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 #2 (comments received on 28 December 2017)

Dear Referee #2,

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, since we noticed you have been nominated only on 17 November 2017. Let us also specify that CM never (sic) received such a lengthy review (while having published more than ~100 papers) so that we know we are very lucky, furthermore the Editor already provided us with lengthy and helpful comments about our Part 1 paper.

Before answering your comments one by one, please let us specify that, having noticed your 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 #1'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 could not want to engage yourself in another review (you are clearly not one of the two Referees of our Parts 1 and 2 papers), let us specify that we would appreciate any "friendly" (i.e. not official) comment (even just a few words!) on the Parts 1/4, 2/4 and 3/4 papers when they will be submitted (before mid-April 2018 if accepted by the Editor) that you could send us in a fully anonymous way with the help of the OS office. In any case, we warmly thank you for all what you did for us.

Please, note that we have split your paragraphs in order to make more focused answers, just to facilitate our discussion and reach a common understanding.

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.

Yes, we actually deal with "the Mediterranean Waters (MWs)".

Following our previous papers (a summary of which can be found in Millot (2014a)), we specify that "the Intermediate MWs (IWs) outflow" while the "Deep MWs (DWs) overflow". We think that this is a basic difference since the IWs enter the Strait over the Camarinal sills with a significant velocity (hence are submitted to the Coriolis effect that will constrain them on the Spanish/northern part of the Strait, i.e. on the right-hand side of the "Mediterranean Outflow (MO)") while the DWs

do not have any significant velocity there, which will make them able to be pushed by the IWs along the Moroccan/southern part of the Strait, i.e. on the left-hand side of the MO, and even over the Moroccan shelf.
We specified, since Millot and Taupier-Letage (2005) that we reserve the word “Sea” to the whole Mediterranean and deal, in particular, with the “western basin of the Sea”, the westernmost part of it being the “Alboran sub-basin” we commonly name “the Alboran”. We also think useless to specify that the Strait exit is in the “Atlantic Ocean” and just specify “Ocean”.
As for what concerns the “sills of Gibraltar” and even though we know that sills are sometimes identified at Espartel, we only consider personally the “southern and northern Camarinal sills”.
Please, consider that all our answers are written with these specific definitions in mind, which can lead to answers not directly consistent with remarks and questions. Doing this, we will avoid specifying “what you call ...”.

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).
Aïe Aïe Aïe ... (ouch!, ¡ay! …).

First, we could accept the name, after “the transition” we suppose to be the Camarinal sills (~5°45’W), of MOW’s. But we consider:
1) what our Part 1 paper evidences: MWs, not MOW’s, are clearly identified at 5°43’W, just 2’ of longitude upstream from the sills,
2) all waters, in particular MWs, are transformed continuously all along their course from their zone of formation downstream,
3) our Part 2 and 3 (this one) paper demonstrate that some of the MWs/MOWs, namely the densest ones “could be” (this is our hypothesis) not so modified while crossing the Strait.
This is why we did not think necessary to specify MOWs and would like to continue using only MWs.

Second, it is not only the “classical oceanographic literature” that is “describing” (we would said “claiming” … and would accept that it could be said “we personally claim for another description”!) “a Mediterranean Water mass” since “all papers” (sic, up to the 2017 one we are referring to … excluding ours) are doing so!
And we demonstrate (or hope demonstrating), or claim, that what is in the Ocean cannot be considered as a unique water mass but should be considered as a set of MWs/MOWs!

Your comment thus clearly specifies the controversy (as introduced in the first Part of our trilogy or eventually tetralogy) about the veins in the Ocean between:
1) A unique Mediterranean Water mass (the general understanding … including yours)
2) A set of MWs that can still be identified, even though markedly modified (our understanding).

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).
We are sorry to specify that, even though we will obviously benefit from your comments in any case (be our work finally published or rejected by OS), **your report is (for us) essentially addressed to the Editor!**
Because the Editor kindly and honestly specified us he was not a specialist of neither the Sea nor the Strait, we need to give answers as explicit as possible. The Editor must be able to understand

both “the scope of the acronyms you are using” and “our own acronyms and understanding of the processes”. **He must be able to clearly understand both the agreements and disagreements we have, just to decide whether our original analysis is worth publishing or not.**

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.
First, we infer from this comment that you are “just” (like ourselves!) an “open-minded scientist”: we too obviously, as in any of our previous papers, “put a warning for our possible errors”. You probably know that at least one of us (CM) has been essentially working in the Sea before focusing, since the HydroChanges program, on the Strait. CM wrote, in several occasions, that he does not consider himself as a “Gibraltar expert”, just as a “Sea expert” essentially interested in the understanding of what the MWs he has always studied in the Sea were encountering when they escape from the Sea

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.
Thanks for having emphasized what we consider is a major interest of our work. Now, whatever the technological improvements one can imagine, I hope you will be convinced that it will “always” be impossible to remotely measure T and S at depth. It will “always” be necessary to have sensors directly sampling the water you want to measure. I agree that one can imagine autonomous vehicle able to automatically cross back and forth the MO. But the necessity we stress of a continuous record of hydrological (and other) parameters across the MO will always remain.

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.
We fully agree with you and already emphasized the tremendous interest of this MO-2009 experiment which, even if not initially focused on the hydrological characteristics of the MO, has used strategies such as yo-yo time series and repeated transects which have proven to be very efficient for our purpose.
For the Editor: we notice you deal, in an uncommon way, with the Gibraltar sill, which is right on a large scale. Specialists of the Strait mainly deal with the Camarinal sill(s) that is not only the shallowest part of the Strait but is also the narrowest, which leads to specific hydrodynamical constrains and, for what concerns our purpose, both an increase in the mean velocities (and, we personally stress, of the associated Coriolis effect) and in mixing processes. The oceanic side of the Strait is for us the Strait exit.

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).

We appreciate your comments and hope they will convince the Editor that it would have been valuable to have a Referee like you (or you as a referee) for our Parts 1 and 2 papers!

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".

We perfectly understand your comment and agree that our papers would benefit from a "less passionate writing". Now, considering your expertise in T-S analyses and, clearly, your expertise in the Sea hydrology, you certainly know us personally and are certainly aware that at least one of us has always been passionate and has always been fighting against generally assumed "theories" about the circulation and formation of various MWs within the Sea.

What you should know about Gibraltar is that we have "always" (since the circulation diagrams we proposed in the Sea) been against this "myth of a homogeneous MO" and that we have been addressing this myth more specifically since our Millot et al. (2006) paper. And we have thus been fighting essentially with the team in Malaga that, you certainly know, is considered nowadays as "the" specialist of the Strait and essentially still supports the historical papers you mentioned.

And what you should also know is that at least one (probably two) member(s) of this team have rejected a preliminary version of the present paper that we submitted for a special issue devoted to, in particular, a Spanish friend of us who was retiring. I am sure about that because I contacted this colleague with whom I have also been collaborating and who has accepted co-signing a paper of mine. Whatever the case, I think he was (they were) not fair in rejecting our paper: they should have given us the possibility to publish our own point of view!
Now, we have been lucky since we have very much improved our paper since that time.

