Interactive comment on "Definitive evidence of the Mediterranean Outflow heterogeneity. Part 3: at the Strait of Gibraltar exit" by Claude Millot and Mikhail Emelianov

Answers to Anonymous Referee #1 (comments received on 25 September 2017)

Dear Referee #1,

Let us first of all very sincerely thank you for your careful reading of our manuscript, your very helpful comments and your efficiency in rapidly providing us with your comments, which explains why we are now apologizing for answering with such a large delay; you certainly understood we were waiting for the report from Referee #2 who has been nominated just a few weeks ago.

Before answering your comments one by one, please let us specify that, even though you did not express any willingness to review the revised version, we will resubmit this Part 3 in a markedly modified form. We will take into account not only your and the Referee #2's remarks and comments but also those we received from the Editor and the Referees of the Part 1 and Part 2 papers. Essentially, we have proposed to the Editor a splitting of the former Part 1/3 in two (which will make a tetralogy), with a new Part 1/4 presenting an overview of the heterogeneity aspect and introducing the Parts 2/4 to 4/4 that will focus on the entrance of the Strait, the Strait itself and the exit of the Strait. For instance, the schematic diagram in Fig.19 and the 1980's-2000's comparison in Fig.20 would be moved to the Part 1/4 paper. We are personally convinced that this will markedly improve both the "Presentation Quality" and the "Scientific Quality", hopefully making the "Scientific Significance" more evident, of the paper you reviewed.

Please, even though we perfectly understand you did not think necessary to review the revised version and do not want to engage yourself in another review, let us specify that we would appreciate any "friendly" (i.e. not official) comment (even just a few words!) that you could send us in a fully anonymous way with the help of the OS office, either on our answers below or on the new version we plan to submit before mid-April 2018. In any case, we warmly thank you for all what you did for us.

General comments The present manuscript is the third part of a trilogy dedicated to provide evidence of the outflow heterogeneity in Strait of Gibraltar itself, based on different sets of (mainly CTD) data collected in the eastern, center and western sides of the Strait, thus contradicting the generalized idea of the main role of Gulf of Cadiz's bathymetry on this heterogeneity. The present manuscript complements the first and second parts (based on data collected at the Strait entrance and along the Strait), by showing evidence of the heterogeneity of the outflow at the Strait exit and also demonstrating the strong spatial and temporal variability of the Mediterranean Outflow. Suggestions are made for future sampling strategies in the Strait of Gibraltar, trying to overcome the problems connected with this extreme variability. Interesting hints for problems to be solved by numerical modeling are also presented.
This is a good synthesis of what we wanted to do … and, hopefully, of what we will finally be able to do!

Specific comments In general, the written text could benefit from clarification in some places. It is my feeling that the manuscript could be more efficient and lighter by much reducing the number of figures that are not essential (e.g., some of the figs. 8), since this is not an exhaustive scientific report but a manuscript.

We perfectly understand your point of view that is somehow shared by Referee #2. Because, as you noticed in your general comments, we have to “contradict a general idea”, our aim was to provide as many arguments as possible in order to try convincing the whole community. However, as we previously said to the Editor about the former Part 1, we think this is an “overall editorial decision”. We could perfectly move any set of figures and their associated comments in what is called by OS a “Supplementary Information” file, as we already did for our Part 1. **We will strictly follow the Editor’s recommendation!**

Technical corrections In the whole text, there are many cases of wrong letterings for the potential temperature (q instead of ïA¸s´) and potential density anomaly (Sq instead of ïA¸s¸ïA¸s´), as it happened already in the previous two parts of the trilogy.
Before all, thanks for having read the two other parts. Referee #1 of Part 1 too noticed this problem and CM answered to her/him: **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.**

Line 25: (over 30 x 30 km)
We are sorry but we disagree. We could have written (over 30 km x 30 km) but this is clearly a 2-D information that we preferred specifying as km².

Line 28: each other
Sorry but we are not very fluent in English. Our sentence is “Four components … each others ...” since we want to say that “Each component can be isolated form the three others”, and we would have written “Two components … each other ...”. We checked in the “Linguee Dictionary” that “each others” was an accepted writing and just wonder whether it is correct or not using it in our sentence. We will rely on the OS editorial office.

Line 33: splitting not needing
You are right.

Line 143: maximum potential densities
You are right.

Line 171: Material (instead of Materiel)
You are right.

Line 246: on the basis of
We wanted to say “on the basis of Fig.2a and on the basis of the analyses here below”. In French it would be “bases”. We will rely on the OS editorial office.

