Evaluation Report:

General comments

This is the 3rd of a series of three papers dedicated to track the transformations undergone to the Mediterranean waters through their outflow across the sills found from the Alboran Sea in the Mediterranean to the Gulf of Cadis in the NE Atlantic Ocean, usually referred to as sills of Gibraltar. This last paper of the series offers a detailed view of the final water masses that, after the transition, are currently known as Mediterranean Outflow Waters (MOWs). These MOWs (note that I'm talking of a set of MOW's) constitute the water type involved forming the Mediterranean Water mass (MW) within the Atlantic Ocean, whose description can be found in all the classical oceanographic literature (*e.g.* Sverdrup et al., 1942). Given that my report is essentially addressed to the authors, this introduction may seem useless because they know much better than I their intentions, but I would prefer to explain the scope of the acronyms I am using here (see *Specific comments* below). As a warning for my possible errors, or just to avoid misunderstandings on my comments, it must be said that I am not currently working with Gibraltar exchanges (indeed I have never being deeply involved in them) so that my expertise in this complex region is incomplete and not updated.

The discussion and assessment about sampling strategies is another relevant aspect of the papers in the series. Based on the problems found processing the information, the authors propose ways to improve the data collection. Such aspects use not to be addressed in many papers whose discussions rarely include a criticism on the strategies. This information is very useful for new research, but in general, sampling strategies tend to evolve more based on technological improvements than on an assessment of previous experiences.

The series of papers is a challenge and a good example of data recovery to reconstruct a sequence in the space with different acquisition time. In particular, this paper is based on the most recent data set within all the series. CTD data was collected in a 2009 survey, covering the oceanic side of the Gibraltar sill and the slopes of the Gulf of Cadis, with an unprecedented detail, after years of work. The analyses in the paper are careful, deep and detailed. Casts have been analyzed one by one, which is a slow and tiring process for the authors, but acknowledged by the reader besides the apparent complexity and lengthy presentation. In contrast of the "big data" techniques in fashion (not exempt of complexity), in my opinion, the detailed TS analyses carried out in the present paper are the only way to sustain relevant results dealing with water masses. In particular, to provide a definitive evidence of the non homogeneous and layered structure of the MOW at Gibraltar, which is the fundamental conclusion of the present paper (and of the main goal of the whole series, I presume). However, the writing style may transmit some idea like: "our paper is the definitive end of the myth of a homogeneous MOW". This feeling, although the word "myth" does not appear in the text, does not help to convince the reader. Indeed myths, while not accepted in Science, we all know that do exist. It is a recurrent fact that there are some opinions that prevail over other, even supported by solid evidences. In the present case, the idea is to offer new evidences about MOW that are in disagreement with some prevailing ideas. To convince the reader, I suggest a less passionate writing. I would try to avoid explicit insistence in controversies or in the "objectivity" of the analyses because it may give the feeling that there are "hidden tricks".

Back to the analyses, the idea to characterize the profiles according to their maximum density (σ_{max}) is very useful for the purpose of this paper, as it defines the MO water type of the mixing lines in the profile. Setting these particular rectangles in the TS diagrams to group the water types to identify four water coloured masses may be arguable. I wonder if a TS diagram covering the whole range of θ and S showing the θS point for all the σ_{max} would help to

highlight the limits of the rectangles, in addition to the comments in the text and Fig. 2a. The different properties of the mixing lines in the profiles, from "pure" AWs and the MOWs, will also depend on the "starting point" in the AW. All these points lie on a straight line, running close to the σ_θ=27 (as can be seen, for example in Figs. 3a and 3b). I would consider that this line represents the NACW, not only the θS minimum as mentioned in the text. Since these waters in the text are considered as SAW although below the seasonal thermocline, it would be useful to know what are the depth ranges involved in the "surface" consideration.

