We thank Referee #2 for constructive comments, which have helped improve the manuscript. The structure of our reply is as follows; each comment from the anonymous reviewer is recalled in blue, and our reply in black.

The validation of the model results against available observations is merely qualitative while a quantitative model performances evaluation is necessary.

We thank the referee for this suggestion, we agree that a more quantitative analyses would be of interest. However the paper published for describing the observations (Roether et al, 2013) is also very descriptive, which not allowed to do many quantitative comparisons with the simulation. In order to make a more quantified analysis of the model results against observations we will provide additional comparison of average vertical profiles along different METEOR section (see Fig.9, example for 1999 cruises). We will provide quantified estimations of the deviation against observations, for the different water masses identified along the profiles (LIW, deep water...).

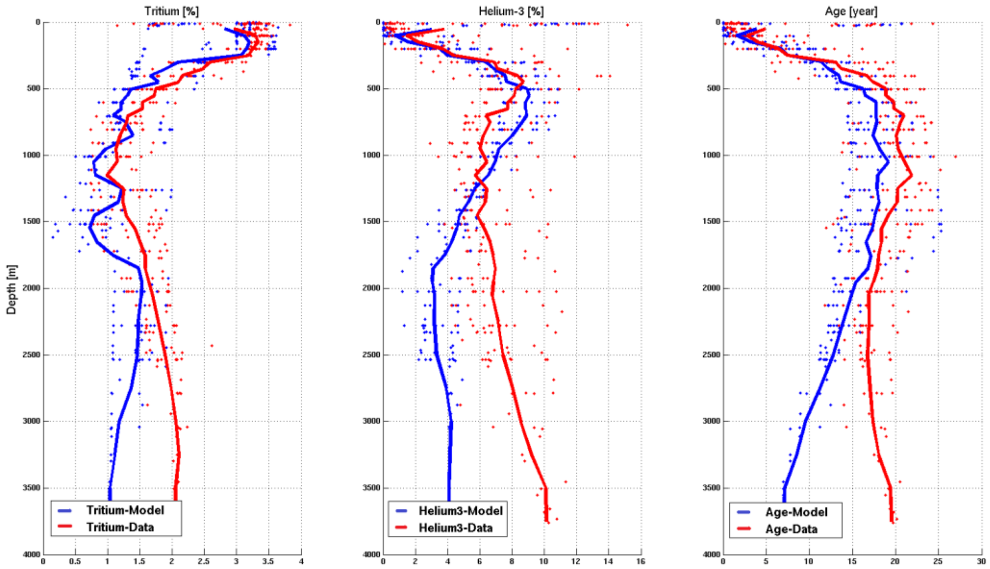

Fig. 9. Comparison of average vertical profiles along the METEOR M44/4-1999 section for (left) tritium (in TU), (middle) helium-3 (in %), and (right) the tracer age (in years). Model results are in blue, while red indicates the in-situ data.

An additional comparison with other measurements, like oxygen, should complement the provided analysis in support of the manuscript findings.

NEMO-MED12 has already been used to study the anthropogenic carbon uptake based on CFC simulation (Palmieri et al., 2015). Tritium helium-3 simulation provides an additional evaluation of the model dynamics that is useful before coupling with a biogeochemical model. We are now working on the coupling between NEMO-MED12 dynamical model with the PISCES Biogeochemical model. The proposed additional comparison with oxygen will be possible in the future, and our expertise with passive tracers (e.g. Tritium-helium) will then be very useful to analyse the results.

The quality of the figures (3 to 8) is too poor. It is difficult to follow the discussion when the figures are commented.

We agree that the resolution is too low when we show the distribution of tritium, helium, and the age for many years in the same figure, However the advantage of this kind of presentation is that we can see and compare different situations on the same figure.
The revised figure is enlarged.

Detailed Comments:

Section 3.1 The Authors state the simulation is performed “off-line”, however the details of this off-line coupling are missing. The Authors refer to Palmieri et al 2014 but also this manuscript does not provide the details of the coupling. See my general comments.
What is the frequency of the coupling? What is used for the turbulent diffusion (horizontally and vertically)? I can assume a constant horizontal turbulent diffusion coefficients, but vertically the Authors mention the TKE scheme. Most of the vertical processes investigated (or validated) in the manuscript, like ventilation or overturning in general, occur at time scale often shorter than a day. This is a crucial point in the coupling exercise. What is the passive tracer equations? And how is solved numerically? I think this is also a major point to be described and discussed in details. Furthermore, due to the relatively low concentration of the investigated tracer the numerical choices could play a major role in the simulation.

