

ANSWERS TO ANONYMOUS REFEREE #1

First of all, **I sincerely and warmly thank you for your careful reading of my paper and your very helpful comments.**

This being said, and even if you do not want to review a revised version of the paper, **I would appreciate receiving, either officially through the OS website or personally, with the help of the OS secretary, which will respect your anonymity and allow a more frankly discussion, your answers to my questions and your comments to the arguments I present.**

General comments *The presence in the Gulf of Cadiz and further downstream, of Mediterranean Water lying at different depths and constituting distinct well identified cores, has been focus of discussion since the years 1970's. Most authors attribute the formation of these veins to bathymetric effects in the Gulf but a few authors sustain the existence of this heterogeneity already in the Strait of Gibraltar.* Even though I recently published several papers about the Strait, I do not consider myself as a specialist of it. I am just a scientist who could be considered as a specialist of the Sea and has some specific ideas about the Strait functioning. As for “*a few authors sustain the existence of this heterogeneity already in the Strait of Gibraltar*”, hence for authors who somehow share/d some of my major hypotheses, I only know the relatively old papers by Howe (1974, 1982). **Please, so as to help me presenting an as complete as possible Background Sect., please could you list who are the “few authors” sharing my hypotheses?**

The fact is that the lack of appropriate data has maintained this uncertainty until now.

I do not share this point of view since there is no “lack of appropriate data”. Indeed, I analyze data collected in 1985-1986 (mainly in Parts 1 and 2 but also in Part 3) that are available to everybody for free since 2002, and data collected in 2009 (in Part 3) that are probably available to anybody, as myself, who did not participate in the experiment but is willing to collaborate with the participants. Even though these data sets are not ideally appropriate, the fact is that either they have never been analyzed at all (for the GIBEX yo-yo time series, at least as far as I know) or they have been analyzed with the postulate (sic) that the MO was homogeneous at the Strait entrance and within the Strait itself. **Since I do not present and use new data sets, the “uncertainty until now” is only due to the “lack of appropriate analyzes”.**

The present manuscript, which is the first part of a sequence of three, is grounded on a set of data collected in the western side (entrance) of the Strait.

It is the eastern side.

The main objective is to show that the Mediterranean Outflow is already heterogeneous in the Strait itself.

This is right and due to the fact that the most recent (2015 and 2017) papers about the Strait claim that the MO is homogeneous in the Strait itself while (only recently) agreeing that the MO is heterogeneous at the Strait entrance.

The importance of the manuscript, which is based on detailed CTD data, is not only the evidence of the heterogeneity of the outflow in the Strait but also the suggestions about the way data in the region should be undertaken in the future.

I understand this general comment as “the importance of the manuscript is significant”.

Specific comments In general, the written text is difficult to follow because it is not always clear and straightforward and would benefit from strong simplification/clarification in some places. The figures (maybe too many) illustrate the main conclusions of the manuscript, but some of them could be simplified/clarified, together with the respective captions. Maybe a figure with the bathymetric features of the whole area (entrance, exit and within the Strait) with the respective names, would help the readers not familiarized with the region.

I am obviously ready to clarify and simplify the writing as much as possible and even move more figures to “Supplementary Information”, and **I will strictly follow the Editor's recommendations.**

Page 11, Fig. 2: the light gray lines of the yo-yo time series are almost invisible; the boxes with information within the figure make it too heavy so, in fact, as most of that information is also in the figure caption, they could be discarded.

I “discover” that colors on the screen of a PC are not exactly retrieved in a printed version of a figure and I will modify the original colors accordingly. As for the information boxes, I personally consider that a self-explanatory figure, as long as scientific information is not masked, allows easily and efficiently comparing similar figures, as long as the caption has been read once, and also allows projection of the figure on a screen for a large audience. In any case, **I will follow the Editor's recommendations.**

Line 73: what is the meaning of “northern Ocean”?