Overall, a general impression I had is that the paper is addressed to the experts. For a general oceanographic audience, the paper is difficult and requires long time (for instance I spent many days to carefully read the text back and forth many times). This is a serious risk of readers' discourage, even abandon, or not to be fully understood. There are several reasons for this problem, in addition to its technical or theoretical complexity, lying in the presentation. Among those:

There are many long sentences that difficult the reading. Unfortunately my English is by far not good enough to be able to offer alternative writing. I think that the paper reading would be largely improved after being carefully revised by someone with better skills than me on English.

The structure is quite mixed (see *Specific comments* below).

There are many figures. This is of course not a problem by itself but some of them are complicate or difficult to understand and others may be resumed (see some technical suggestions below to address this issue).

As a final impression, I think that if authors cope with all these formal issues, the paper will be clearer and its thesis much more evident for the whole oceanographic community.

Specific comments

1. Introduction.

In L97 "*MWs*" are defined as referring to any water mass formed in the Mediterranean basin. The text is coherent with this definition until L148 when introduces the "coloured" water masses saying that they were so defined "*to avoid dealing with MWs names…*" … in the MO, but, at the end of the introduction, the authors, "*in their mind*", associated colours to MWs. In my opinion the text should say MWs only when they could be identified as any of the MWs. Those in the outflow, identified by colours because *"…links cannot be purely objective…"* should be referred to as MOWs. Authors then could say "*in their mind*" that this or that coloured MOW correspond to this or that MW. In this way, as I mentioned in the introductory paragraph of my general comments, the acronyms will be consistent with those defined in CIESM group (2001). In addition, although it is not relevant within this paper, remember that the acronym MW is currently being used for an Atlantic Ocean water mass, among others such as: NACW, SACW, AIW, LSW or NADW.

2. Material and Methods

The sentences "*As detailed…topography*" (L175-179) and the next "*We do not… heterogeneous*" (L175-183) would be better in the Introduction while the last may also be useful in the Discussion.

Table 1 is very useful but not all the mentioned stations appear represented in the map (Fig. 1b), which is concentrated to the "*cross*". My suggestion is to put in this section a series of maps: A general view, such as Fig. 1a, as it is but with two rectangles inside: one for the regional surveys, which are now shown in Fig 16 and the other for the "*cross*". In addition,

locations of Camarinal and Espartel sills will be useful in this general map. Then, a new Fig 1b equivalent to the current Fig. 16 for the regional surveys, and finally, the actual Fig 1b (now converted to Fig. 1c) split into two maps showing stations of Part 1 and Part 2 respectively, instead of having all the stations points overlaid (note that some marks are completely hidden by others). The proposed order of Figs 1a,b,c is related to the Table 1, that may contain, in addition to the colour codes given (warning: there is an error!), indications of the corresponding figure where the stations appear.

Although TS diagrams have been always used for water mass analyses, I would include a final paragraph describing the particular TS characteristics expected, according to the specific situation of MWs intruding into a AWs environment, and how to characterize the water masses in the results. This will help the reader in face of the first TS diagram in Fig. 2a and simplify its caption (see the next section and *Technical corrections* below).

Finally, although it is implicitly mentioned in Table 1, I would say the cruise was carried out in summer. In areas with marked seasonality, it is worth to highlight the season of the sampling. This gives a rough idea of the thermocline depth, for example.

3. Data analysis

The overall description of the results at the introduction of this section (L206 to 212) would be a good place to say something else about the rectangles in Fig. 2b (and many others) and grouping of stations in coloured groups (see also the *Technical corrections* below).