In the revised manuscript, we will improve this section to be more understandable, in particular to better explain the offline approach and the looping.

Tritium-helium3 are passive tracers and do not affect the ocean circulation (as opposed to active tracers such as temperature and salinity). They can be run in an “off line“ mode. It means that a simulation is made first with the dynamical model (circulation model only) that provides velocity, temperature, salinity, and mixing coefficient fields. These simulated circulation fields are later used to simulate the transport of passive tracers such as Tritium. Offline simulations are done for computational efficiency. They allow us to run simulation of different passive tracers in pre-computed transport fields instead of re-computing them, which is very costly.

To simulate the propagation of tritium as passive tracer in the Med sea, we have used a circulation from NEMO-MED12 model (Beuvier et al. 2012a).

The transports of passive racers are driven by NEMO-MED12 physical fields using an advection-diffusion equation. Physical forcing fields are read daily, and interpolated every hour. The time step of our passive tracer model is 20 minutes.

The model resolves a transport equation, with a term representing the source and sink" for each specific passive tracer C:

$$\frac{\delta C}{\delta t} = S(C) - U.\nabla C + \nabla.(K\nabla C) \qquad (1.1)$$

S(C): the source-sink of the tracer; in our case this term represents the radioactive decay of tritium to helium3

$U.\nabla C$: Advection of the tracer in the three directions

$\nabla.(K\nabla C)$: Lateral and vertical diffusion, with the same parameterization as for the hydrographic tracers. For the turbulent diffusion (horizontally and vertically), we use the same parameterization than Beuvier et al., 2012.

The dynamical model was forced with the ARPERA forcing during the 1958–2013 period. The first 7 years (1958–1964) are considered as a spin up and are not used in the offline simulations of passive tracers. Rather, the next 10 years (ARPERA forcing during 1965–1974) are continuously repeated until 1975 to drive our offline simulations of both passive tracers from 1951 (beginning of the simulation) until 1975.
That forcing period was selected because it does not include intense events like the EMT or the WMT (Schroeder et al., 2008); we thus considered this period as best suited to produce reasonable circulation fields for the Mediterranean Sea (Beuvier et al., 2010, 2012b; Beuvier,2011). Then to complete the offline tritium-helium3 simulations, we applied the NEMO-MED12 circulation fields corresponding to the remaining 1975-2011 period forcing.

The low concentration of the tracers is not an obstacle during the simulation, the advective scheme are adapted to treat this problem. The NEMO model has been tested in more severe configuration, e.g. global simulation of oxygen isotopes run for thousands of year, that have demonstrated that the conservative characteristics of numerical code was satisfactory.

Section 3.2 The Authors state that there are large uncertainties on the investigated passive tracer concentration in atmosphere and thus pseudo-observed values are imposed at the ocean surface. However the uncertainties related to this approach are no discussed and, in particular, the potential impact on the final model results discussed. I suggest the Authors provide, also for the approach used, a guess of the error and thus argue, at least, on its propagation during the model simulation. No mention on the impact of lateral open boundary condition in the Atlantic or in the Black Sea (or Marmara). The Atlantic side could have a minor impact on the simulation since surface water are then intruding in the Med. I do not know about the Black sea connection because of the relatively unknown variability of the flow through the strait.

We thank the referee for this suggestion, it would have been very interesting and very useful to estimate the error of tracer propagation during the model simulation, but that requests to run many simulations, which is very expensive and not affordable with the high resolution resolution of our model (1/12°). Many previous study uses this same approach (e.g. Arsouze et al 2009, Palmieri et al 2015, Dutay et al 2002) based on the comparison between the available data and the model output, without possibility to investigate the propagation of the error in the simulation. This simple comparison allow to a better calibration, and a useful evaluation of the model performance using the many available in situ data. All the details on the methodology and the uncertainties (standard error) on the data imposed at the surface are given by Roether et al,.2013.

NEMO-MED12 covers the whole Mediterranean Sea and also extends into the Atlantic Ocean to 11°W (buffer zone). We simulate the exchange with the Atlantic through a buffer zone, and for each year we use the same surface boundary condition for tritium as in the WMed. The incoming surface water from the Atlantic has a significant impact on the simulation, but it is reasonably well simulated since we use the information from in-situ data to constraint the model in the surface layer.

In the dynamic run the Black Sea is not explicitly represented by the NEMO-MED12 model. Instead, the water exchanges between the Black Sea and the Mediterranean Sea are considered, in the model, as a river (surface freshwater input) located at the Dardanelles strait, that water flux corresponds to Dardanelles' net budget estimates of Stanev and Peneva (2002), as done in Beuvier et al. (2010, 2012) and Palmieri et al (2015).