The first sentence of the Introduction (l. 66-69) introduces the names “Mediterranean Sea”, “Atlantic Ocean” and “Strait of Gibraltar”. The second sentence (l. 69-71) only uses “Sea”, and the word “Strait” is used at the beginning of the third sentence (l. 71); I thus thought/think that the term “northern Ocean” (l. 73) can only be linked to the Atlantic. Note that the “specific use of the terms Ocean, Sea, basin, sub-basin, Strait and Channel we make” is specified in the 1st sentence of the Background Sect. 2.

Lines 142-145: clarify the sentence contained in these lines

The major controversy I address is about homogeneity vs. heterogeneity of the MO. Addressing this major controversy is made difficult by the fact that there is an underlying and relatively minor controversy about what does heterogeneity mean, furthermore arguments claiming for homogeneity have markedly evolved in recent years. Indeed, while I have always claimed for the occurrence of up to five identifiable MWs at the Strait entrance, what is now strongly supported by the yo-yo time series data set (without any specific analysis), most other papers that have claimed (up to 2015) for the occurrence of only two MWs (LIW and WMDW), now (2015-2017) tend to recognize four of them. Furthermore we do not identify the various MWs in the same way, we do not agree on which MWs could be possibly identified at the Strait entrance, hence on what does heterogeneity mean.

Lines 295-297: clarify the sentence contained in these lines

In propositional logic, this is what is known as transposition. You can simply have a look at [https://en.wikipedia.org/wiki/Transposition_\(logic\)](https://en.wikipedia.org/wiki/Transposition_(logic)).

Line 339: from the observation of Fig. 1, it seems that the mixing lines are converging

Fig.1 does not represent observations but conceptual mixing lines that do converge in θ , S (and σ_θ) ranges (in fact a single point) possibly associated with a given MW. Observations are shown in Fig.2 and associated mixing lines do not converge in realistic MWs ranges: therefore, one cannot think about a “single given MW”, hence about “a homogeneous MO”.

Line 350-352: clarify the sentence “Just because. . .”

Relatively homogeneous components of the MO are identified by different sets of dots in cyan. These sets (in particular at points 6, 3, etc.) cannot be associated with unmixed MWs that are markedly cooler and saltier (e.g. Fig.4a). One must thus invoke a re-homogenization process of waters that have previously (in the Camarinal sills surroundings ... but not yet at $5^\circ43'W$) resulted from a mixing of MWs with AWs. Such a process is briefly introduced at the end of Sec. 3.1 and illustrated with more details in Part 2.

Lines 512-514: clarify the sentence contained in these lines

As illustrated by Fig.1, the mixing of two given waters (e.g. an actual NACW and a hypothetical MO) characterized by two different points on a θ - S diagram is characterized by points located in between, i.e. on the mixing line joining these two specific points. It is thus clear (from Fig.4a and 5) that the blue water cannot result directly from the mixing of the pink and violet ones. This could be possible only assuming that, where the blue water was formed, either the pink or the violet waters (assumed to be present in the zone of formation) would have been the coolest and saltiest (i.e. cooler and saltier than the actual blue values), and then would have encountered mixing (with warmer and fresher waters) more intense than the blue water itself. The blue MW that is the densest, the coolest, the saltiest (from Fig.4a, 5a, 5b) of the MWs while being possibly the shallowest (Fig. 5c) is also the youngest (less homogeneous / most θ - S spread one, Fig. 4a), hence the one formed in the nearest zone of formation (necessarily the north of the western basin). The actual fact that the densest of the MWs has been observed to be the shallowest can only be a direct consequence of its southernmost location due to a north-south tilting of the MWs, which supports my schematic diagram and understanding of the processes presented in Sect. 4.6 and Fig.19 of my Part 3 paper.

Fig. 6a: confusing figure with theta-S diagrams mixed with the density profiles; does the size of the coloured dots in the theta-S diagrams mean something? The figure caption is also very confusing and does not explain some of the things that appear in the figure (e.g., the green dot, the n3-5, the numbers 23, 10). As the following figures 6b-6f depend on fig. 6a' caption, this caption should be very clear.

