**“Definitive evidence of the Mediterranean Outflow heterogeneity.
Part 2: all along the Strait of Gibraltar”**

Answer to Referee #2 (Comments received on 2 January 2018)

Dear Referee #2,

Your first overall comment:
the paper presents valuable data and evidence of heterogeneity of the MW current in the Strait of Gibraltar.
First of all, I note you agree “***the paper presents*** *valuable data and* ***evidence of heterogeneity****”!!!*

This is for me, and should be for the Editor, a key-comment which clearly emphasizes the valuable scientific interest of my work … at least in the Strait itself, which is the region essentially concerned by the last-published studies, hence necessarily both upstream and downstream!

However, before answering your other comments which, I hope you understand, is done **essentially to the attention of the Editor**, I would like to specify a series of points:

1) As I said in answering the five precedent reviews I received (you are thus the last one … which offers me the occasion to **provide the Editor with some kind of “overall answer**” before he takes his decision), I would appreciate receiving your own “reaction” to my answers, what you could do in a fully anonymous manner via the OS editorial office. I have not been able yet to access your Report (that specifies your overall appreciation and your willingness to review the revised version or not), but this cannot change my “plans”.
This being said, the Editor is aware about the fact that, even though this answer is the last one I write, my own answers about the two Part3 reviews are presently checked by my co-author who comes back to work on January 10, so that I cannot hope posting them before at least a couple of weeks. Furthermore these two reviews are relatively positive and do not ask for marked changes, my willingness is to provide the Editor with my answers to your “relatively positive -your first comment- and relative negative -below” comments **as soon as possible. Indeed, your review led me to propose efficient solutions to improve the whole set of papers and I would like to give the Editor a time for reflection as long as possible.**

2) A general comment I already received from the Editor is that all papers in the series should be “stand-alone” ones, with the first introducing the whole series. I clearly understood and accepted this very valuable comment and I already proposed him to split the Part1/3 in two, hence submitting a tetralogy. As soon as I finish the writing of this answer, hence not waiting for the Editor decision, I will start the elaboration of Part1/4 with already clear ideas in mind. In particular, **two figures will be moved from Part2/3 to Part1/4**: the diagram in Fig.1b will be enriched and presented as Fig.2-Part1/4 while Fig.1c will be presented as a complement to Fig.1-Part1/3 as Fig.3-Part1/4. I am convinced that this will markedly improve the “Presentation Quality”, the “Scientific Quality” and the “Scientific Significance” of the paper you reviewed.

3) Even thought I provided the Editor with a list (as required by OS) of five potential referees, with three of them having told me they would accept reviewing the series of three papers, I consider **I have been unlucky** in having had only one Referee (#1), out of those three, actually aware of all my work since having reviewed my three papers.

4) I answer your comments with my own “language”, for instance I do not deal with “the MW heterogeneity” but with “the MO heterogeneity” … and I do not use the general naming of “Gulf of Cadiz”, just because “strait dynamics”, and the dynamics of the Strait of Gibraltar in particular, have nothing to do with “gulf dynamics”, be it the Gulf of Cadiz that should be concerned, mainly if not only, by continental shelf (stricto sensu) phenomena.

Your second overall comment:
But the paper is also very polemical and this does not have its place in a major journal.
is similar to the one from Referee#2/Part1 and, I think, “unfair”. You will certainly agree with me that we will not convince each others so that **I propose the Editor the following OVERALL SOLUTION**:

The last paper published (in 2017) about the Strait focuses on the Strait itself (this Part2) and, together with the previous published one (in 2015), it **clearly synthesizes the general “timeless” opinion that has scientifically motivated not only this series of papers, but most of the scientific interest I have always had for Gibraltar,** much before my first dedicated publication (Millot et al., 2006). Based on this:

1) Considering **this 2017 paper is an Ocean Science one**, I (propose to) will “plagiarize” (essentially copy-paste) the Introduction Chapter of this paper to get the Introduction Chapter of my Part1/4 dedicated to an overview of the homogeneity vs. heterogeneity question. I hope that you and the Editor will admit that **for Ocean Science in particular, this is an irrefutable and acceptable way to present the question and introduce the other papers without any polemics.**

2) Still with the major aim of avoiding any polemics, I hypothesize that exactly reproducing parts of published papers, without any additional comment, is both fully legal and fully neutral. Therefore, **I (propose to) will then just copy-paste without any additional comment**, at the end of this Introduction Chapter of Part1/4, **only three portions of already published sentences**, what I previously did in this Part 2 (l. 84-92) and in other parts as well:
i) "... in good agreement with the previous study of Millot (2014b)”
ii) “While up to four MWs are spatially distinguishable east of the main sill of Camarinal in the Strait, most of their differentiating characteristics are eroded after flowing over this restrictive topography due to mixing. West of the sill, therefore, speaking of a unique Mediterranean Water seems more appropriate"
iii) “...the severe mixing and dissipation that takes place …downstream … blurs this spatial pattern and tends to form a rather mixed outflow … in which the MWs are barely distinguishable”.
I (propose to) will just link these three portions of sentences without any additional comment, **hence forming a given paragraph** at the end of this Introduction Chapter of Part1/4. **And I (propose to) will just reproduce this given paragraph as the unique paragraph of the Introduction Chapters of Part2/4, Part3/4 and Part4/4**, just specifying that a more complete Introduction is provided in Part1/4**.**

This will fully satisfy the Editor’s justified requirement of having a series of “stand-alone papers”, with the first introducing the whole series, … while avoiding any polemical aspect.

