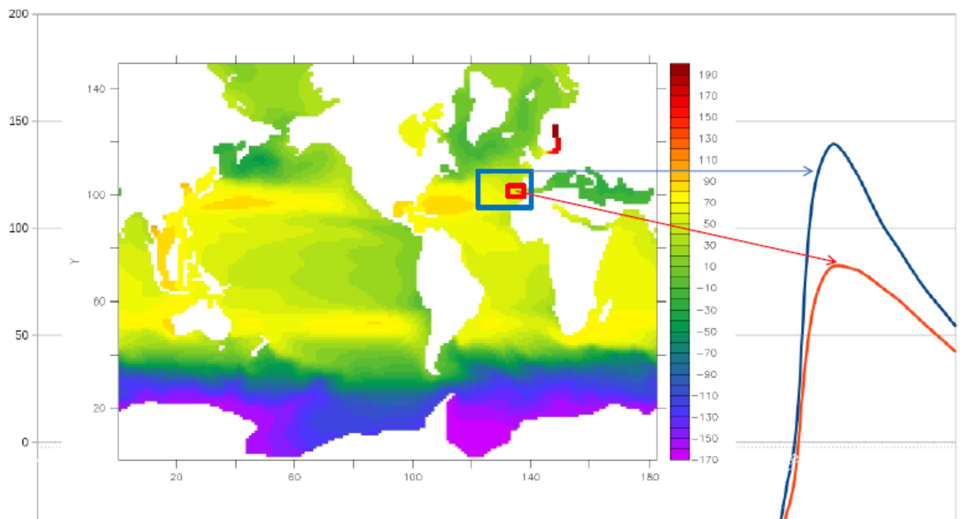

Fig. 1: The concentration of radiocarbon in the Atlantic inflow (NEMO global model, Mouchet et al. 2016).

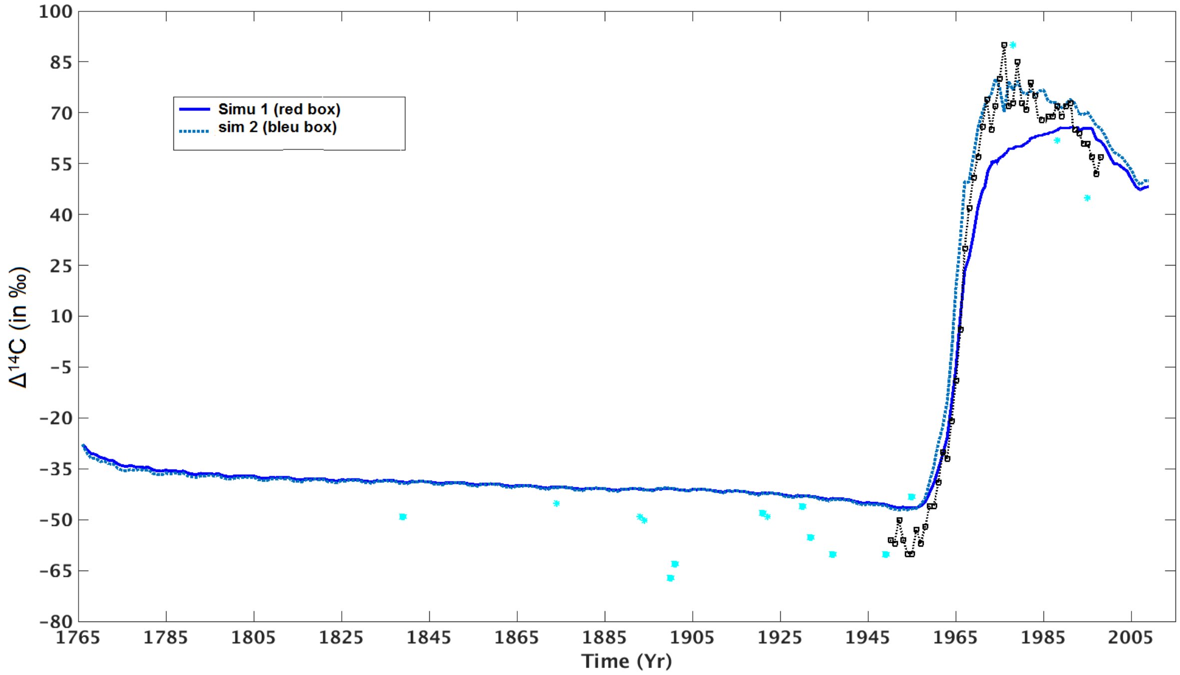

Fig.2: $\Delta^{14}C$ values (in ‰) in the Ligurian sub-basin from 1765 to 2008 for the surface water (0-10 m depth), together with available in-situ observations (Tisnérat-Laborde et al., 2013) from coral (black dashed line). Simulated data obtained using the smaller box (blue dashed line, simu 2), and the larger box (blue line, simu 1).