Back to the analyses, the idea to characterize the profiles according to their maximum density (σmax) is very useful for the purpose of this paper, as it defines the MO water type of the mixing lines in the profile.

Thanks for this comment. Now, it is a pity that you were not asked to review the paper devoted to the Strait entrance. Please have even just a look on it: you will be convinced, from just a plot of some σ_{max} values, that what you call "the MO water" is composed, just before Camarinal, of "a set of MWs" more homogeneous than anywhere else in the Sea. Now, in the preliminary version of this Part 3 paper that was rejected, we did not use the σ_{max} values; and it is while we were trying to improve our work and were looking for data upstream that we discovered, in particular, the yo-yo time series at 5°43'W for which the σ_{max} values have such a large importance. This has lead us to use them in all our work, in particular in this Part 3.

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 σmax would help to highlight the limits of the rectangles, in addition to the comments in the text and Fig. 2a.

Please, let us first make some general comments.

Claiming for homogeneity of a data set is a very easy and even trivial task which only needs considering it at a relatively large scale. Claiming for heterogeneity is a much more complicated task, furthermore in such a spatio-temporal variable environment. In the present case, just due to the bathymetric variability at the Strait exit (constraining of the available section, steepness of both the cross-Strait and along-Strait slopes) and the hydrodynamical variability of a hypothetically homogeneous MO (downstream momentum and energy evolution of the MO on which the MO-

2009 experiment focused … note that the acronym used by our colleagues is not MOW; barotropic and baroclinic tidal variability), spatio-temporal variability is extremely large. This variability tremendously increases in case the MO is heterogeneous.

When dealing with hydrological variability (whatever the consequences are from a dynamical point of view), you know for sure that the main (we think "sole") tool we have is a θ-S diagram. Trying to demonstrate heterogeneity with such a tool needs defining specific zones, hence geometric drawings. None can be perfect and we thought rectangles were the easiest to define and use, even though we are perfectly conscious that any specific zone will arbitrarily separate the points inside from the points outside … and that there will always be points "just on the limit"!

As for your specific comment about a "*a TS diagram covering the whole range of θ and S showing the θS point for all the σmax*", please do not forget that the MO-2009 Experiment covered 1) a nine-day period (Table 1, p. 8) and 2) a roughly squared zone of ~25' in both latitude and longitude. The more you extend in time-space any given data set the more you increase the variability, hence the spreading of points on a θ-S diagram. For instance, the θ and S of a given particle will continuously evolve downstream, which preclude any specific localization on a diagram.

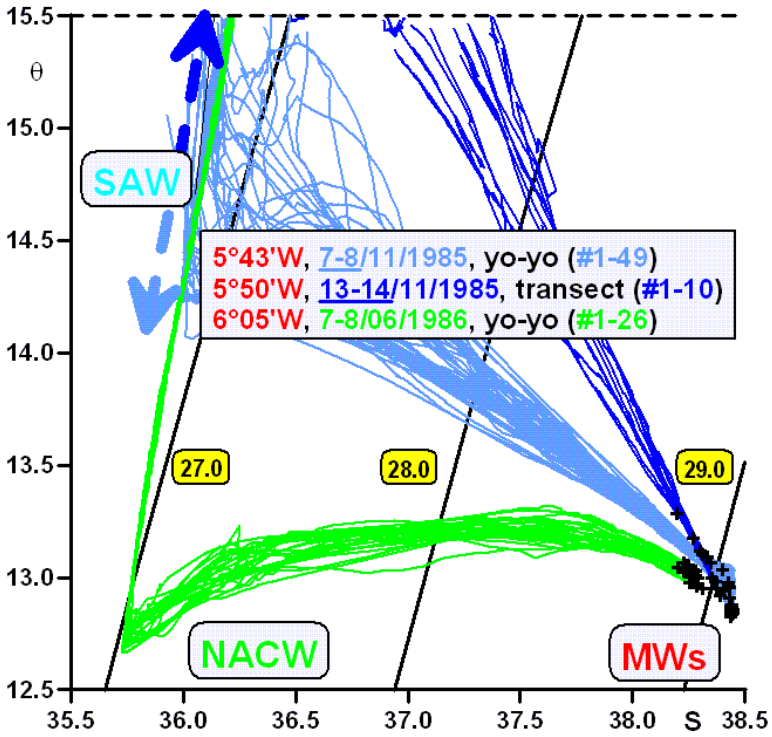
Seems to us the only way is to deal with diagrams obviously covering the whole θ and S ranges but only for "coherent" set of data, we mean data collected in relatively limited space (yo-yo time series) or time (a given transect) intervals. In such a way, what appears from all (sic) diagrams is concentrations of points near some specific places … that can be accurately positioned and easily compared, thanks to the green rectangles that, as specified in all our text, do not "define" the MWs.

This is why, to be as objective as possible, we first grouped the whole set of hydrological data (not only the σ_{max}) according to the sampling (from transects to time series). And the only question the reader has to ask her/himself is: do I see come concentrations of points or not? And it is because we personally saw, in the first plot of raw data we present (Fig.2a), such concentrations that, in a second step, we elaborated Fig.2b that is the basis of the whole analysis we present. Now, the general question the reader has to ask her/himself is: are the concentrations of points on different diagrams located in some erratic way or are they roughly located at similar places? Maybe anybody can have her/his own answer …!!!

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 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"?



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).

We obviously agree with you but, debating about homogeneity vs. heterogeneity can only be done by "experts" or, more realistically, using arguments that need a careful analysis of data sets using a simple tool (θ-S diagrams) that any oceanographer (not necessarily neither a physicist nor a specialist of the Sea) can perform.

Now, and just as an example, you, as a "Mediterranean oceanographer", know the General Director of CIESM who is spending a lot of efforts to promote the interest of the Mediterranean Sea. If we succeed in publishing our hypotheses (note that even though we think that the demonstration of the MO heterogeneity is definitive, we are still hypothesizing what are the actual processes) don't you think that this person will understand the impact these hypotheses could have for this or that organism or commission or whatever? And don't you think that, for all "Oceanic oceanographers", knowing that this or that Mediterranean vein in the northern Atlantic Ocean comes from this or that basin or sub-basin of the Sea (i.e. not only "from Gibraltar") will stress the importance of a better understanding of the Sea while allowing, in the future, a possible differentiation of, for instance, nuclear pollution coming from this or that part of the Sea?

Whatever the case, we agree with you and must specify that we submitted to OS thinking it was the adequate revue to submit such a "sophisticated" analysis. **It is the task of the Editor to appreciate whether the "sophistication" is too high for OS or not** and be sure that, in case this expert-dedicated presentation of our work is published in OS, we will be perfectly able to publish a general-audience version.

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.

We are sorry but we do not share your point of view.

