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Interactive comment on "The Mediterranean Outflow in the Strait of Gibraltar and its connection with upstream conditions in the Alborán Sea" *by* Jesús García-Lafuente et al.

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Review of the paper entitle:

"The Mediterranean Outflow in the Strait of Gibraltar and its connection with upstream conditions in the Alborán Sea" by Jesús García-Lafuente, Cristina Naranjo, Simone Sammartino, José C. Sánchez-Garrido, Javier Delgado

The manuscript tackle an important and relevant scientific issue regarding the analysis of observations downstream and upstream the Gibraltar Strait; in particular in four different sites (Espartel Sill (ESP), Camarinal Sill North (CSN), Camarinal Sill South (CSS) and in the Western Alboran Gyre (WAG) through the proxy (deduced by altimetry

data), in order to evaluate a connection between the strength of the WAG and the hydrological composition of the Mediterranean Outflow (MO).

This study is very interesting because increase understanding on the dependence expected between the variability of the two different ocean regions that the Gibraltar Strait connects (Gulf of Cadiz-Tangeri Basin (TB) and Alboran Basin respectively).

The analysis of the connection between the two side of the Strait is based on the study of the physical mechanisms that drive the variability of the MO; focusing on the main ocean phenomena observed in Gibraltar Strait, like mixing between different types of water of Mediterranean and Atlantic origin. The present paper is also relevant for ocean climate variability studies and in particular for research and simulation of the interaction between Mediterranean Sea and North Atlantic ocean.

For all these reasons that the results of this paper are very interesting for the oceanographic communities and in particular for those scientists more implicated on the Mediterranean-Atlantic interaction or on its parameterization in numerical climate models. Therefore I recommend this paper to be published, almost in the present version, however I would like to give some general comments and minor revisions in order to increase the impact of this manuscript around the oceanographic (and not only) community.

AC: Thank you for your opinion and the points raised in your review, which we address below.

General comments:

The scientific matter of the manuscript isn't a really new augment, actually in the literature there are many example on this, either in the modelling field or like in this case in the analysis of the in situ observations. But the novelty of this manuscript (at least in my knowledge) is on the very detailed analysis of the hydrological characteristic in the key points, like CSN and CSS and ESP, crucial on determine the interaction, at

different scale, between Mediterranean Sea and the Atlantic ocean.

However I have some doubts on the efficacy of the proxy (the altimetry height) used for analyse the WAG strength and its variability. I think that the matter of this manuscript is a little bit more complex. Is matter of fact that the inflow/outflow is regulated not by a "single gyre" (WAG) but from the "double gyre". Actually, from satellite observations and from numerical simulations (see references 2 to 6 below), the Alboran Sea is dominated by the 'double gyre' system and the Almeria-Oran front could be a good "proxy" of the variability of this system and consequently of the variability of the Atlantic water enters the Alboran via the straits of Gibraltar. Long-term monitoring of these currents is possible using data from satellite altimetry and finally will represent better the complexity of the processes of which the authors of this manuscript want to connect at the variability of MO.

Moreover, correctly in the manuscript the authors investigate in details on the mixing that take place inside and outside the Gibraltar Strait and the Almeria-Oran front again is also a proxy of these events, because is link also to the hydraulic condition along the Strait.

AC: As far as we know, the likely relationship or link between the Almeria-Oran (AO) front and the hydraulic condition in the Strait, was first put forward by Garret (1996, "The role of the Strait of Gibraltar. . ..." in Dynamics of Mediterranean straits and channels, CIESM Science Series n°2), and connects with the concept of an overmixed Mediterranean proposed previously by Bryden and Stommel (1984). As you know, this general relationship comes from a simplified conceptual model, yet sensible and enlightening, which, when applied to the actual ocean, shows some weaknesses. Numerical models suggest that most of the time the flow in Gibraltar is hydraulically controlled (except for tidally-induced control flooding, which happens in the short time-scale), but the presence of the AO front is not so regular and permanent, experiencing dilated periods of time during which the front is not present or, at least, not identifiable (the very Report by Lanoix mentioned in your reference list provides an illustrative example). Therefore,

search for an external AO-front-based proxy is not the best choice to establish a reliable link with the Strait of Gibraltar hydraulics. In any case the remark you pointed out is quite interesting since, as you mention, the complete Alborán Sea system includes the two anticyclonic (western, eastern) gyres and the AO-front at the eastern boundary. Expectedly, the whole system influences the exchange. However, when looking for a proxy that represents this system, we finally opted for the Western Alborán Gyre (WAG) for three main reasons: 1. Being the nearest structure to the Strait, the WAG is expected to have a greater influence on the exchange. 2. As repeatedly mentioned in the literature (Vargas-Yañez et al., 2002, for instance), the WAG is a quasi-permanent feature while the Eastern gyre is more elusive (Garcia-Lafuente, 1998). The WAG is identifiable almost all year round, although there are exceptions occurring during some periods when it collapses and disappears (Sanchez-Garrido, et al., 2013), exceptions that are of the greatest interest to our work. 3. Last but not least, choosing the WAG allows us to define a rather simple and intuitive proxy from altimetry, even if the WAG is weak, while including all the mesoscale structures of the Alboran Sea complicates the definition of a representative index for our purposes and would introduce uncertainty about the proxy utility.