Line 265: Fig. 2b’ caption should be under the figure
You are right. We used “Libre Office” for the first time in place of “Microsoft Word” and generally encountered problems with figures included in the text. For instance, having a correctly set paper (figures, captions and text in correct places), we experienced a total “disorganization” by just, for instance, adding a line at the beginning of the paper. In any case, we did not check enough.

Line 285: but all four colors are
You are right … but we wanted to say that “all five colors are...”. Indeed, the gray color is an essential color for us since it demonstrates that the MO has been crossed in totality (gray must be found on both ends of any kind of transect).

Line 412: is coloring in Figs. 4a and 4b connected with coloring in Fig. 2b?
Yes, coloring in Fig.2b, as well as in Fig. 2c, 3a, 3b is not only connected but identical to the coloring … of the dashed vertical lines in Fig.4a and 4b since each of the profiles (of the whole MO-2009 experiment) has been given one of the five (blue, violet, red, orange and gray) colors objectively defined in this paper.

Line 517: isn't the "northern part of the transect" on the right hand side of the MO?
We are sorry you did not catch this essential aspect of our analysis that is clearly explained l. 1013-1021 and demonstrates that "the northern part of so-called along-MO transects is actually on the left-hand side of the MO!".
Among many arguments for that, please consider:
-Fig.1b: the so-called along-MO transects (1 and 2) are not aligned with the mean axis of the Strait as inferred from the isobaths, and depths in the northern part of the transects are shallower than in the southern part, which is illustrated in Fig.5 and Fig. 7 about each of the along-MO transects.
-Maximum potential densities at the north-eastern points (Fig. 13a, 13b, 14a) are much larger than the densities at the central points (Fig. 10a, 10b, 11a, 11b) and that the densities at the south-western point (Fig. 15c). This is fully consistent with i) our analysis in Part 2 and ii) our schematic diagrams (i.e. all our previous analyses):
The largest (resp. lowest) densities in the MO are observed on its left (resp. right) hand size.

Line *530: much shallower*
You are right.

Line 806: associated with (for not repeating linked to)
You might be right but we are writing a scientific text in which we want to clearly emphasize the opposition between two hypotheses about the splitting, namely "the heterogeneity of the MO from its origins (the Sea)" and "local bathymetric features". Using the same formulation with "linked to" seems to us more adequate to emphasize the opposition. We will rely on the OS editorial office.

Line 881: refer the black circle meaning in the figure's caption
Sorry we are not sure this is a circle. This drawing has the same use the oval in Fig.7 has. We will specify in all concerned captions that these drawings just aim at specifying which of the profiles concerned by computations in gray (profiles in between two series of profiles of different colors) evidence a significant interface layer. For instance, in Fig.10c and the caption you refer to, such computations can also be made for profiles #181 and 229 that do not indicate an interface layer, contrary to profile #227 on which the drawing is centered. More explanations for l. 936.

Line 929: shallower by 25 m in the two. . .
You might be right and we will rely on you.

Line 936: refer the black circles meaning in the figure's caption. In fact, the gray lines and little crosses and dots within the black circles in Figure 11c (and also in Fig. 15b) are not clear at all.
In the caption of Fig.7, which is the first figure in which the gray signs and segments appear, surrounded by black circular drawings, we specified: "In gray, immersion of the #759-s max isopycnal at neighboring locations." We did the same comment for Fig. 10c and other figures (11c, 15b) as well and we lengthily discussed this signs in the text but, for sure, not clearly enough: **gray lines just display the variation in depth, from one profile to the other, of a given isopycnal**!
More precisely:
-these figures essentially display crosses that specify the immersion of the maximum potential densities (σ_{max}) for each of the profiles (without any information on the density stratification).

-the crosses are colored according to where the σ_{max} are located on a θ-S diagram, in particular in gray i) when the σ_{max} are out of the rectangles in green (the water there is not very influenced by the MWs) and ii) when the corresponding gray profile is in between two profiles of different colors (for instance blue and violet). Gray crosses are thus possibly linked to interface layers of waters influenced more by the AWs. Note that Fig. 11c also evidences crosses in red isolated in between crosses in either blue or violet.
-to check if a gray cross is markedly associated with AWs, one cannot rely only on the sole σ_{max} numerical value since a gray profile can just be not deep enough, so that it is necessary to specify the variation of the density stratification from one profile to the other, hence drawing isopycnals as a function of depth and with either time (at a specific location) or space (along a given transect).
-drawing isopycnals for the whole set of profiles could have been classically done by an automatic interpolation (as in Fig.9a and Fig. 12a) but the provided information would be smoothed and would make the figure very complex with a lot of unnecessary information.
-the idea is thus to just plot the variation of a given isopycnal during three successive profiles by specifying what are, on the two neighboring profiles, the immersions of the σ_{max} gray isopycnal, these immersions being specified with small gray dots.
-it is only when the σ_{max} gray isopycnal is immersed on the neighboring profiles at depths much shallower than the depth of the σ_{max} of the gray profile, hence when this isopycnal actually describes a marked V, which is indicated on the figures by thick V-shaped segments in gray that mask the small dots, that one can be sure the gray profile evidences an interface layer.
-when the σ_{max} gray isopycnal is immersed on neighboring profiles at depths similar or even deeper than the depth it has on the profile in gray, the segments linking these immersions are relatively thin gray lines, which makes the small dots apparent.
-to draw the reader's attention, only thick V-shaped segments are surrounded by circular drawings.