Since we present only:

- θ-S diagrams, the basic form of presentation of hydrological data we (and probably you) were taught by our professors in the 1970's,
-2-D distributions of single parameters as functions of space or time,

what kind of "technical complexity" do you see?

And, for what concerns the "theoretical complexity", seems to us we just differentiate IWs from DWs, explaining why the former outflow (with a significant velocity) while the latter overflow (with a relatively low velocity), and just hypothesize some importance of the Coriolis effect. Is the basic Coriolis effect too complex to be understood by a general audience?

Among those:
There are many long sentences that difficult the reading.
We know ourselves and our tendency to write relatively long sentences … which just results from our willingness to provide the reader with as much information as possible.

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.
We dissociated this comment from the former one. We clearly have similar competencies in English and we too ask for an improvement of our language. But this is not linked to the length of the sentences. Even in "our English", we can write short sentences, just cutting the longest ones. The problem … asked to the Editor … is how much additional/helping information is required? We personally prefer having as much information as possible in order to be sure we understand an author's purpose. **We will follow all Editor's recommendations about our writing (accepted length of sentences, total length of the paper, level of additional information, etc.).**

The structure is quite mixed (see Specific comments below).
Even though we will answer with more details below, we agree with you on such a "quite mixed structure" which is why, following the Editor's personal analysis of the comments he received about our Part 1 paper, we proposed him to now submit a tetralogy.

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).
For us too, the number of figures is not at all a problem. Just because OS allows publishing, in a classic pdf file, what is called "Supplementary Information", we accept removing from the paper itself all figures (and associated comments) as requested by the Editor.

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.
We hope we have convinced you at least on some of your "formal issues" here above and that, since you have accepted re-reviewing our paper, you will be able to provide the Editor with a "definitive list of formal issues" that will finally allow him in taking his definitive decision.

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.

You are right, we are not fully coherent. However:

1) First of all, we want to specify that we actually did a non-coherent introduction and misused the MW acronym we defined. Your comment is thus sound and very helpful.

2) The western basin of the Sea has always (since the 1970's) been known to form only one intermediate MW (WIW) and one deep MW (the so-called WMDW for which you know that the M stands for Mediterranean, which is a somehow obvious, hence useless, qualifier).

3) The eastern basin of the Sea forms a series of intermediate and deep MWs. And one of us (CM) was a member of the CIESM group who published the 2001 document (we can forward it to you) that lists, in particular, several intermediate and deep MWs (we call IWs and DWs, furthermore the I and D were dedicated by the group to "intermediate" and "deep") formed there. We wanted to check what were exactly the IWs and DWs we listed, having in mind in particular a paper by B. Manca et al. (we did not retrieve and are unable to download for free now) that was dealing with the identification of IWs in the Ionian (sub-basin). Even though we have been surprised to realize that the most "famous" of our Italian colleagues at these times (M. Astraldi, G.-P. Gasparini, B. Manca) were not members of the group, let us just list these IWs (CIW and LIW; we have always been thinking that an IW could also be formed in the Adriatic) and DWs (ADW, CDW, LDW). Now:

-for the DWs: we have introduced the term EOW (Eastern Overflow Water), which can be considered as a correct one with our own specification that "overflow" necessarily means "overflow of DWs". Note that doing this, we assumed that ADW, CDW and LDW were no more distinguishable in the Channel (of Sicily).

-for the IWs: why didn't we introduce some equivalent term for the IWs … and why has one of us continued recognizing (up to Millot, 2013a,b) only LIW in the western basin … after having written several papers about the circulation of LIW in the western basin? In other terms, why did all of us have forgotten CIW for instance, and what about introducing an Eastern Outflow Water … an EOW-bis?

4) The CIESM (2001) document also mentions a "Tyrrhenian Deep Water" (TDW) which, probably, did not report correctly a study CM made in 1999 (following Sparnocchia et al., 1999) who proposed the acronym TDW for "TDenseW", arguing that the deeper part of the Tyrrhenian was occupied by, mainly, WMDW having crossed the channel of Sardinia. In our mind, TDW thus resulted from the mixing of 'EOverflowW" with the MWs resident in the Tyrrhenian. Therefore, we now think that such a TDW acronym is not the most appropriate one since it rubs out the actual origin of such a (set of) water(s).

5) To make all previous studies consistent while having acronyms as simple as possible, we thus proposed (in Part 1) and specify (in Part 3) that we color what are "in our mind", in both the western basin and, at least, at the Strait entrance (western part of the Alboran) with increasing densities:

-WIW: the unique IW from the Western basin;

-EIW: the set of IWs from the Eastern basin (that have outflowed through the channel of Sicily),

-EDW: the set of DWs from the Eastern basin (that have overflowed through the channel of Sicily),

-WDW: the unique DW from the Western basin.

Doing this we make a logical use of the I, D, E and first W letters while clearly specifying, at the Strait entrance, all the story about these components of the MO. We also consider that **all MWs or sets of MWs** encountered at the Strait entrance (and, we think, within the Strait and at the Strait exit) **are MOWs** … provided MOWs stands for **"both MoutflowWaters" (IWs) and "MoverflowWaters" (DWs)!** Therefore, we do not see any improvement in using MOWs, and **we propose to continue using the MWs general acronym and just clearly specify that it represents an ensemble of two individual and western MWs and two sets of eastern MWs.**

6) Considering your comment "*the acronym MW is currently being used for an Atlantic Ocean water mass*", we suppose that the M stands for Mediterranean. **If we are right, we cannot accept your recommendation** just because all the work we have been doing about Gibraltar, in particular with the present set of papers, aims at demonstrating what we have always been claiming: **"there is no MW in the Ocean"** or, using your own acronyms: **"the MW is a set of different MOWs"!**

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.

You are right. We do not answer in detail because we have proposed to the Editor to now submit a tetralogy, which will imply a total reconstruction of all papers.

Table 1 is very useful but not all the mentioned stations appear represented in the map (Fig. 1b), which is concentrated to the "cross".

Thanks for the comment but we disagree with your understanding of Fig.1b which does not focus on the cross-MO set of transects (in red). Fig.1b also represents all along-MO sets of transects (in pink and cyan) as well as all yo-yo time series (in yellow, green and blue). If you consider the latitudes and longitudes, you will see that Fig.1b focuses on a central zone clearly included in the overall zone surveyed at the beginning and the end of the experiment as represented in Fig.16.

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.

We understand and appreciate the comment but would like:

1) to specify that, such a general view would probably be more suitably shown in the new Part 1/4 (the overview).

2) we understand the necessity to have two rectangles, a large one for the surveys and a small one for the central region actually covered by Fig.1b … which does not show the sole "cross".

3) we agree that specifying the sills' names could be useful … but i) while we think the Camarinal northern and southern sills are actual sills clearly signed by isobaths, this is not markedly the case for Espartel, ii) Camarinal has a tremendous dynamical importance while more or less nothing has been reported for Espartel, iii) it is clearly specified in the text that the HydroChanges time series (green circles in Fig.1a) were set at each of the sills. Considering that specifying names for features not very clearly located such as the Espartel sills and not especially studied in this series of papers would complicate the figure while being neither very efficient nor very informative, we propose to just draw dashed lines across the Strait were the green dots are located.