The authors know very well (they wrote many papers on that) that the hydraulic control produces a hydraulic jump (forced by the tide) and consequently more vertical mixing is enhanced. These events in my opinion can modulate also the variability of the MO at larger frequency respect to the tide.

AC: We are aware of the great tidal induced mixing in the hydraulic transitions within the Strait, a fact that has been already commented on the manuscript (see Figure 2 and comments on that in pages 5-6). You are right mentioning that this short time-scale mixing may induce low-frequency signals in the outflow if one of the water mass involved (it shall be NACW in our case) changes seasonally. Although very intuitive and, probably somewhat redundant, we have opted for modifying the text and including a short sentence (lines 20-25 in pag. 15 of the new version) to mention explicitly this

process.

The second suggestion that I would like to put on the table for further discussion regard the Gulf of Cadiz, everybody know that when the MO pass over ESP has lost its original characteristic, taking now the properties of the source water of the MW that will be later observed in the North Atlantic, but still maintaining the memory of the originated Mediterranean water, in fact, following Fig. 4 of Fusco et al, 2008, is very evident the impact on the MO hydrological value of the quasi-periodical extraction and evacuation of WMDW from the Mediterranean into the Atlantic, that is the matter of this manuscript. Therefore, should be very interesting to verify the hydrological characteristic of MO in the Gulf of Cadiz and its interannual variability in relation of those observed in CSN, CSS and ESP.

AC: Additionally to Fusco et al. (2008), the variability of the Mediterranean outflow was recently revisited by Bellanco et al. (2016) using a collection of CTD casts collected from 2005 to 2015. Both, Fusco et al., and Bellanco et al., found a salinity signal below 250 m (May-June in Fusco et al., March in Bellanco et al.) that is the most outstanding seasonal signal of the Mediterranean outflow in the Gulf of Cádiz. Despite being a remarkable suggestion, the possibility of verifying or concluding beyond any doubt that the seasonal signal of the MOW in the Gulf of Cadiz comes from the processes discussed in our work is not realistic with the available data. It rather appertains to the speculation world from which can be drawn down only after carrying on a synoptic field experiment that involves the simultaneous deployment of mooring lines along the MOW path and the sills of the Strait. Even though such experiment is not discarded for the future, presently its realization is beyond our possibilities. Notwithstanding, we think it interesting to mention this possible link, so that in the Introduction of the new version (lines 10-11, pag.3) we have made a short mention to it, citing the abovementioned authors, and we do it again in the Discussion section (lines 27-33, pag.14), a bit more extensively in this case.

Minor revision:

Fig.2 the range of the value of density is wrong; AC: Actually, Fig.2c shows sigma-theta, not density. We have revised the units and checked they are correct

Line 30-31 page 10 and line 1-3 page 11 I suggest to cut this sentence, the difference of the flow is so small that don't permit further considerations. AC: We are not sure to have understood the point. The flow difference is usually well-discernibly different from zero, confirming that the flow across the southern channel tends to be consistently larger than across the northern channel, a fact already mentioned in older works (Candela et al., 1989; Bryden et al., 1994). The interesting point is that a time comes when the difference changes sign (we admit that it is not much different from zero then), indicating that the tendency of the flow to be greater across the southern channel has been broken. And that this time coincides with a notable weakening, if not a collapse, of the WAG, what, under our hypothesis, gives a good chance to LIW for flowing out in greater volumes. Therefore this small difference, even if small, is critical for our analysis and the starting point of the discussion of the data. It is because these differences are observed that we can justify the importance of the WAG in the cross-structure of the flow through the Strait. The sentence cannot be removed.

Fig.6 using this data set could be very interesting to do a SSA (Singular Spectral Analysis) of the time series, in order to capture the main low-frequency variability of this time series and verify the occurrence of a ghost limit cycle related to a physical oscillation of the dynamical system that has generated the time series (Ghil et al. 2002).

AC: Thanks for your suggestion, in fact the SSA is an interesting tool to obtain a clean signal in noisy time series and it also gives information about the different oscillatory components in the signal. We have applied the SSA to the original temperature series that was used to obtain Fig.6 and we have found that, even if the reconstructed series results in a smoother signal compared with what we show in Figure 6A, no difference in the interpretation of the results stems from the application of this spectral analysis. Thus we prefer to maintain the previous method used in the manuscript (that is, the simpler Gaussian filtering of the original series) to avoid complicating the methodology

section without further improvement of the results.

Please find in the supplement file the formatted document of these comments and the new version of the manuscript with corrections highlighted.

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Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/os-2016-90/os-2016-90-AC2-supplement.pdf

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