Line 1090: As in Fig. 13a
Sorry but you will see that the same "As in Fig.3a" is also specified in the caption of Fig.13a … as in the captions of all θ-S diagrams as well.

Line 1194: and Survey-2, Transect-1
You are right.

Line 1297: clarify "is almost far upstream the central zone"
The central zone is shown in Fig. 1b. It is centered at 6.35°W and its upstream limit is 6.3°W. As indicated by Fig.18, Transect 2 of Survey 2 is at 6.25°W which we consider is "far upstream" from the central zone.

Line 1314: being very (?) south
As you can infer from Fig.17c and 18, #817 is blue, over 355 m and at 35.79°N while #815 is gray, over 270 m and at 35.75°N. One result of our overall analysis is that differences of 85 m in depth and 0.04° in latitude are "very large" differences when one is concerned by the MO sampling. We thus maintain that #815 "being very south" or "too much south" did not sample the MO.

Line 1382: light gray lines are referred Fig. 17e'caption but these lines are almost invisible
We are sorry but we personally can easily see these light gray lines; whatever the case, we will make them darker. Note that we specify we do not comment these light gray lines (done in previous CM's papers).

Line 1407: along-Strait transects
You are right.

Line 1456: up to now, a correct understanding

You are right. We previously wrote “...from correctly understanding...” and did not correct!

Line 1493: in the references, besides CIESM Group 2002 we have also Millot and Briand 2002, which is the respective Executive Summary. Only one of these references should appear.
You are right.

Lines 1508-1510: left-hand side of the MO and right-hand side of the MO shouldn’t correspond to southern and northern sides?
This is a comment we already answered. This just depends on which kind of transect you are considering. For instance, the left-hand (densest) side of the MO is sampled on the northern part (we tried not using “side”) of the so-called along-MO transects and on the southern part of cross-MO transects. Actual along-MO transects performed in the middle of the MO would sample neither its densest side … nor its lightest one.

Line 1538: can be obtained in an efficient manner
We are sorry but we would have liked maintaining the “definitive” since we address in this sentence the specific aspect suggested by our analysis that is “the structure of the MO components, i.e. the various MWs,” and, mainly, “the existence of interface layers in between them”. Obviously, this structure will evolve with time and interface layers will be more or less thick from one sampling to the other. But the kind of sampling we propose (towing a CTD across the MO) will definitively check the hypothesis we make about the existence of interface layers.

Line 1555: a tow-yo transect
You are right.

Line 1565-1573: explain the “never” that appears in the Figure 19.
Explanations must also concern the “2009” and “Up to ~6°30’W” indications too.
Our original diagram was proposed on the basis of the 1980’s set of data. It indicates that, at Espartel, the MWs were juxtaposed side by side while, at 6°15’W, they started becoming superimposed, hence able to form independent veins relatively soon downstream. This was suggested by the fact that θ-S diagrams there were no more linear (as the light gray ones you did not clearly see in Fig.17e).
In 2009, all θ-S diagrams in the study area from 6.2°W to 6.6°W (6°12’W to 6°36’W, Fig.18) are straight mixing lines so that, in 2009, the schematization we proposed for Espartel was valid “up to ~6°30’W” … at least! A situation such as the one encountered at 6°15’W in the 1980’s was thus “never” encountered in the 2009 study area. Such a situation occurred more downstream … necessarily since it is the sole possible transition phase between MWs/veins juxtaposed side by side and MWs/veins superimposed along the Iberian continental slope (as schematized in Fig. 19).