4) We do not understand the splitting you propose. The surveys being described at the end of the paper, it is not logic to have the actual Fig.16 as Fig.1b and, in the actual Fig.1b (that would be 1c), there are no Part 1 and Part 2: there are two surveys (located in the actual Fig.16) that can valuably

be analyzed together and a series of cross-Strait and along-Strait transects as well yo-yo time series in the central region (in the actual Fig.1b). As long as we will be allowed to present a tetralogy and present Fig.1a in Part 1, we thus think that the actual Fig.1b and 16 could be presented in the same order and way.

5) We do not see a major problem in having, we think in the actual Fig.1b, marks completely hidden by others for the following reasons: i) we took care of having the most important cross-Strait marks in red not masked, ii) even if several cyan marks are hidden by violet ones, we do think this is not a problem since the overall location, size and extremities of the hidden transect are still clearly specified, iii) marks of the time series form big patches but we did not find correlations between specific positions during such time series and specific data. Finally, as an overall comment about the marks, you know that positions are generally those at the very first beginning of a vertical profile and that both the ship (now GPS-positioned, but radar-position in the mid 1980's) and the CTD (never positioned during such profiles even if this could nowadays be possible with an acoustic basis) drift during the profile; even if additional positions would have been specified at the end of the profiles, allowing the computation of intermediate positions (of the ship) when the CTD was close to the bottom (in the MWs we are especially interested in), we do think that such an accuracy would not improve our analysis. Which would not be the case with the strategy of towing CTDs that we propose!

6) We do not think that "*The proposed order of Figs 1a,b,c is related to the Table 1*". Table 1 is ordered with time and presents Survey-1 (Fig.16/your 1b), the central zone (Fig.1b/your 1c) and Survey-2 (Fig.16/your 1b).

7) Your "*(warning: there is an error!)*" is "helpful" and denotes your careful reading of our paper. In case we do not find the error (please, check the revised version to come), please let us know.

8) Yes, we will add the mention of the corresponding figure.

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).

We would like to comment on "*… a final paragraph…, …expected…,...characterize…, ...first TS diagram...*" all together.

-Why "in a final paragraph" after having tried understanding and not "in the Introduction" to help the understanding to come?

-We are sorry but "we cannot expect"! Please, remind the figure we included above!

-In the same way, "we are unable to characterize … a priori"!

-Last but not least, the first diagram in this Part 3/3 should not be, for a "normal non-expert" reader and even referee, the first one she/he would have to consider. In the first paper of the series (be it Part 1/3 or Part 1/4), it is/will be clearly specified that all papers must be advantageously read one after the other. We will add some kind of "general instruction" about what is the information potentially provided by such diagrams.

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.

You are right … even more than what you are thinking! Indeed, you might know in particular Millot and Garcia-Lafuente (2011); if not, please have a look at it and eventually at Millot (2014a). You will see that, from the HydroChanges time series (CTDs moored on the bottom), and even

though we do not directly measure the thermocline, **we demonstrate that the slope of the mixing lines (with time) close to the bottom at both Camarinal and Espartel displays a marked seasonal variability**: the mixing lines' slopes are much larger (directed towards SAW) in the winter than during the summer (directed towards NACW)!
Note that this remark provides us with another argument to answer your remark about the NACW extension towards the surface and the necessity we see in specifying the occurrence of SAW.

3. Data analysis

First of all, let us specify that your comments are, for most of them, so useful and numerous that we found it necessary to answer "sentence by sentence", even if we interrupt the continuity of your argumentation.

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 colored groups (see also the Technical corrections below).
At this stage of your comments, we understand that we should include (we agree for that) details as those we made in answering your general comment about the rectangles (roughly between l.190 and 230).

The first results shown correspond to the MO-Transect+St#479 of the Yo-yo Central Pt-2. The flavor 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).
Yes, this Fig.2a is "The" key figure of this Part 3/3 … and Part 3 is expected to be read after Part 2, hence after Part 1.
Yes, there are similar figures in Parts 1/3 and 2/3 (thanks for having had a look at them!).
Yes, and even if information must not be totally repeated on all papers, we agree on what was specified by the Editor: all papers must be "stand-alone".
We thus agree that, for everybody, information must be sufficient, hence must be increased.

St#, depth of the profile and the θS point of σmax 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).
We are first pleased that you clearly see the "*almost homogeneous bottom layer*" in the lower part of most of the AWs-MWs mixing lines.
But we are not sure about what you qualify a "*stratified layer*" and its associated gradient. We imagine that, on the basis of the 1-m data spread over a density range of ~0.05 kg.m^{-3} and a depth range of ~10 m, you define this "*stratified layer*" as the layer associated with the gray points. If this is the case, you are certainly aware that such a layer roughly extends over most of the water depth, linking the "*almost homogeneous bottom layer*S / MWs" with the AWs (as defined by the figure above included for instance). In this is the case, we do not think such a layer can be qualified as "***stratified layer***". As you can infer essentially from Fig.1/Part1 we more think about some "**interface layer**" between the AWs and the MWs. Note that we also evidenced in this Part 1, and represented in gray in Fig.8/Part1 as well as in the sloping parts of the σ(z) profiles added to all θ-S diagrams, interface layers between contiguous MWs!

How homogeneous are these bottom layers is not evident in the figure since no Δσ criteria are given (e.g. "homogeneous" bottom layers of #449 and 451, have Δσ as high as ~0.05 kg m-3).

We disagree since, even if not quantified, the homogeneity of these bottom layers can be very accurately estimated:

-First note that, specifying some "*Δσ criteria*" would have been totally arbitrary, hence not at all objective, and would necessarily have been incoherent from one profile to the other: indeed, and just because the 1-m interval is too large to accurately describe such rapidly varying "interface layers", the last point in the interface layer and the first point in the bottom layer (these are downcasts) would have been separated by different distances from one profile to the other.

-We have preferred "objectively characterizing visually" this homogeneity (comments below about the coloring).

-You easily and perfectly estimated the Δσ ranges … but you did not notice the Δz ranges specified in both blue and cyan after the profile #. You are thus easily able to specify the Δσ/Δz, hence able to answer "h*ow homogeneous are these bottom layers*".

-As for what concerns #449 (10, 16, 433) and #451 (8, 24, 420), you are thus able to specify that, even though both evidence a relatively homogeneous MW spreading over a relatively low Δσ of ~0.05 kg.m^{-3}, both have relatively similar thicknesses (10, 16 hence 26 overall; 8, 24, 32) and are located at relatively similar depths (433, 420), they evidence relatively/very different MWs that belong to relatively/very different groups of profiles!

Aïe Aïe Aïe ... (ouch!, ¡ay! …): in case you fully share our own analysis of the #449 and #451 profiles … you should reconsider your position about the "MW vs. MWs controversy"!!!

