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Interactive comment on “On the relationship among the Adriatic–Ionian Bimodal Oscillating System (BIOS), the Eastern Mediterranean salinity variations and the Western Mediterranean thermohaline cell” by M. Gačić et al.

Anonymous Referee #2

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In my view, the principal result of this manuscript is flawed by poor statistics. The authors claim there is a statistically significant correlation between Straits of Sicily salinity and Levantine salinity 17 years earlier. The correlation is based on time series of Straits of Sicily salinity and Levantine salinity that overlap only by 15 years: Straits of Sicily time series in Figure 2 and 3 extend from 1986 to 2010, while Levantine time series extends from 1946 to 2001. With standard correlation calculations for time series of only 16 years of overlap (1986–2001), a statistically significant correlation at a time lag of 17 years is impossible. In fairness to the authors, they are relating the 1990 peak to

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the high Levantine salinity in 1974 and the 2009 peak to the high Levantine salinity in 1992. They should make that argument (and there may be statistical grounds for doing such one-sided correlations). But standard correlation calculations applied to the 16-year period of overlap between the records would likely show maximum correlation at -1 or -2 year lag. The authors do not calculate these correlations because there is a bias in their mind that there should be a positive lag. A major revision of the statistics presented in this manuscript is needed before it can be published.

The title of the manuscript must be changed. The title promises that the relationship between the BiOS, eastern Mediterranean salinity and western Mediterranean thermohaline cell would be explored. In fact, the manuscript is principally about the Straits of Sicily salinity time series. It is an excellent, long time series, as shown in Figure 2. The manuscript would be worth publishing just for the presentation of this time series. I see two dramatic maxima in 1989-1992 and then in 2007-2009. From the common time period, I conclude that the high salinity in Sicily Channel occurs at about the same time as the high salinity in the Levantine. My hypothesis would be that the diversion of low salinity AW into the Ionian corresponds to the period of export of high salinity waters through Sicily Channel and with higher salinities in the Levantine. That is my conclusion from Figure 3 and I would carry out the correlation calculations to show that there is a maximum correlation with about a one-year time lag.

Many of the Conclusions are only weakly supported in this manuscript. Those relating to BiOS mechanism must be based on analysis in other papers for there is no BiOS index, nor Levantine-Ionian correlation analysis, made in this manuscript. The conclusion for a nine-year 'propagation' from Straits of Sicily to the formation of western Mediterranean deep water is based solely on the observation of new deep water formation in 2005, which is clearly dependent on the exact year when a severe winter occurs and leads to deep water formation. All of these major flaws need to be addressed.

Technical issues:

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line 164, I would say the LIW changed and then the EMT happened. I do not see any evidence in the literature that the EMT changed the LIW.

lines 112-124, The motivation for development of the time series was to represent the amount of AW in the Ionian and Levantine basins. Later in the manuscript the (negative) amount of AW in the Levantine is called LIW. I did not understand how or where the switch in definition and usage arose.

line 269, The calculation of 'extra salt imported from the EM' is not defined. Please describe exactly how it is calculated.

In summary, I urge the authors to submit a thoroughly revised manuscript with a new title that uses the long Straits of Sicily salinity time series to best advantage.

Interactive comment on Ocean Sci. Discuss., 9, 2561, 2012.

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