Line 1621 and 1622: explain the meaning of DsM and DsO (which I presume are delta sigmaM and delta sigmaO; what are the M and the O?)
Yes, major elements of this sentence are: “ ...some original $\Delta\sigma M$ at the Strait entrance, ... by some final $\Delta\sigma O > \Delta\sigma M$ at the Strait exit, when the MO enters the Ocean.” We should have specified that M stands for “the Mediterranean Sea” while O stands for “the Atlantic Ocean”. Note that we systematically use the words “Sea” and “Ocean” (not to repeat the useless words “Mediterranean” and “Atlantic”) but that using the letter S would have been misleading so that we used the M.

Line 1666: were no longer straight
You are probably right.

Line 1677: was not yet split
You might be right.

Line 1748: . . .Experiment, kindly made available to us, with. . .
You are right.

Line 1753: with respect
You are right.

Line 1817: Béthoux et al. 1990 is not referred in the text
You are right.

Line 1832: the date (2017) should come at the end and not in the middle
You are right.

Line 1857: Millot & Garcia-Lafuente 2011 is not referred in the text
You are right.

Finally, Dear Referee #1,

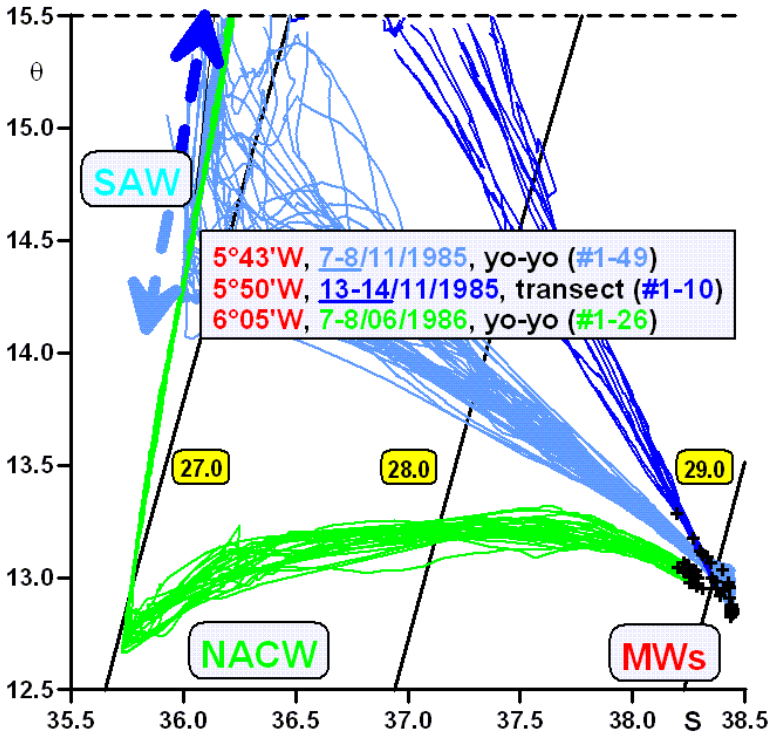
Let us specify that we copied to Referee #1 some of the answers we made to your comments. Reversely, please find below two of our answers to Referee#2 which might be of interest for you.

We very sincerely thank you for your help.

