We would like to thank the reviewer for her/his constructive criticisms and helpful suggestions.

Answers to the general comments:

Point 1) and 2):
"1.The paper aims at putting the results of two oceanographic basin-wide cruises in the context of the Mediterranean circulation and its evolution. I think at least that only that aim of the paper has sense instead of a mere description of results.

However, in order to do that one has to describe rather well long-term Mediterranean circulation variability and sub-basin scale features. The paper does not succeed to do that since, apart from the rather poor description of EMT no other decadal variability has been mentioned. Here I refer to the BiOS mechanism in the Eastern Mediterranean (see e.g. Borzelli et al., 2009, Gacic et al., 2011, Gacic et al., 2013) and the Western Mediterranean Transition – WMT (Schroeder et al., 2009)."

The paper aims primarily at analyzing observations (water mass characteristics, in situ velocities) from two oceanographic basin-wide cruises carried out during 2011. A more detailed description of specific mechanisms and modes of long-term variability constitutes the focus of different papers to appear in the special issue of "Physical, chemical and biological oceanography of the Mediterranean Sea" for which also our contribution was conceived. However, putting our observation in the context of the known Mediterranean circulation and its variability is a very challenging task and, in accordance to the suggestions of the reviewer, we now extend the discussion to such important aspects, including a broader description of the so-called "BiOS" oscillation and Western Mediterranean Transition (WMT). We now also quote the excellent work of Borzelli et al. (2009), Gacic et al. (2011), Gacic et al. (2013), and Schroeder et al., 2009.

Point 3):

"Effects of the thermohaline changes observed in the Adriatic can be quantified only by knowing the present BiOS phase i.e. Ionian basin-wide circulation mode. Therefore, only by understanding in which phase were these two decadal variabilities one can interpret the cruise results in 2011. As far as the sub-basin scale features in the specific period of the campaign are concerned, detailed description can be given analyzing altimetric data which are normally available to oceanographic community."

Following the suggestion of the reviewer, we now present a discussion of observed mesoscale and large scale oceanographic features based on the analysis of altimetric data.

Point 4):
"In addition, in the abstract authors specify that one focus of the paper was to compare the geostrophic velocities and ADCP data which cannot be self sustained scope of the paper."

We also now better elucidate, starting from the abstract, the goals of our investigation.

Point 5):
"The main weakness of the paper is that authors interpret collected data in a "twodimensional" fashion, i.e. horizontally along the transect neglecting the fact that the Mediterranean is strongly asymetric. These considerations could have been done at least in the Ionian from the Poseidon cruise and so some horizontal maps of the principal water masses such as LIW and AW is essential for understanding the BiOS mode. This would help very much in determining water mass pathways which is not necessarily along the trans-Mediterranean section."

We now better address, within the paper, the fact that sections like the one we present do not allow to resolve the three-dimensionality of stratification and velocity distributions in the Mediterranean Sea. As stated in Point 4) we now integrate this discussion using an analysis of altimetric data. Obviously, we have to note that it is inherent in ship-based in situ observations that important aspects of the oceanic spatial and temporal variability cannot be resolved.

Point 6):

"Comparison of the geostrophic velocities and ADCP measurements keeping the reference level equal in both sub-basins (EMed and WMed) does not justify the conclusion that in the WMed the large-scale flow is mainly geostrophically balanced while in the EMed the ageostrophy is much more important. The differences can simply be due to the fact that sub-basin scale features in the EMed are deeper than those in the WMed."

We performed a sensitivity study in order to assess the variations induced by changing the reference level on the calculated geostrophic velocity and we could conclude that, keeping such level around 1000 m represents a rather stable choice. This fact is now stated in the revised version of our manuscript. Also we now better elucidate that, in general, the observed sub-basin scale features in the EMED are deeper than those in the WMED. We also now better specify that a comparison between observed and calculated velocities gives an assessment of the relative magnitude of the ageostrophic velocities.

Point 7):

"Authors should use the ADCP measurements at a certain depth as absolute velocity, then add to this value the geostrophic shear and see to what extent geostrophically sheared absolute velocities differ from ADCP measurements at other levels determining the ageostrophic contribution"
Further investigations on ADCP velocities were not performed due to the rather high uncertainty related to these observations.

Point 8):

"Another important shortcoming of the applied geostrophic method was that authors were able to compute only the vertical shear of the geostrophic component perpendicular to the section."

See our answer in Point 5).