I agree that the caption could have been clearer. However:

- θ - S diagrams are not “mixed” with density profiles. As indicated in the caption, θ - S diagrams are presented together with two inserts: one (in the lower left corner of the diagram) presents the latitude-longitude position (near $5^\circ40'W=5.66^\circ W$), the other (in the upper right corner of the diagram) presents the $\sigma_\theta(z)$ profiles.

-as for the colors, it is indicated in the caption that coloring is first inferred from the $\sigma_\theta(z)$ profiles in which homogeneous layers are easily identified by relatively constant σ values (vertical portions of the

$\sigma_{\theta}(z)$ curves); as indicated, specifying the color is then made according to the separating isopycnals (specified only on the θ -S diagram). Colored dots are plotted with a normal size on the $\sigma_{\theta}(z)$ profiles not to mask the small scale features that characterize these relatively homogeneous layers. They are plotted with a larger size on the θ -S diagrams to smooth these small scale features and somehow give a better and more realistic idea of these homogeneities.

-it seemed obvious to me that the profile number specified in the caption and retrieved in green within the figure (#1 for Fig. 6a) should be directly associated with the position of the green dot, furthermore I specified (l. 358) that profiles on all cross-Strait transect are numbered from south to north.

-it also seemed obvious to me, furthermore I specified the units (m/db with a specific comment in l. 321-324), that the colored numbers would be directly associated with the thicknesses of the various homogeneous layers (which can be roughly but easily checked from the $\sigma(z)$ profiles), and that n3-5 (with 3 and 5 in orange and n in black) would directly be understood as “several layers 3-5 m/db thick of orange water”).

In any case, **I will make the Fig. 6a caption clearer.**

Line 669-670: clarify the sentence “this periodicity. . .nominal bottom depth”

The nominal bottom depth has already been defined several times (l. 397, l. 485, l. 533, l. 662) to be 250-300 m and it is specified (l. 398, l. 667) that the yo-yo time series lasted ~ 23 h. The sentence specifies that “a semi-diurnal periodicity appears from the maximum and minimum thicknesses of the MWs overall layer above the nominal bottom depth”, which means that, “the thickness of the MWs altogether above 250-300 m displays maxima (near #16-22 and #42-48) and minima (near #4-6 and #27-33) that are roughly 12-13 h apart”. In my answers to the comments from Referee #2, I specify that the layer of MWs (resp. AWs) can be qualified as lower (resp. upper), just to avoid using “deep” that I reserve to the differentiation with “intermediate”, the former (resp. latter) MWs circulating sluggishly in the interior (resp. significantly along the continental slope) in the Sea and overflowing (resp. outflowing) through the Strait. Also see below my answer to your comment about l. 825.

Line 782: clarify the sentence “These two passages. . .through them”.

Such a sentence results from basic computations involving, over a given domain, the budget $E-(P+R)$ and representative parameters (as salinity) of the various inflowing and outflowing waters. For the Sea and the Strait, the budget (yearly loss of ~ 1 m of freshwater) represents the difference (~ 0.1 Sv) between the AWs and MWs fluxes; and considering typical values for e.g. S as in Fig.1 leads to fluxes in the order of ~ 1 Sv. In the case of the AWs within the Sea, i.e. without any topographic constrain, they circulate anticlockwise (due to the Coriolis effect) as alongslope currents several 10s km wide; to maintain the fluxes, AWs must thus accelerate through the narrow (~ 10 -km wide) Strait, which will increase in particular the slope of the AWs-MWs interface there. I make similar comments for the intermediate vs. deep MWs: circulating MWs (the IWs) must increase their speed when they outflow through the Strait, so that the IWs-DWs slope must increase, which rejects the overflowing DWs more to the south.

Lines 798-800: clarify the sentence “let us fill . . .Mediterranean Sea”

In the Background Sect. 2 (l. 172-175), I specify that “I never thought necessary to invoke the uplifting of deep MWs through the agency of a suction effect” as invoked in all other papers since decades (see the references I give). I always thought that DWs overflow from the Sea (or more precisely tend to flow out from the Sea without considering the IWs) just as water overflows from a bathtub having a

filled evacuation hole and an open tap.