The rest of the information is not clear to me, for instance, what is the difference between cyan and blue points?

First, you can directly see that cyan σ values are larger than blue ones. And if you match the points spreading (visually estimated) with the number of points (quantified), i.e. with the thicknesses, you immediately conclude that the cyan denser sub-layer is more homogeneous than the blue lighter one.

I can realize that cyan < blue values. Is it relevant?

If your "<"concern σ values, yes.

Does it mean that cyan points are in the "most homogeneous" zone within the whole "homogeneous" bottom layer?

You are perfectly right. We would say "define" instead of "*are*".

Why this different colour treatment?

Just because "the larger the number of colors the more accurate the description of these bottom layers". Note that you can easily forget about such a differentiation and imagine an overall "blue layer": the "cyan sub-layer" is "very" dense, the "dark-blue sub-layer" is "slightly" dense and the "blue layer is "moderately dense"! It would have been a pity i) not to emphasize the "very homogeneous" deeper part and the ii) overall thickness of these layers of … relatively unmixed MWs!

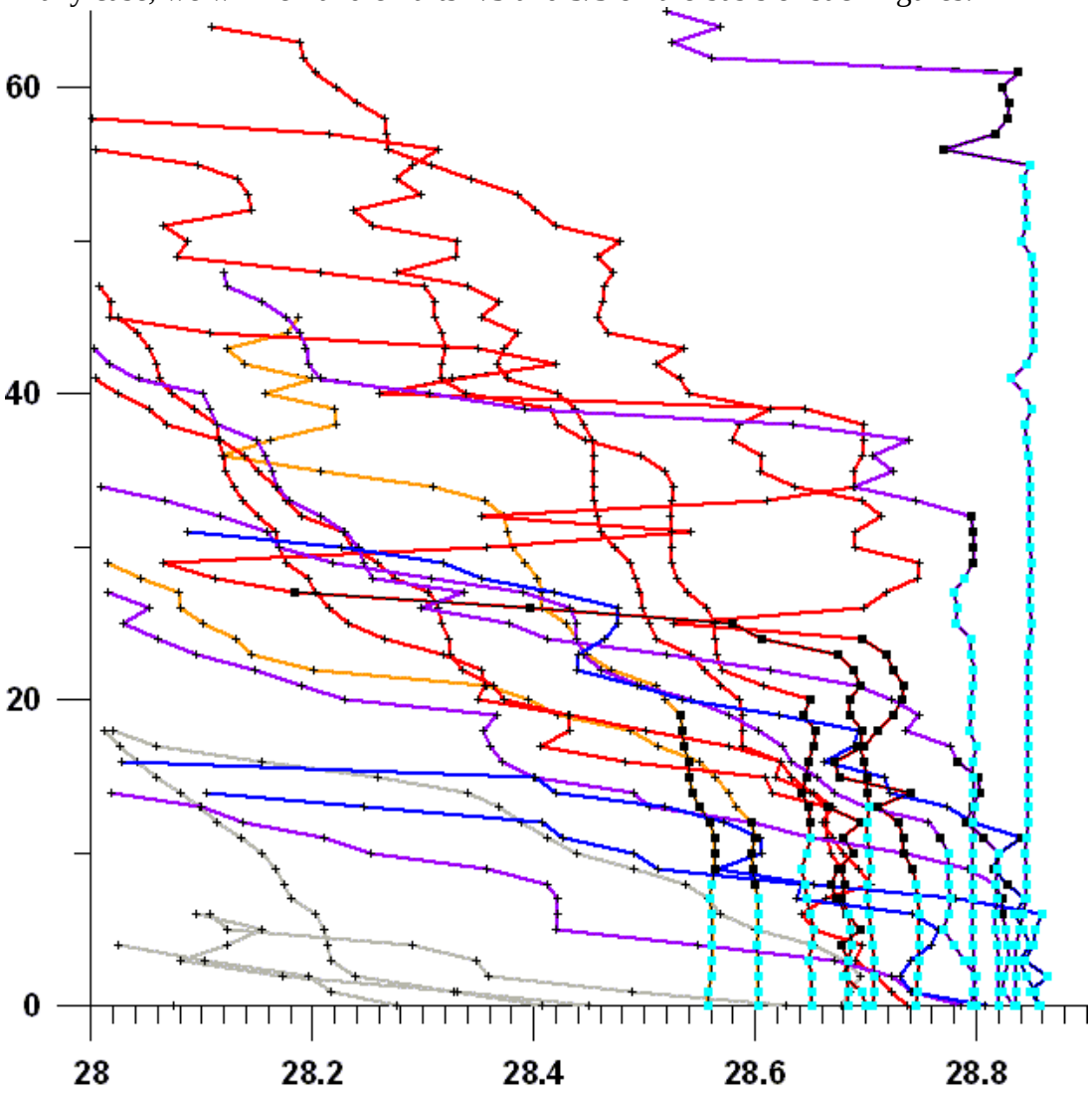
May any vertical profile of θ and S help? Any hint will be welcome.

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 know what could be the criterion you had in mind?

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.
Yes, more explanations are provided in the Part 1 and Part 2 papers, as well as with Fig.1c/Part2. All this will be hopefully (all papers in a tetralogy accepted by the Editor) improved. However, a figure such as the one you suggested would not be adequate for these parts: thicknesses are too large in Part 1 and sampling is too rough (in both time and space) in most of Part 2.

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.
We understand and hope you have now clearly understood.

Finally, it will be useful to know what is the σmax point for each one of the four not labelled profiles (#435, 445, 465 and 469) shown in Fig. 2a, as mentioned in L234.
We do not understand the interest of knowing the four associated σ_{max} values with an accuracy larger than what can be estimated from Fig.2a, furthermore we do not focus on the σ_{max} values associated with the homogeneous MWs.
We can make the same comment for the two σ_{max} values associated with #467 and 477 that are out of the figure.
Please, could you specify your comment?
Note that at least two of them have σmax higher than #475 and 455.
Yes … and so what? This is not for us a significant and/or even interesting comparison!

We do not want to influence you too much but you should consider that:
1) the gray color is objectively given only to profiles that do not show any homogeneous bottom layer … and we "blindly rely" on the "assertion" that the CTD was effectively lowered as close to the bottom as possible: the GIBEX teams were very experienced ones, the maximum depth recorded by the CTD is very close to the reported bottom depth… and we do not have any way to check whether reported bottom depth was correct on not. And relying on that, we just checked that the profile was as complete as possible.
2) as shown by Fig.2c:
- all gray profiles are encountered at one or the other end of the cross-Strait transects,
- #467 and 477 are the northernmost ones on the shallowest depths. They are characterized by values of $\theta>13.8$ °C, $S<37.6$ and $\sigma<28.2$, hence being associated with mixing lines that are similar to those associated with #455 and 475 colored in orange, as is the still northern and relatively shallow #465 associated with the lightest (and warmest) of the σ_{max} values displayed in Fig.2a.
- #435, 445 and 469 are the three other gray profiles collected at relatively great and similar depths in the south, the lightest being #469 and the densest #445. Note that the associated mixing lines are similar to those associated to the densest (blue) of the MWs. We will number the gray profiles in Fig.2a.
3) You should thus conclude that profiles in the south (resp. north) reach relatively large (resp. low) densities, be they reaching the blue (resp. orange) MW or not, the whole mixing line in gray (out of the MWs) being thus influenced in both position on the diagram and intensity by the involved MW.

