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Interactive comment on "Modelling of the anthropogenic tritium transient and its decay product helium-3 in the Mediterranean Sea using a high-resolution regional model" *by* M. Ayache et al.

Anonymous Referee #2

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General comment

The manuscript describes an intermediate-high resolution modeling exercise of the spreading and decay of tritium in the Mediterranean Sea in the past 50 years, mostly with the intent to validate the Mediterranean circulation model skills in simulating ventilation/formation and spreading of the Mediterranean Water Masses.

The manuscript is in general well organized however there some major deficiencies I think should be corrected prior to publication.

- The description of the experiments set-up is poor. The 'off-'line' coupling technique is not explained with the required details. The frequency of the coupling; the temporal averaging of the NEMO model fields (if any); what is used for the sub-grid scale parameterization in the passive tracer equation; the numeric of the passive tracer equation. All this aspects are relevant to understand the general validity of the results, the applicability of the method and to identify potential source of error or uncertainties.

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

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

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

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.

Section 3.2 The Authors state that there are large uncertainties on the investigated

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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.

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.

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.

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..

Page 2070 lines 12-17. “Severe mismatches between model and observations are clearly associated with shortcomings in model physics, otherwise this parameteriza-

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tion 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.

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.

Minor Comments:

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

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

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details as the seasons and the regions where the present resolution could be enough to represent the mesoscale.

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?

Interactive comment on Ocean Sci. Discuss., 11, 2691, 2014.