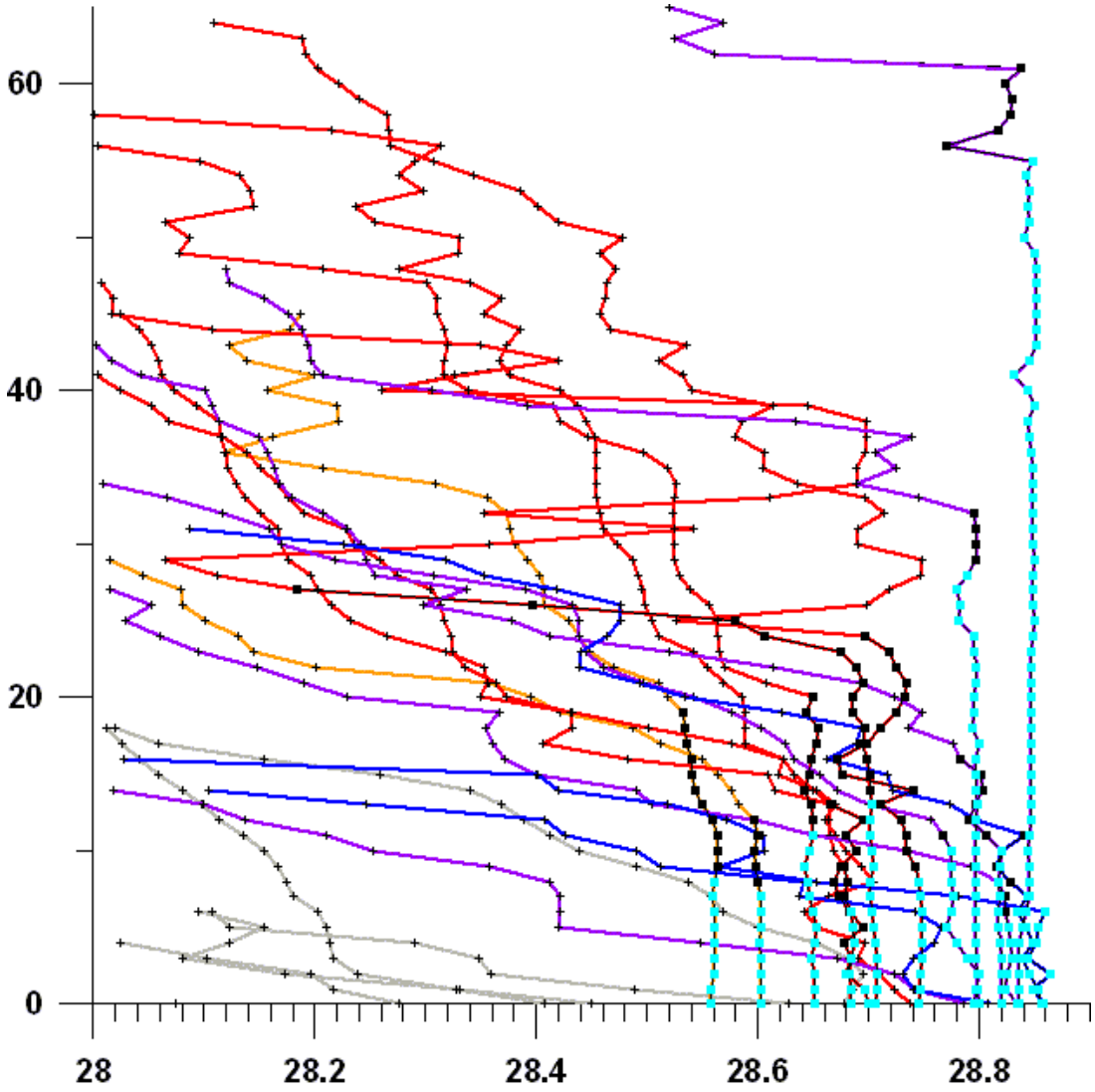
With our kind regards,

Claude and Mikhail

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 σθ=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.
Sorry but we have to stress the fact that, even though we agreed with the Editor that it would have been important to have referees willing to review all three papers, it is a pity that you were seemingly asked to review only this Part 3.
Even though we agree with you on the fact that "all starting points lie on the σ_θ=27 isopycnal", we disagree with you on the fact that this line could represent NACW. More efficiently than any discussion, please consider the figure here below that will be Fig.1 of the Part 1/4 to come and is, in a simplified form, Fig.1b of the actual Part 2/3. We could obviously provide you with the whole diagram but would you continue saying that "the starting points of the dark blue mixing lines in particular (out of this figure) represent the NACW"?



May any vertical profile of θ and S help? Any hint will be welcome.
This an extremely valuable comment. Yes, for sure, we could add (even more valuable) σ(z) profiles … as we did as inserted figures with all the θ-S diagrams in our Part 1/3 paper!
We could, for instance, add a single figure showing all σ(z) profiles of this cross-MO transect. Just because the spreading in density is relatively large while the spreading in thicknesses (we would not refer to depth) is relatively low, a convenient figure (all points colored with one of the five colors according to Fig.2b) would clearly illustrate both the heterogeneity of each MW and the differences between the MWs.
We blame ourselves for not having had such an idea. **We sincerely hope the Editor will accept that we add such a figure in our Part4/4!**
This figure is "very rough" since we rapidly drew it so that both you and the Editor could rapidly let us now your opinion and thus allow us to re-handle our paper "asap".
First, let us say that this figure, drawn from roughly the information reported in Fig.2a reveals that our estimation of the two thicknesses of "very homogeneous" (in cyan) and "roughly homogeneous" (initially in blue, now in black to avoid confusion with the blue MW) can be markedly improved, and even maybe reformulated. Indeed, for instance:
-the thickest violet layer can be somehow correctly described with this two-color classification
-but the second thickest violet layer was clearly not: maybe the cyan layer could have been increased by 4 m and the roughly homogeneous layer could have been defined above.
In any case, we will re-handle Parts 2/3 and 3/3 on the basis of such figures.



Now, hoping you will provide us with your comments on this very important figure, please could let us now what could be the criterion you had in mind?