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.
We will try doing our best.

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!).

Thanks for the positive comment … and good luck with the technicians!

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.
We hope you noticed that, whatever the color (blue or gray), *the presence of θ minima (referred to the NACW in the text)* is essentially a characteristic of the southern profiles, the more to the south (hence the "more gray") the profile, the more the signature of the θ minimum.

Note in particular that there are two kind of "grey" profiles: (i) those with low σmax, typically on the north side, without θmin, that are "similar" to the red and orange, and (ii) those with high σmax and θmin, that are similar to the blue (e.g. Fig 4).
We are pleased to notice that you are exactly reacting … as we wanted! Indeed, we specified in Part 1 (l. 109-111): "...With such a basic evidence in mind, the reader is thus proposed, all along our trilogy, to make his/her own point of view about the characteristics of the MO heterogeneity, our personal results and analyzes being only proposed as guidelines ...”!
Yes, you thus fully share our own analysis.

If a line joining the tips (σmax) 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.
We understand you deal with your analysis of Fig.3a and b. We have a somehow slightly different analysis since we would describe first of all the increase in θ and the decrease in S, hence the decrease in σ, of all colored profiles from south to north. This clearly demonstrates that the MWs that are superimposed at the Strait entrance are now juxtaposed side by side, with the densest in the south, and that the densest have encountered less mixing (less decrease in density) than the lightest. Then, out of the MO, mixing lines in gray roughly indicate similar differences with the closest of the MWs (the blue in the south and the orange in the north). Such a line such describes a √ shape, the left branch quantifying the difference. Obviously, such a shape directly depends on the shape of the AWs diagram.

The along-MO transects are typically dominated by "blue" and "violet" waters.
Yes, but this is an artifact linked to the fact that these transects are only "so-called" along-MO transects and are mainly located in the southern part (left-hand side of the MO) of the Strait, hence sampling only rarely the northern part of it (right-hand side of the MO) as done during legs 8 and 9 of the along-MO Transect-1. Would have these along-MO transects been located more on the right-hand side of the MO, they would have been dominated by "red" and "orange" MWs. With "four equally spaced and strictly along-MO transects", each of them would have been dominated (due to the MO meandering and MWs amount variability) by one or the other color.

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.
It is not "looks like": legs were actually performed downstream.
It is not a matter of "irruption": it is just a matter of "so-called along-MO" transects that are "also cross-MO transects" but not long enough in the sense that they do not correctly sample the right-

hand part of the MO. As we previously said, to sample the whole MO, cross-MO transects must have gray profiles at both ends.

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.
Or it might be your misunderstanding: the northern ends of the so-called along-MO transects are in the southern side of the Strait (and vice versa). Orange and red waters are on i) the right-hand side of the MO, ii) the northern side of the Strait and iii) at the southern end of these transects (carefully read l. 1012-1021).

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 σmax should be replaced by σθ).
You are right. Note that all "re-visits" confirm! All "visits" (be they transects, yo-yo time series or survey) confirm the other ones: all our analysis are coherent with the schematization we propose!

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.
We understand your comment but: i) we show a vertical section only for leg 6, ii) first presenting the diagrams (Fig.8a to h) and then the Leg 6 section (Fig.9) would lead the reader to "come back" t Fig.8f after having analyzed Fig.8h, iii) would have the Leg 6 be performed with the LADCP functioning, its single analysis would have justified a single dedicated paper! **Whatever the case: are you convinced that #759 did sample and interface layer between the blue and violet MWs?**

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.
If we understand well your comment, you can imagine the results when having a look at Fig.18 that stacks two surveys, or all figures such as Fig.2c, 5, etc. Due to the natural variability of the MO components, essentially due both to the relative amounts of each of the components and to its meandering, you will come with, for instance, zones essentially blue and violet separated by a zone mixing blue and violet. Also, due to the incomplete sampling during the so-called along-MO transects in particular, you will have a biased information about the MWs amount. Please, compare the "blue and violet image" you have in mind from all data sets in the central zone with the "mainly orange one" you have from the surveys. Therefore, we do not think your suggestion would provide valuable information.

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.
Yes, our writing is misleading … but we do not mention "*positions*", we deal with "locations". Whatever, it is clear that both the "location in space" (Fig.1b) and the "location in time" (Tab.1) must be specified and we will re-write the sentence!

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).
Yes, green points are more or less hidden … "and so what?": we previously argued for that and we continue thinking that the most important is to effectively be able locating the green CP at the center of the "cross". For the time being, we do not see the problem with Fig.9 but will answer about 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?

The ship didn't have a dynamic positioning system and was maintained at a yo-yo position by means of engine operation. The software used (IFM GEOMAR/LDEO, ver. 10.3, Sept. 2007) to process the LADCP data corrects them either perfectly, when the LADCP signal reaches the bottom or on the basis of the ship drift inferred from GPS data.

Of course during GIBEX no GPS could be used.

We would say "no GPS could be used efficiently". At those times, we were working off Algeria: we performed CTD at places specified either with radar measurements or classical estimation over time, but we waited for satellite positions (a few times a day) to launch sub-surface moorings.

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).

Or km or nautical miles, no problems. Our intention was just to give a number that could be directly, easily, visibly and accurately estimated from Fig.1b were latitudes and longitudes are specified with decimal numbers.

Fig. 10c. Does the depth interval from the crosses to dots correspond to the "homogeneous layer" mentioned in Figs. 2a? (see my comments above).

It is clearly specified in the caption that + specify the depth of the σ_{max} while the dot specify the bottom depth: the depth interval is thus the altitude of the σ_{max} above the bottom.

However, if one looks at the vertical sections of θ, S and σθ (Fig. 12a), doesn't seem realistic. For example, the cross (σmax) 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?

Seems to us "your problem" is essentially the problem of "kriging" (see our comments l. 380, 746, 787, 796, 985): essentially, sections represent smoothed data sets contrary to all other figures.

In any case "homogeneity"criteria must be given.

Sorry but we already explained why we are personally unable to specify a criterion that would be adequate for all situations of mixing. Any criterion (a number) will not allow matching all situations in the sense that visual displays will not be representative of the mixing intensity and of the homogeneity. In case you have idea for specifying one, please, let us know.

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

We are pleased to note that the general point is clear, so that we will simplify and try improving the writing.

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.

Sorry but everything is correct. The sentence says: "Such an inflexion is also observed on the next profiles (#495-499) that are blue, before it disappeared for both the other blue #501-509 and the violet-blue #511" that is consistent with Fig.11a: Among the set of blue profiles #495-509 (as specified in the "lowest" green triangle), only profiles #495-499 (as specified in the left of the figure at the 13.45 °C level) display the inflection.