The first results shown correspond to the MO-Transect+St#479 of the Yo-yo Central Pt-2. The flavour of the general arguments of the paper is said to be shown in the first TS diagram (Fig. 2a). This must be a key figure (I found similar figures in the all the three papers), but the information given either in the text (L229-238) or in the figure caption (L216-228) is insufficient (at least for me). St#, depth of the profile and the θS point of σ_{max} are clearly shown in the figure. We can also see that most of the profiles show a stratified layer (with a gross average density gradient, by eye, of ~0.05 kg m^{-4}) with a mixing line between AWs and MOWs, and an almost homogeneous bottom layer (absent in some few profiles, as indicated). How homogeneous are these bottom layers is not evident in the figure since no $\Delta\sigma$ criteria are given (*e.g.* "homogeneous" bottom layers of #449 and 451, have $\Delta\sigma$ as high as ~0.05 kg m^{-3}). The rest of the information is not clear to me, for instance, what is the difference between cyan and blue points? I can realize that cyan < blue values. Is it relevant? Does it mean that cyan points are in the "most homogeneous" zone within the whole "homogeneous" bottom layer? Why this different colour treatment? May any vertical profile of θ and S help? Any hint will be welcome. May be the explanation should be found in the Fig. 1 of the first paper and Fig. 1c of the second (that introduced respectively their TS diagrams similar to the present Fig. 2a. Unfortunately they are also not clear enough for me. For instance, I did also not understand what represent cyan and blue colours in those figures.

Finally, it will be useful to know what is the σ_{max} point for each one of the four not labelled profiles (#435, 445, 465 and 469) shown in Fig. 2a, as mentioned in L234. Note that at least two of them have σ_{max} higher than #475 and 455.

The grouping subsequent to this TS diagram is introduced in the next paragraph (L240-250) and explained in the next caption (Fig. 2b) before being systematically applied across all the results. I suggest rewriting these paragraphs and caption according to the comments in the *Technical corrections* below.

A little forward, appears the first application of colours in successive sections across the MO (L280 onwards and Fig. 2c). It is a very useful application to evidence the structure of the MO and to provide recommendations for sampling. Whether these explanations should be there or later depends on the style. I personally agree to include them here but also to highlight them in

the Conclusions (in particular because of the typical "battle" with the technicians to descend the CTD as close as possible to the bottom, even in good weather conditions and using a pinger or altimeter!). As I mentioned above in the *General comments*, extending the "colour properties" to the whole profile has some additional interesting properties evidenced in Figs. 3a and b where the whole TS diagrams are shown. One of these properties is associated to the presence of θ minima (referred to the NACW in the text) in the "blue" and some "grey" profiles, that are related to the "history" of the mixing lines and the circulation, as explained. Note in particular that there are two kind of "grey" profiles: (i) those with low σ_{max}, typically on the north side, without θ_{min}, that are "similar" to the red and orange, and (ii) those with high σ_{max} and θ_{min}, that are similar to the blue (*e.g.* Fig 4). If a line joining the tips (σ_{max}) of the profiles in an enlarged TS diagram from south to north is drawn, it will show first an S increase to a maximum (blue) then an increase in θ and decrease in S. There is also a good relation between the "starting" point of the mixing line and those tips. In my opinion it may be useful to further explore these properties and their relation with the stratification and the inflow and outflow.

The along-MO transects are typically dominated by "blue" and "violet" waters. According to Fig. 5, it looks like the legs ran from E to W so that in a given leg, the St# increase to the W. This may help to interpret the individual TS diagrams and the "irruption" of red and orange waters at the end of the series. Note that there is some incoherence (or a misunderstanding) in the text L627-628 because in the cross sections, orange and red waters appeared on the north side.

The second visit of the along-MO transect seems to confirm the change from south to north although less markedly (by the way, I found an error in L724 where σ_{max} should be replaced by σ_θ). I understand that vertical sections (Fig. 9) among the TS figures (Fig. 8) are located to explain the particular situation found in Leg 6, but this is not a common way to present the results.

Overall the descriptions of the along-MO transects, I wonder if to draw a stack of maps of "coloured" positions would help to show the water masses evolution. The same could be said with vertical sections such as Fig. 9.

For positions of central points, CP1 and CP2, the reader is referred to Fig 1b and Table 1 (L835). The table does not refer any position. In the figure the green points are more or less hidden below other crosses (see my comments above) but roughly at the centre of the "*cross*" thus in contradiction with Fig. 9 (see the *Technical corrections*).

