Review of the paper entitle:

Thermohaline properties in the Eastern Mediterranean in the last three decades: is the basin returning to the pre-EMT situation?

by

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This manuscript dealing with an important scientific issue related to the Mediterranean circulation, in particular it analyzes and manages a consistent number of in situ observations in the Eastern Mediterranean Sea (EM), representing the most updated analysis for the whole EM, revealing also that the thermohaline properties in the study area are still far from those observed in the pre-Easter Mediterranean Transient (EMT) phase more likely dated in the 1987.
However more precisely the authors concentrate their analysis on three areas: Cretan Sea, Central Levantine and Central Ionian and on two oceanographic cruises carried out in April and June 2011, revealing that the imprints of the EMT is still present in the deep layers.

The analysis performed by the authors is standard and adequate to the final aims of this manuscript, which content is essentially a description of CTD data with a small analysis on the decadal variability that is actually confined mainly in section.4, where there are also more interesting results.
I have read and studied the manuscript with interest, but has been very difficult to me to understand where and what are the new results respect to the a plenty of papers in the same field, also already published by the same authors in the last few years, this manuscript for this reason is a little more than a review paper about a well know phenomena of EMT, BIOS, etc., moreover this paper don't make the BIOS process more robust than before, leaving it, in my opinion, a process limited in time and not well identified in the past observations, in this context could be useful a more accurate comparison with oldest data set (MEDAR_MEDATLAS) or using analysis of modeling data for the last 60 years (say from '50 to now) and finally more discussion on the scientific results obtained.

From my point of view one strategy very useful for increase the impact of this manuscript could be to give to the manuscript a different vision or if you want a different strategy; starting from a consideration that the real new data discussed in the manuscript are very few, the new results could be derived by focusing on the question of the water transformation at deep and intermediate level and considering all the limiting factors that determine its variability at inter-annual and decadal/multi-decadal time-scale.
Considering all the pre-condition factors that can determine the loss of buoyancy: advection of fresh-water (this is the real deep meaning of the BIOS in this context), advection of salt, external forcing (heat fluxes, wind stress etc.) and the last but not the least all the bathymetric constrains, in the special sites where the

convection is more active, that the authors know very well.

So in conclusion the section 4 should be the core of the manuscript, while all the previous sections have to be included part in the introduction and part in the discussion and conclusion. Consequently, the manuscript will demonstrate how much is complex and extremely non-linear the behavior of the Eastern side of the Mediterranean sea and how much there is still to do before we understand the mechanism behind it, finally this will throw new light in the shadow on the Mediterranean mode of variability and in particular those related to the non-linear interaction between the ocean and the atmosphere and those related to the internal ocean variability.
In general, my personal feeling is that the authors, to make this manuscript a real new paper, have to spend more time and effort in a new direction.
In conclusion, for all these reasons that the results of this paper even if could be very interesting for the oceanographic community and in particular for those more implicated in the Mediterranean climate studies, however the present version has some deficiencies that have to be overcome before the publication.

Specifically the authors have to improve the manuscript addressing the following questions:

1. Section 1 Introduction

Line 17-18 page 395

The real aim of this paper isn't " *The aim of this paper is to study the evolution of the thermohaline properties of the EM during the last two-three decades....* (this was done in the previous papers, which are almost cited in the manuscript)" but more precisely " *is to study ..property evolution of the AdDW, the EMDW, and the LIW.....the central Ionian and the central Levantine,....*"
is crucial in this paper to understand what is real new and what is largely a review of already published papers;

2. Section 3 Thermohaline conditions

Line 25-28 page 400:

Here there are two very interesting analysis: why the authors don't exploit it with more analysis, for example why we observe and double inversion in the T-S diagram, we have simultaneously more than one Deep Waters, in same sense both active, one driven by salt and the other one in temperature? and again is the entire water column (until 4000m) involved?

Line 21 to the end of the page 401:

This site (south Adriatic) is very important, actually is the site where the source water of the deepest water layers of the Ionian sub-basin is formed, is necessary more discussion, and a figure than include also the temperature (in Fig. 5); moreover why the temperature is so cold at 800 meter and why the authors declare (line 24 same page) that this water don't interact with the LIW, how this water then became warmer? instead I think that here there is a strong mixing and is important that the author make more analysis in this special site;

Line 15 to the end pag.402:

Looking the Fig. 6, seem to me that the front of AW is more large, grossly speaking from 36N to 40N and the flowing of AW isn't limited only by the anticyclone located at 19E, its work like a sorting point from which the AW splits in two branches: one to east and the other ones to the north, as also the authors explain very well in other part of this manuscript.