Line 825: explain why “with a nearly null speed”

I consider that IWs are those that are continuously circulating, first alongslope anticlockwise within the Sea, then through a passage along its right-hand side where they are said (by me at least) to outflow. And I consider that DWs are just sluggishly circulating within the Sea so that they have a nearly null speed everywhere in the Sea, exactly as the bath water in the bathtub example. The DWs get a significant speed just when they flow out or overflow (according to my definitions at least), i.e. when they reach the sill, exactly as when the bath water flows over the edge of the bathtub.

Line 924: explain “a $D\theta/DS = 10^\circ\text{C}$ scale”

θ -S diagrams are necessarily plotted over some specific ranges of both θ and S with θ in $^\circ\text{C}$ and S dimensionless (according to the “practical salinity scale 1978”). The ratio of both ranges $\Delta\theta/\Delta S$ thus defines a scale in $^\circ\text{C}$. Slopes in θ -S diagrams can be compared only if scales have the same numerical value (1 in our case).

Line 991: explain the sentence “double diffusivity . . . Strait entrance”

Double diffusivity is characterized by a layering of only θ and S that leads to density (σ_θ) homogenization, hence to a smoothing of the $\sigma_\theta(z)$ profiles through some MWs (you certainly know that σ_θ is used for “potential density anomaly”). Such a well-known process has thus consequences opposed to those observed at the Strait entrance where some unknown process leads to an increased layering of the $\sigma_\theta(z)$ profiles through some MWs. To help specifying what could be such a process, I hypothesize that a layering of these MWs at $5^\circ 43' \text{W}$ larger than more upstream (i.e. at $5^\circ 40' \text{W}$ herein and even more to the East, as shown in my 2013 and 2014b papers about LIW) could be due to a tilting of the MWs linked to the Coriolis effect and to the necessary acceleration of the MO in the Camarinal surroundings.

Technical corrections In the whole text, there are several cases of wrong letterings (normal instead of symbol) for the potential temperature (q instead of θ) and potential density anomaly (Sq instead of σ - θ)

I am sorry but I did not check enough the conversion of my docx files into pdf ones and I did not realize that errors occurred in converting the Symbol format only in the end of my files (in this paper after l. 289-302 only) and in a very strange way, for instance on l. 553 and not on l. 554! I will obviously check the totality of my files in the revised versions.

Abstract: define “entrance” and “exit” of the Strait

I am dealing with the Mediterranean Outflow and thus imagined that “entrance” and “exit” of the Strait would be directly associated to its “eastern” and “western” sides.

Line 84: to the Strait, where. . .

I am not sure I correctly understand the comment. The previous and first paragraph of the Introduction ends with a sentence (l. 77-81) explaining that “homogeneity” of the MO has generally been a

“supposition” which has never been tested so that I qualify it as a “postulate”. Meanwhile, I have always claimed that several types of MWs can be continuously identified. Seems to me that these two sentence conveniently introduce the major controversy (homogeneity vs. heterogeneity of the MO) I address.

Line 106: cut the “Now,”

I would have appreciated having an argument but I will replace “Now” by “This being said”.

Line 118: analyze in detail

I will remove the “s”.

Line 157: lower part

I am not sure since I could have written “an upper part and a lower part”. In French, it would be “les parties supérieure et inférieure”, then with an “s” at “parties”. I will ask English-fluent persons.

Line 165: were the only components

Yes, were.

Page 6, Table 1 legend: a deep water from. . .

Same answer as for the comment about l. 157.

Line 206: Gasser et al.2017 is not included in the References

I only had a manuscript and now have the exact reference of this now-published paper.

Line 247: clarify the definition of the layers (bottom-250 m-200 m-170 m?)

I imagined that giving four depths would allow easily understanding that the three layers were defined by “bottom to 250 m”, “250 m to 200 m”, “200 m to 170 m”. I will modify my writing.