For precision in positions (L870-878) was the ship in MO-2009 repositioning before every cast or she was using a dynamic positioning all the time? It looks like it was repositioning during the upcast but I am not sure to interpret the text correctly. What can be the effect of repositioning on the LADCP data?

Of course during GIBEX no GPS could be used. By the way, note that (L871) 10^{-3} ° is a very strange measure of deviation. I propose to use seconds (~3-4) or fractions of minute (~0.06).

Fig. 10c. Does the depth interval from the crosses to dots correspond to the "homogeneous layer" mentioned in Figs. 2a? (see my comments above). However, if one looks at the vertical sections of θ, S and σ_θ (Fig. 12a), doesn't seem realistic. For example, the cross (σ_{max}) of St#483 in Fig. 10c is located at ~468 m depth and the dot at ~482 m, but at least θ decreases ~0.05°C in this depth interval, according to Fig. 12a, does salinity decrease? In any case "homogeneity" criteria must be given.

The details in the description below Fig. 10c (L884-894) are difficult to follow although the general point seems to be clear.

Fig. 11a. Is there is an error in labels of St#? According to the text L911, it looks like the labels within the rectangle of blue waters should say 501-509 instead of 495-509. Please check this and correct either the text or the figure.

L986. What is correctly represented in Fig. 12b (LADCP)? I presume it is a typo and should say Fig. 12a (σ_θ). Please check it and correct or explain.

Fig. 12b. According to the caption, if axes are rotated an angle of 203° to the 247°T, positive V must be directed to 157°T. This will indicate a relatively significant tilt of the main current towards the south (some 15-20° by eye, *i.e.*, to ~230°T) when its magnitude decreases at St#485, followed by a slight increase, with no significant correction in the new direction. A general decrease of the current comes in St#489 followed by a new increasing to St#493, still maintaining the tilted direction towards the south sector, that includes an increasing of the cross section velocity in the above layers. Finally, at the end of the series, St#497 the original direction of the current seems to be recovered. Could all this be related to the changes in the shape of the TS diagram (Fig. 11a)?

The introduction to the regional surveys is hard to follow (especially from L 1160 to 1167) in addition to the problems of Fig. 16 (see *Technical corrections* below). Most of the comments here refer to a TS diagram not shown, since even the largest one nearby (Fig. 17e) do not cover the NACW line. The strategy in the detailed description of the surveys is both chronological and from the "source" towards the Ocean thus implying a successive enlargement of the scope. Why not to include a TS diagram of the whole regional surveys to "locate" the details to be seen. Such a figure would also help the interpretation of Fig 18. This impression is further confirmed when I noticed that Fig. 17e has to be mentioned before Fig. 17d (L1322).

Figs. 17a (and d). It looks like the colour codes in these figures do not match the general rule. As if the profiles were shifted but, according to the text, the references to the density values are coherent with the figure. Whatever reason for this change it should be mentioned. In any case, Fig. 17a shows the south to north sequence of σ_{max} characteristics I mentioned above (increase of S then increase of θ and decrease of S, from south to north). According to this sequence, it looks like the second survey shows an overall displacement of the water mass towards the north in this easternmost zone.

Most of the comments on TS diagrams to illustrate the regional surveys mention depths that do not appear in the figures.

With Figs. 17d and e one can also speculate about a deflection of the main MO flow with the densest waters (violet and blue) that would pass from near St#29 to south of St#31 and then towards the NW, stretching, to pass between St#51 and 53. Unfortunately the distance between stations in the regional survey was too wide to confirm or reject this hypothesis. Anyway, this would be consistent with the bathymetry (Fig. 18). In my opinion, this possible deflection is different from that reported by Gasser *et al.* (2011) and Nash *et al.* (2012), that is the one detached in S1-T3. It affected lighter waters and may be the responsible of St#35 be grey. It can be noticed that the "greyness" of St#35 is much different from that of St#53. The authors points out, I think, that the diversification at S1-T4 continues to the west so that the violet and blue waters would be found away of St#53 if T5 was longer. In my opinion if so, the two grey stations will share similar characteristics but it is not the case as can be seen in Fig. 17e.