L986. What is correctly represented in Fig. 12b (LADCP)? I presume it is a typo and should say

Fig. 12a (σθ). Please check it and correct or explain.

Please, consider our comment above about "kriging": any section of any parameter drawn with any kind of software "correctly represents" the actual distribution of this parameter only if this parameter actually displays smooth variations, which we demonstrate referring to numerical values in this case. But in the case of Fig.9 for instance, the variations we are interested in are too large for being accurately represented with a software: even if less "beautiful", isolines must connect measured (not interpolated) values … which introduce another problem during a transect (the CTD is moving in both depth and distance, which is not the case during a yo-yo).

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.

Sorry, we did not retrieve the mention of 203°. What we say is that the mean U direction computed from the eight profiles is 247°T so that >0 V are effectively towards 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.

Sorry, we are not sure we correctly understand your comment. Shifting cannot be directly inferred from these sections since one must consider a U/V ratio. We could have provided a section of the shifts, maybe in place of the U and V sections. In any case, we did not find any correlation with hydrological data and just wanted to emphasize the overall variability of the MO.

The 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)?

We did not find possible relationships.

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.

Sorry but we do not fully agree. Seemed to us that, in addition to all the "full θ-S diagrams" we previously published as well as the diagram we inserted in our answers, Fig.3a and 3b provide significant examples of what is actually occurring independently from the seasonal variability (see one of our previous comment and Millot and Garcia-Lafuente (2011)). Using your own understanding of "*the NACW line*", you must be convinced that the slopes of the mixing lines in the south (resp. north) are directed roughly to the "lower/cooler/fresher" (resp. "higher/warmer/saltier") part of this 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.

Sorry but we did not find any other objective and common way to describe such surveys.

Why not to include a TS diagram of the whole regional surveys to "locate" the details to be seen.

We think this would be an inextricable "dish of spaghetti": diagrams are varying both locally with time (between two surveys/transects/yo-yos), and spatially (assuming surveys are performed "at a given time). But obviously we can provide such a diagram … what is partially done in Fig.20 … provided we are not asked to comment it and/or add any specific information.

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).
We are far from being convinced by such a help and could just have modified the ordering of these figures, or first presented an "overall analysis from Fig.17e … named 17a" before going with detailed analyses.

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.
We disagree since:
1) we clearly said (l. 1187-1189): "The green rectangles are still displayed in the figures hereafter to allow easier comparisons with previous ones, but since the coloring must consider the along-stream mixing, figures must be analyzed with Fig.16 in mind." The "rule" we defined from the cross-MO transect cannot apply there.
2) depending whether the surveys transects are upstream or downstream from the cross-MO one, sampled waters are either less or more mixed … while both the MWs and the AWs have changed. Colors cannot match and/or a given set of colors/rectangle cannot apply for such a too large (in space) and long (in time) experiment.
3) Whatever the case, we rely on the fact that the mixing lines either upstream or downstream (hence their prolongation), are consistently colored according to the rectangle they cross.

In any case, Fig. 17a shows the south to north sequence of σmax characteristics I mentioned above (increase of S then increase of θ and decrease of S, from south to north).
Again, we do not "share" this description (see above): from south to north, the density of the MO components decreases. Out of the MO, the density is still influenced by the nearest MO component.

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.
We disagree. Let us first be surprised by the fact you are still using "*the water mass*". Considering that the "*easternmost zone*" is in fact the "easternmost transect" (4 and 5 profiles during surveys 1 and 2), the 3 intermediate profiles are: violet (1) then orange (2), violet (1) then red (2), blue (1) then violet (2). Overall, the MO has thus shifted towards the south.

Most of the comments on TS diagrams to illustrate the regional surveys mention depths that do not appear in the figures.
You are right but: seems to us that all bottom depths can be easily inferred from Fig.16. Bottom depths were specified in dedicated figures for all analyses in the central zone and could be specified in the diagrams of all profiles from the surveys (just an additional information). We think that we specified correctly all the "thicknesses" we are dealing with.

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).
As you specify yourself, this is speculation that cannot be tested with such a too large sampling.
Please, also consider that we already have "too many non-speculative results" to present in a single paper ...

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 point 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.

We are not sure we correctly understand what "*deflection*" and "*diversification*" mean. In any case, we think that the sampling is too large to get results as definitive as the other ones presented before.

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).

Thanks for the information. We will have a look at the paper.

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).

We never use in our text the terms "*deflection*" and "*diversification*". We deal with "splitting". We are thus not sure we correctly understand your comment.

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.

Thanks for your agreement and support. One of us (CM) has been using pingers for the while and perfectly imagines himself in front of a screen with a microphone in hand, or just close to the "winch-man" asking for "5 m up" or "2 m down" … while having asked the bridge to sail "as slowly as possible, ship maneuverability permitting (l. 1559)".

In this experiment, the CTD winch was controlled from the laboratory using a joystick. The operator of the winch had the opportunity to observe changes in the bottom relief on the sonar screen and simultaneously was seeing the vertical CTD-profile and altimeter data on the screen of CTD acquisition data computer. In order to avoid a collision with the bottom in the area of strong currents and changing bottom relief, the rope with cargo is hung on the CTD's protection cage, and, in case of touching the bottom, dramatically changes the tension of the CTD cable. The measurement technique with the aid of a towed CTD probe was developed and successfully used until now in the Atlantic Branch of the Shirshov Institute of Oceanology of the Russian Academy of Science (Paka V.T., 1974).

Perhaps fast bottom layer profiles, from just near the AW-MW interface to very close to the bottom, with the ship maintaining the position …

As demonstrated by all MO-2009 transects, tow-yo strategy is a very easy and valuable one … not along-Strait (aliasing) but cross-Strait. I would just try to perform them "as slowly as possible, ship maneuverability permitting (l. 1559)" while asking for as rapid as possible up-and-down casts.

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

This is a copy of our answer to Referee #1: Yes, major elements of this sentence are: " ...some original ΔσM at the Strait entrance, ... by some final ΔσO > ΔσM 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.

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.

You provide us with an additional argument about our remark that "it is a pity not having asked you to review the Part 1/3 paper". In particular, we explain our understanding of the DeepWs specificity. In this respect, the amount is (obviously at least in our mind!!!) less important than the mean density value. In other words, if a "*dramatic amount*" is associated with a "dramatically low density", then this WDW will "almost directly" overflow through the Strait. But, if it is associated with a "dramatically large density", then this WDW will sink down to the bottom of the whole western basin and it will uplift all the "older and lighter" WDW above … the characteristics of which have to be monitored simultaneously at the Strait entrance. We will eventually add additional comments when submitting (if accepted) our Part 1/4.

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.

Yes, as a sea experimentalist, we too!

The point is that CM has done most of the figures without any help. In case you know how to automatically plot axes in degrees and minutes with Grapher and Surfer, please let us know. Otherwise we will specify some correspondences in the captions.