The levels of tritium in Black sea are very poorly documented; we have thus no information on the tritium flux from the Black Sea. No flux of tritium from the Black sea is then prescribed in our simulation.
Additional explanations aimed at clarifying these different issues raised here by the reviewer have been included in the simulation.

Section 4.1 Provides a descriptive evaluation of the model results. This is a nice exercise explaining the potential usage of this passive tracer to study the Mediterranean thermohaline circulation. However the depicted structures and dynamics are not new.

Although the depicted structures and dynamics are not new, the tritium-helium3 simulation are new results that provide an additional and independent diagnostic , which allow assessing in more details the time scale of the ventilation processes simulated in NEMO-MED12. More specifically, the derived tracer-age estimates represent an additional diagnostic on the time scale of the water masses renewal that is not available with the hydrological tracer (T, S).

This evaluation is fundamental before using NEMO-MED12 as dynamical components in coupled models (NEMO-PISCES/Eco-3M) for studying the evolution of the climate and its effect on the biogeochemical cycles in the Mediterranean Sea, and is essential to improve our ability to predict the future evolution of the Mediterranean Sea under the increasing anthropogenic pressure.

Section 4.2 What are the values shown in Figs 7 and 8, daily mean? Monthly averages?Yearly? Synoptic with the observations? Please include the details. To my understanding the good or bad model performances in the surface layer are related to the surface imposition of the passive tracer concentration, could please the Author better comments on this point? In general the model results validation method is poor. The Author present only a qualitative comparison with available observations and also in this case few possible explanation for the model failures are provided. In my view this should be the core of the present scientific exercise and a detailed quantitative evaluation should be also included.

Colour filled contours represents simulated tracer concentration (tritium, helium3, and the tracer age), whereas colour-filled dots represents in-situ observations. Both use the same colour scale and are taken at the same period.

The values shown in Figs 7 and 8 represent the monthly values, which corresponds to the in-situ data from different Meteor cruises: M5/6-(Sep 1987); M31/1-(Jan-1995); M44/4-(April-1999); M51/2-(October-2001); M84/3-(April-2011), and Poseidon 232-(November-1997)
We will add these details in Figs 7 and 8.

For the quantitative evaluation, we will add a comparison of average vertical profiles along different METEOR section (see Fig. 9, example for 1999 cruises), and provide a quantitative comparison between model and observation along those vertical profiles allowed the identification of the main water masses present in the Med sea (like the LIW).

Referee is right; the model produces realistic performance in the surface layer, because the concentration is imposed from observations. However we also show in the paper that the vertical penetration of the signal from the surface is realistic, indicating that the mixing and dilution with subsurface water is satisfactorily simulated.

Section 5. Page 2710 lines 9-10-11 “Several available observations along large-scale sections allowed a careful evaluation of model performance in the Mediterranean Sea on a decadal time scale.” Based on my previous comments, I would rephrase the sentence.

We will change this sentence in the revised manuscript.

Page 2070 lines 12-17. “Severe mismatches between model and observations are clearly associated with shortcomings in model physics, otherwise this parameterization led to realistic values of the tracer distribution in the water column. These results suggest that this approach is appropriate for generating a tritium simulation sufficiently valid to evaluate model performance on decadal time scales in the Mediterranean Sea.” I do not agree with this sentence. It could be true but this has not been demonstrated with the present methodology. No sensitivity to passive tracer initial or surface boundary conditions has been performed which is mandatory in order to distinguish between the different sources of errors.

Clearly, the large differences between model and observations that we point out in our study cannot be associated to uncertainties in our boundary condition (e.g. low representation of AdDW, and the low southern propagation of the simulates WMDW), and therefore must be attributed to problems in the model dynamic. Moreover, our conclusions are supported by the CFC simulations (Plamieri. et al 2015), which point to identical shortcomings of the model dynamics.

The additional quantitative comparison between data and the simulation (Fig.8 and 9, in the revised manuscript) support the conclusion of the present methodology.

Page 2713 lines 15-25. The discussion about the mechanism of the EMT should be carefully addressed here. Since the Author are modifying the surface forcing in order to obtain the correct model response, I think they are forcing a specific process. This can be correct or not, by the present time there is not enough evidence that this could be the only mechanism to simulate the event. Input from the Marmara Sea or other, not represented, physical processes could play a significant role.

As detailed by Roether et al.(1996, 2007), the EMT was a temporary change in the Eastern Mediterranean Deep Water (EMDW) formation that occurred when the source of this deep water switched from the Adriatic Sea to the Aegean Sea during 1992–1993.