4. Discussion

L1418. I just found a new article of Sánchez-Leal *et al.* (November 2017; see the *References* below) while doing this review. The article reports many surveys in a large area, including the zone covered by the present paper. Curiously it does not include the MO-2009 set although at

least two of the authors were participating in the cruise (see http://advances.sciencemag.org/content/suppl/2017/11/13/3.11.eaao0609.DC1).

L1421-1423 sentence points to a diversification of the MO between S1-T3 and S1-T4. This would be coherent with the deflection I mentioned above that can also be seen in Fig. 18. I am not sure that the comments of the authors would refer to this deflection or to the bifurcation mentioned by Gasser or Nash (*op.cit*).

L1548 to 1559. I fully agree with the proposed strategy but I would not see how a CTD can be towed safely near the bottom with strong currents, even with a pinger. Perhaps fast bottom layer profiles, from just near the AW-MW interface to very close to the bottom, with the ship maintaining the position …

L1621-1624. This sentence may be related to the stratification changes that I cannot understand (probably due to the bad transcription of symbols Δσ).

L1677-1690. Although I did not deeply review all the information available for the region, I wonder if the dramatic amount of DW formed in the W Mediterranean in winter 2005 (CIESM, 2009), changing the deep structure of the water masses could have had a further important role in this transition, not recorded in Millot et al. 2006 (*op. cit*). According to Fig. 20 it looks like the saltiest values correspond to the 2004-2008 period. I would suggest to enlarge the discussion on this aspect or at least to say why this may be or not important.

Technical corrections:

Throughout the paper geographic coordinates are mixed. Sometimes positions are referred to as degrees and minutes (typically but not always in the text) and sometimes as degrees with some decimals (typically but not always in the figures). The interpretation involves mentally dividing or multiplying by 60 thus adding an unnecessary difficulty to the reader. My suggestion is to unify (my personal preference is in degrees and minutes) the coordinate system.

Although I think it is a lost battle, I'm a strong supporter of the B&W use for figures. It is not a nostalgic view of the old books style but just a practical advantage when the printer at hand does not reproduce colours (less expensive). To consult papers such as the present one, I use to spread the figures on top of my table to help reading. To understand a scientific paper nowadays, a typical "linear" reading use not to be enough. Clear figures in B&W enable quick understanding with a glimpse. An old professor I had used to say that a graphic with more than three functions is not clear and should be avoided. Unfortunately this does not apply anymore in scientific publications. It looks like if there were a shortage of space, curiously when journals are on the web and many webs are "heavy", but not precisely because of their scientific content. You can find as much examples as you want in any journal, but this does not imply that simple and clear figures were not better. Note that improvement of technical abilities for graphic representation in the last few years is not associated to an evolution of human brain abilities that require thousands of years. Let me just give a suggestion to the authors about figures: "Before sending a figure for publication, print it in B&W and look at it. Can you see what you wanted to show?" If this rule is applied in the present case, many figures can be easily modified but some others have to be deeply redrawn, perhaps selecting a different colour palette…

Let's just apply the above suggestions to Fig. 16 that introduces Figs. 17.

Change coordinates in the axes to 35°42' N, 35°48' N, … and 6°36' W, 6°30' W…

Remove shades in bathymetry. Isobaths are indicated so the reader can recognize the bottom structure without shades.

Then, all the station numbers could be easily seen or it may just be a matter of finding a colour palette for the dots and numbers. I assume that this may pose difficulties. Another way to deal with this is to put different symbols for dots or different typography for the numbers.

Let's now go to the Figure captions. Many of the TS diagrams in the paper say:

"*As in Fig.3a for the ……*".

The reader, then, is directed to:

"*Figure 3a. Θ-S diagrams of #445-455 of the cross-MO transect, Leg 2. Colors as in Fig.2b*."