Page 11, Fig. 2 caption: there is no reference to the enclosed graphic with lat.-long. values

I will refer to the inclosed graphic.

Line 335: The fact that

There are two different facts: one is “a relatively coarse sampling”, another is a “not north enough transect”. I think that the correct writing is “The facts that ...” but I will ask.

Line 356: a few hours

I will modify.

Fig. 3 caption: Since latitudes and longitudes in the figure' axes are in decimal format, there should be a correspondence when lat. or long. values are referred in the caption, e.g., 5o 40'W (- 5.66 oW);
I agree. Just note that I am doing all figures by myself and I don't know how to write automatically values in the degree-minute format. Even though correspondences such as between 40' and 0.66° is almost straightforward, I will add the correspondences.

HYDROCHANGES CTDs (green)

Yes.

Line 521: . . .and red groups in Fig. 5

I can add "in Fig.5" but I must note that these words would also have had to be added in the previous paragraph dealing with the blue group. Also note that these two paragraphs come just after a paragraph dealing with Fig.5a (l. 482-490) and another paragraph dealing with Fig.5b and Fig.5c (l. 499-505) so that links between both sets of paragraphs might be obvious. Finally, note that comments about the groups characteristics are illustrated in all other figures of the paper.

Line 524: larger than that

I am not sure since I want to say "... is larger than the spreading associated with the pink group, the spreading associated with the violet group and the spreading associated with the blue group". All three spreadings, furthermore they are inferred from 7, 20 and 16 profiles, respectively, are different.

Line 559: at profile #1

In case a word must be added before the sign "#", it should be "point" since I used "at". I could also have written "from (profile)". **I will ask the Editor for a more general writing: "Does a word must always be specified before the sign #?"**

I will also ask the Editor about the possibility to use "m" in a sentence (as in l. 560 and in reference to your comment about my use of "h" in l. 356).

Line 561: a general feature of

Yes.

Line 622: difference from previous

I don't know and will ask.

Line 639: more to the south or more to the center?

I understand your comment that, seems to me, is "influenced" by the fact that the central part of the transect is much deeper than its southern part: there will always be more DW in the central part of any north-south transect than in its southern part. I deal with "north" at the beginning of the sentence (l. 638) and want to introduce the notion of a "north-south sloping" that is illustrated in particular by Fig.7a (more specifically by the kriging plot; see what I specified to Referee #2 in this respect). In addition, and as demonstrated in all my papers, I am convinced that the densest MW (blue herein) is the one found in the southernmost part of any transect, whatever the bottom depth is.

Lines 661, 677, 704 and 973: reference is made to Supplementary Information figures which do not seem to be available.

You should have asked the OS office ... furthermore a lot of interesting information is contained in what is named "Supplement" when I access "My Manuscript Archives" where I have several sections: MS Records, Interactive Discussion, Minor Revision, Initial Submission. In this last section, and at the end of the "Topic Editor Initial Decision" (before my answer to the Editor), you have "File Upload (10 Jun 2017) ... "Supplement". I easily got my Supplementary Information file.

Lines 685-688: the "marked irregularity" mentioned in this paragraph is hard to observe in the figure (Fig. 9a) because the coloured dots in the theta-S diagram are too big.

I agree. I previously explained (about Fig.6a) that I choose to have relatively big dots for colored parts of the θ -S diagrams. I will add small black dots over the big violet ones.

Line 745: leading to the

Yes.

Line 756: Millot (1987) is not in the references

Yes, I will add it.

Line 758: a role that is much more (?) important

Yes. I wanted to say "more important" and I will correct.

Line 1038: heterogeneity leads

Yes.

Line 1087: much more unbiased

Same answer as for l. 758.

Line 1127: García-Lafuente et al., 2011 does not appear referred in the text

Yes, I will remove.

Line 1139: Gasser, 2017 is missing (although it is referred in the text).

Answer as for l. 206.

Line 1150: Millot, 1987 is missing (although it is referred in the text)

Answer as for l. 756.

Line 1185: the title of the paper Peliz et al., 2009 is missing

Yes, I will add it.