Beuvier et al.(2010) showed that a previous simulation with the circulation model NEMO-MED8 (1/8◦ horizontal resolution) was able to reproduce a transient in deep-water formation as observed for the EMT, but the simulated transient produced less EMDW.

Beuvier et al.(2012b) later made a simulation with NEMO-MED12 with comparable forcing between October 1958 and December 2012. To improve the characteristics of the simulated EMT, namely the density of newly formed EMDW during 1992–1993, its weak formation rate, and its shallow spreading at 1200 m, they made a sensitivity test with modified forcing. For that, they modified the ARPERA forcings over the Aegean sub-basin, increasing mean values as done by Herrmann et al. (2008) to study the Gulf of Lions. More specifically, during November to March in the winters of 1991–1992 and 1992–1993, they increased daily surface heat loss by 40Wm−2, daily water loss by 1.5mmday−1, and the daily wind stress modulus by 0.02Nm−2.

That resulted in average winter time increases in heat loss (+18%), water loss (+41%), and wind intensity (+17%) over the Aegean sub-basin. The increased heat and water losses allow NEMOMED12 to form denser water masses in the Aegean Sea during the most intense winters of the EMT, while increased wind stress drives more intense mixing via winter convection. Furthermore, enhanced convection accelerates the transfer of surface temperature and salinity perturbations into intermediate and deep layers of the Aegean Sea.

In our study, we have made a sensitivity test by comparing this tow forcing from Beuvier et al. (2012). Although this may not be the only way to produce a more intense deep water formation in the Aegean sea during EMT event, however, this modified forcing improves the propagation of tracer into the deep water during the EMT event. Based on this comparison we choose to use the modified forcing in our simulations.

The mechanisms generating EMT are still not fully understood and is still in debate (e.g. see Nittis., et al 2003; Vervatis et al., 2013), and it is not the scope of this paper to elucidate this question. Rather we consider to investigate the effect of EMT on the ventilation of the water masses and their redistribution in the basin.

For this modelling effort we need a circulation that simulates a realistic EMT. Beuvier 2011 (used in Palmieri et al 2015) proposed a solution for generating more intense deep water formation during EMT by modifying surface boundary conditions. In the final version of the manuscript we will more clearly present the current knowledge on EMT mechanism and also state more clearly that Beuvier et al.,2012 only proposed a solution for generating more intense EMT that is not unique.

We have also discuss in more detail different hypotheses concerning the preconditioning of the EMT and its timing in our response to reviewer #1

In the introduction section additional references to recent climatic studies exercise could be included:

S. Gualdi et al. 2013. The circe simulations: Regional climate change projections with realistic representation of the mediterranean sea.

K. Schroeder et al 2012. Circulation of the mediterranean sea and its variability

N. Pinardi et al 2014. Mediterranean Sea large-scale low-frequency ocean variability and water mass formation rates from 1987 to 2007: A retrospective analysis

We would like to thank you for the mentioned references; we will introduce the references in the introduction section.

Page 2699 line 3-5. I would carefully state that a 1/12 Ocean model permits the representation of Mediterranean Mesoscale especially referring to the Roosby radios of deformation. This characteristic length scale in the Mediterranean basin is quite variable spatially and temporally and in some of the Mediterranean Regions, like the Adriatic See, is of the same order of the model resolution. I would suggest to include some details as the seasons and the regions where the present resolution could be enough to represent the mesoscale.

We thank the referee for this suggestion, you are right; this scale permits representation of mesoscale features, however all these details including a comparison with the first Rossby deformation radius in the Mediterranean Sea are already presented by Beuvier et al., 2012a.

Page 2699 line 29. Even if the Authors refer to previous studies it would help to include details on model set-up and parameterizations. For instance, what is the reference SST dataset used for the relaxation? And the relaxation coefficient?

For the surface temperature condition, a relaxation term toward ERA40 Sea Surface Temperature (SST) is applied for the heat flux (Beuvier et al., 2012a). This term actually plays the role of a first-order coupling between the SST of the ocean model and the atmospheric heat flux (Barnier et al., 1995), ensuring the consistency between those two terms. The value of the relaxation coefficient is spatially constant and taken equal to - 40 $W.m^{-2}.K^{-1}$, following (CLIPPER Project Team, 1999). It is equivalent to a 1.2-day restoring time scale for a surface layer of 1 m thickness.

These additional information have been included in the revised manuscript.

Lastly, we thank the anonymous reviewer again for the helpful comments towards improving the manuscript, and look forward to comments on a revised version.

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