Point 9):

"In the Abstract authors repeat the same statement with a slightly changed wording: "The focus of our analysis are the water mass properties, also in the context of the recently observed variability, and a comparison between the velocity fields observed using a vessel-mounted ADCP and those calculated from the observed density fields." and: "Here, our focus is a discussion of the observed water mass properties analysed through T –S diagrams and through an Optimum Multiparameter (OMP) analysis. Additionally, ADCP velocities are compared to geostrophic calculations."

Following the suggestion of the reviewer, we now reformulated the abstract.

Point 10):

"Authors have to be more coherent in using the water mass acronyms as presented in: https://www.ciesm.org/catalog/WaterMassAcronyms.pdf . They for example use for Atlantic Water MAW in one part of the paper and in the other AW."

We now use a more consequent classification.

Point 11):

"*The sentence: "As a consequence, which adds to the differences in bathymetry as well as forcings existing between the two sub-basins, the WMed and EMed show distinct differences both in their hydrographic conditions and circulation." cannot be understood, please reformulate."*

We reformulated that sentence.

Point 12):

"Page 2402, line 6: Deep waters in the WMed did experience large changes because of the preconditioning thermohaline variations and the event was named as WMT – Western Mediterranean Transition (see various papers by Schroeder, and Gacic et al., 2013)."

We now included a description of the WMT and quote those papers.

Point 13):

"As said in the line 4 it seems that BOUM was restricted to specific area."

It is now explicitly stated that BOUM is one of the cruises which were not restricted to a specific area.

Point 14):
"*The major goal of the cruises in the Northern Ionian and S. Adriatic should be, if authors took into consideration the central role played by the Ionian in driving the Mediterranean overturning circulation, to analyze the present state of the Ionian basin-wide circulation and BiOS and its effect to the AdDW production."*

As stated above, a detailed analysis of the state of the BiOS is not the main topic of our paper and it was not the main goal of our cruises. Such analysis certainly deserves much attention and will be very probably the goal of future observations. As far as our paper is concerned, we now better discuss BiOS throughout the paper.

Point 15);
"Theocharis and not Teocharis;"
We now present the corrected name.

Point 16):
"Page 2408 – 2409: These long considerations of water mass characteristics are rather tedious. Please summarize somehow!"

We now reformulate and, somehow, summarize those considerations of water mass characteristics

Point 17):
"*Page 2410: "During the EMT, this kind of shape of the T –S diagram ("peaking") was prominent in the Ionian Sea only in the northern part of the Eastern Mediterranean Ridge, the principal entrance for water of Aegean origin (Roether et al., 2007). Remarkably, now it can be found in the whole Ionian Sea (Fig. 5b), but it is characterized by a much less pronounced range between minima and maxima." The fact that in Roether et al. (2007) there was no signs of the "peaking" in the Ionian can simply be due to the fact that Roether's data do not have such a good synoptic coverage of the Ionian (bi-dimensionality versus three-dimensionality! and non-synoptic data in Roether et al.) which is a very important aspect considering the BiOS variability."*

We now explicitly state a possible explanation in accordance with the suggestion of the reviewer.

Point 18):
"Page 2412: Map of horizontal distribution of contribution of various water masses in the Ionian should be very useful, considering mainly LIW, AW and AdDW."

We now add a discussion on possible causes of the observed asymmetry.

Point 19):
"Larger departures from the geostrophy in the EMed should somehow be explained, assuming obviously that the reasoning of the constant 1000-m reference level for the whole Mediterranean is valid (wind forcing!?)!"

See our answer in point 6. Apart from that it is not clear what the reviewer wants to express with his/her comment.

Point 20):
"Please make this sentence less generic: "In the EMed the typical post-EMT temperature and salinity inversions were found in the deep waters throughout the whole region, still indicating an increase in property distribution."
We reformulated that sentence.

Point 21):
"Page 2414: Statement: "...whereas LIW is filling the intermediate depth levels with a core in the eastern part of the WMed." is again a consequence of the "2D reasoning" in this paper!"
See our answer in point 5

Point 22):
"Page 2415: Why in the WMed in contrast with the EMed "current field is more geostrophically balanced and the structures observed can be related to known sub-basin scale gyres"? EMed is populated by number of sub-basin scale gyres thus this cannot explain the differences between the two sub-basins!"
We reformulated that sentence that was indeed not clearly expressed.

Point 23):
"Likely, the effects induced by the EMT are still evident in the Mediterranean Sea, and considering the time delay of many years which characterizes oceanographic processes on a basin scale, we could expect that they still will be

present for some time." Very generic statement, please be more specific or delete it!"
We deleted this sentence.