That, in turn, directs to:

"*Figure 2b. An as objective as possible grouping and coloring (orange, red, violet, blue) of the profiles is made herein, just for convenience and easiness of the discussion without having any scientific implication, according to the position of the ϑ-S-σ_{max} set with respect to the rectangles in green defined from isotherms (13.65, 13.55, 13.40, 13.28 and 13.18 ℃) and isohalines (37.90, 38.02, 38.10, 38.15, 38.20, 38.30 and 38.35). When the set is out of one or the other rectangles, the profile is colored in gray and, when it is very close to one or the other limits of the rectangles it is bi-colored, mainly with respect to the actual position of the set and partly with respect to the color it could have had with a slightly different position (e.g. #155, Fig.6g).*"

This figure and the message in its caption are crucial throughout the whole paper because the "*objective*" rectangles in the TS and the corresponding colours are the key to the analysis and classification of the MOWs (I assume -not aware of any exception- that these four colours are exclusively used for that purpose in TS diagrams). Therefore I strongly suggest a full description of the grouping in the text referring to this figure as the illustration. Note that the text must also include an explanation why some of the stations are "*bi-colored*" (e.g. #155, Fig.6g and some other).

Then, if such a clear description is written starting section 3 (~L205 to 212) then, the caption of Figure 2b will become:

"*Figure 2b. TS diagram of stations in the cross-MO transect used to illustrate the description of grouping of the stations (see a detailed description in the text) used throughout the paper. Groups are defined according to the σ_{max} position (+) in the green rectangles shown (see the text for their corresponding TS vertices). Each colour: orange, red, violet and blue, corresponds to a group. Stations out of any of these groups are coloured in grey. Note that these colour codes are being used in all the figures of this paper.*"

Then, Figure 3a will say:

"*Figure 3a. Θ-S diagrams of #445-455 of the cross-MO transect, Leg 2*."

And the other TS diagrams could say:

"*Θ-S diagrams of …… of the ……*" (which, in addition, is shorter than mentioning Fig. 3a).

Figure 9 shows the position of the CP between St#753 and 755 while in Fig 7 should be near St#759, as far as the caption is said to be the same as Fig. 5. Again, why not to repeat the meaning of the discontinuous lines represented in Figs 5 and 7? It also seems in contradiction with the "gross" position of the Central Points in the map (Fig. 1b). This is not a minor comment since the position of the CP relative to the along-MO transect is being repeated insistently along the text (*e.g.* L1000).

Also in Fig. 9. The distribution of profiles is set according to time but since the section roughly runs from E to W, why not distribute the stations according to longitudes? thus reversing the X axis (labelled accordingly). This would make it easy for interpretation in a quick look (this of

course cannot be applied to the other vertical sections, shown in Fig. 12) where position does not change.

Back to Fig. 17e, the profiles of GIBEX overlaid in grey are almost invisible on the screen and on a printed page. Similar problems could be found in Fig. 20.

Not related to the figures, there are many "double" sentences such as for example: *"… the lightest (resp. densest) of them will sink through, circulate within and thus mix with relatively light (resp. dense) oceanic waters*" (L1630-1631) that make the text harsher. I would suggest avoiding them even if it requires repetitions. Again, paper should not be the limiting factor today.

These are just suggestions but they will simplify the reader's life and contribute to a better understanding of the paper.

References

In addition to those appearing in the manuscript:

CIESM, 2009. Dynamics of Mediterranean deep waters. N° 38 in CIESM Workshop Monographs [F. Briand, Ed.], 132 pages, Monaco.

Sánchez-Leal R.F., M.J. Bellanco,1L.M. Fernández-Salas, J. García-Lafuente, M. Gasser-Rubinat, C. González-Pola, F.J. Hernández-Molina, J.L. Pelegrí, A. Peliz, P. Relvas, D. Roque, M. Ruiz-Villarreal, S. Sammartino, J.C. Sánchez-Garrido (2017). The Mediterranean Overflow in the Gulf of Cadiz: A rugged journey. *Science Advances*, **3**: 1-11.