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…

I can understand but …: I wrote my first paper in a lab of 20-30 persons in total with four (sic, 1 woman and three men) of us employed as full-time "draught-persons", hand-making all our figures with Indian ink. I rapidly preferred drawing figures by myself, obviously in B&W, sometimes by hand and till 2005 for circulation diagrams and with graph plotters (never in color) for plotting data. And I fully agree that B&W figures are "more smart" (like dinner jackets!) that color ones.
Unfortunately, Editors require papers as synthetic as possible so that the number of figures is limited … and we already have many figures so that coloring seems to be for us the only way to synthesize sufficiently our results.
But, since our three papers make an intense use of coloring, please could you just suggest how you would draw a figure as essential as Fig.2b? Would you use different tones of gray? Or lines with different symbols? And how would you comment this figure in the text? Don't you think it is somehow more efficient, simple and more clear to deal with colors?
Anyway, we will follow the Editor's recommendations (hoping he will accept colors!)

Let's just apply the above suggestions to Fig. 16 that introduces Figs. 17.
Why do you choose as an example one of the simplest figures to reproduce in B&W?

Change coordinates in the axes to 35°42' N, 35°48' N, … and 6°36' W, 6°30' W…
By hand or with which software?
Remove shades in bathymetry. Isobaths are indicated so the reader can recognize the bottom structure without shades.
All maps I have seen up to now (except paper charts for navigation … when they were the only available ones), including those hand-made by our four-person team, are shaded ones. Even without glasses, in case you have specific problems, or if you are too far from a screen during an oral presentation, shading and/or coloring gives you an easy overview.
As a sailor, CM also found tremendous advantage in the coloring of bathymetric maps: even in the middle of the ocean, seamounts or shallows are made much more obvious with colors than with numbers.

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.
You should probably be an expert also in figure drawing (not only in Mediterranean hydrology). As a researcher (having worked alone most of the time and now retired), I prefer spending my time in "thinking", not "drawing" and am sorry but I use the software I am able to use. You can see in Fig.16 that I know how to use different symbols. But why do you need "*finding a color palette*"?

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-σmax set with respect to the rectangles in green defined from isotherms (13.65, 13.55, 13.40, 13.28 and 13.18 °C) 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).

We are not sure we understand your recommendation.

Yes, Fig.2b is a key-figure. We thought that the description would be more suitably done in the caption to be sure having both close together while being able to easily retrieve the description. Do you suggest to move this description in the text or to duplicate it in the text?

As for "*the text must also include an explanation why some of the stations are "bi-colored""* let us specify that you copied the explanation ...just 6 lines above!

For what concerns the "As in Fig.NN ..." that you seemingly find too numerous, do you suggest the information be repeated for all figures … not considering the total length of the paper?

Whatever the case, we will follow the Editor's recommendations.

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 σmax 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."

We think that saying "see … in the text" is an information less accurate (that will require more efforts to the reader who will have to look for a sentence) than "see the Fig.NN caption" or "as for Fig.NN" (it is easier for the reader to look for a figure number).

Whatever the case, we will follow the Editor's recommendations.

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).

Seems to us that you just cut in our captions the mention "Colors as in Fig.2b"

Whatever the case, we will follow the Editor's recommendations.

Again, why not to repeat the meaning of the discontinuous lines represented in Figs 5 and 7?

Because the meaning is the same for all these figures … and we need to "save lines".

Whatever the case, we will follow the Editor's recommendations.

We moved up this comment.

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. It also seems in contradiction with the "gross" position of the Central Points in the map (Fig. 1b).

You are probably right, we probably made a mistake and will eventually correct Fig.9. The conditional is just used because we will check during the re-writing.

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).

Even if the relative positions are "*repeated insistently*", we think that, obviously the error must be corrected, but this is a minor comment since we do not make comparisons of data collected during the along-MO transect, or during the yo-yo time series or even during the cross-MO transect. Even

if it is important to locate the various samplings relatively to each other, the comments would be the same be CP located between #753 and 755 or between #759 and 761.

Also in Fig. 9. The distribution of profiles is set according to time
Sorry, profiles are distributed according to their position along the transect, not time
but since the section roughly runs from E to W,
Sorry but the section runs more from ENE to WSW
why not distribute the stations according to longitudes?
Because **the "key-section" is the cross-MO** one that roughly runs from SSE to NNW, so that Fig.2c shows data as a **function of latitude**. And positions of the yo-yo time series are just specified, in "this first and key-figure", as a function of latitude. Since along-MO transects are straight lines, we have preferred continuing with plots as a function of latitude.
thus reversing the X axis (labelled accordingly).
What is the interest of this?
This would make it easy for interpretation in a quick look
We do not understand where is the easiness!
(this of course cannot be applied to the other vertical sections, shown in Fig. 12) where position does not change.
So why having different representations instead of a unique one (since a straight transect can obviously be plotted in either longitude or latitude? Would you like having both latitude and longitude?
Whatever the case, we will follow the Editor's recommendations.

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.
They seemed to us visible. But we will darken them.

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.
We would personally have no problem doing this.

Again, paper should not be the limiting factor today.
We understand you mean *"paper length".*
Dear Editor, what do you think about that?

These are just suggestions but they will simplify the reader's life and contribute to a better understanding of the paper.
We obviously rely on you and are sure you just aim at helping us as much as you can. But, you now, we have to satisfy the Editor first of all …!!!

To conclude:
Dear Referee,

we have sincerely appreciated all your scientific comments, furthermore some of them led us to markedly improved our analysis.

Again, it is a pity that you, clearly a specialist of the Mediterranean hydrography, were not asked to review Part 1 and 2 (we cannot imagine you preferred reviewing only Part 3), furthermore we have now received all comments and have had three different Reviewers #2.

Since you asked receiving the revised version, we hope you will be satisfied. However, **we would appreciate receiving**, before our re-submission that will occur in mid-April 2018 if the Editor accepts a tetralogy, via the OS office to preserve your anonymity, **a short "reaction" on some general points**. For instance, and mainly:

1) Would you continue dealing with "a MW mass in the ocean" or with "a series of (obviously modified … but not so much … in any case "distinguishable") MWs"?

2) Would you continue thinking about a NACW line or considering also a (seasonally variable) SAW?

3) Would you continue asking for a Δσ criterion to numerically delimit homogeneous layers and what would it be?

For doing this in an interesting (for you) and efficient (for us) way, we think you should first have a (closer) look at our Parts 1 and 2. **Would you accept, could you just send us (with eventually a copy to the Editor … *only in case you find these Parts 1 and 2 worth publishing, obviously!!!*) within just a couple of lines, some general comments. For instance:**

1) are Fig.4,5/Part1 interesting figures that display features the whole community never expected up to now?

2) do you think, from Part 2, that the MO can be considered as homogeneous in the Strait?

Sincere and numerous thanks for your help.

Claude and Mikhail

Thanks for these reference.

References

In addition to those appearing in the manuscript:

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