Referring to certain recent papers to support polemics is limited; many older papers do not claim homogeneity of the MW in the straits.
You probably know that I have mainly worked within the Sea and you might have read that I do not consider myself as a specialist of the Strait. I just consider myself as a specialist of the Sea who is

interested in understanding where are going the MWs he has studied and followed all along their course in the Sea.
Therefore, I am sorry but the only work I know that "*do not claim homogeneity*" is by Howe.
Please, could you provide me (anonymously via the OS editorial office if you want) with additional references and, hopefully (I am retired and do not have any access to free libraries), **with the pdf's of such papers**?

I am surprised that the papers by Madelain (1970) or by Zenk (1975) are not mentioned.
I am personally surprised (furthermore I maintain I have been "unlucky") that you did not check the references cited in the Part1 paper as well as in what I (and the Editor) consider as a reference paper for my previous works, i.e. Millot (2014a) … in which you will see that there is even a Madelain (1967) that you should know!
I have had in mind Zenk (1975) but have been unable to retrieve it. In case you have a pdf … please forward it to me.
Whatever the case, you certainly know that Madelain is actually at the origin of the hypothesis of "a homogeneous MO split by bathymetry" that is still supported by the most recent 2015 and 2017 papers previously mentioned.

Furthermore, the paper goes all the other way, which is excessive.
Don't **ALL** (not only the previously cited one) **other papers go their "other" one way??????????**
In case you know papers hypothesizing heterogeneity, please let me know and send pdf's!!!

By declaring that the MW heterogeneity is sufficient to make it form several veins in the Gulf of Cadiz, the author denies the role of further diapycnal mixing on the gulf slope (clearly shown by Price and Baringer 1988 and later on by Cherubin 1997), or by the topographic steering effect of the canyons in the gulf.
I am sorry but I must reject such an assertion:

1) You did not just have a look at the Part1 and Part3 papers! In particular, for what concerns your focus on the Strait exit (Part3), I do not "*declare*", I think I "provide evidence", if not "demonstrate". **Please, just have a look at them and let me know if you still think I "*declare*".**

2) Please, just consider the "heterogeneity/homogeneity" (chose the term you want) evidenced from the cross-MO transect in Part3, in particular Fig.2a/Part3 … that you will find very similar to all the θ-S diagrams shown in the Part2 you reviewed.
Notice that this cross-MO transect was performed clearly upstream from any marked topographic feature.
Then, just have in mind what you "declare" as your first overall comment: "*the* Part2 *paper presents valuable data and evidence of heterogeneity of the MW current in the Strait of Gibraltar."*
With this in mind, please could you finalize a similar sentence: "*the* Part3 *paper presents* ____ *data and* ____ *of heterogeneity of the MW current* at *the Strait* exit, upstream from any marked topographic feature.*"?*
Do you continue thinking I *"declare"*?

3) In Part3, we analyze data (only my co-author participated in the MO-2009 experiment) that were collected:
-only for some of them as general surveys upstream from ~6°36'W,
-for most of them, and the cross-MO transect in particular, in a central zone near 6°20'W … that is markedly upstream from any marked topographic feature.

Please, have a look at Fig.1b and 18 in Part3, or provide me with the bathymetric map you want, and let me know which are the marked topographic features that could be considered as responsible for the "heterogeneity/homogeneity" at 6°20'W, hence upstream from there!

4) I do not "*denies*"! It is not my point to refer to and comment papers dealing with the Iberian continental slope that is much further downstream from the area I an interested in. Note that this slope extends even out of the gulf … so diapycnal mixing of the veins has nothing to do with the Gulf of Cadiz itself: you should deal with an "alongslope dyapicnal mixing". The same remark can be made with the canyons that only have their upper part "in the gulf", at the outer edge of the continental shelf; and canyons are classical features of all continental slopes worldwide, hence having dramatic effects of the alongslope circulation worldwide. But, as far as I know, diapycnal mixing and canyons have never been invoked to explain hydrodynamical processes upstream from ~6°20'W hypothetically leading to the splitting. **Please, could you acknowledge or provide me with adequate references?**

5) I am sorry to say that, with your "*clearly shown*", you "clearly declare"! The authors you cite are "simulers", or "modelers" as you probably use to name them, what I refuse to do: most of the "simulers" I have encountered during more of 50 years tend to think that they are producing "models" that, therefore, have to be retrieved or respected by the whole community, including experimentalists like myself. I will never forget a remark of a colleague of mine (named Nadia) who told me that my twenty-five (1-year, 1-h) current time series (off Algeria) were more or less bullshit compared to the thousands of "data" (as she named her numbers) she got from her "models"!
With my own language, **colleagues working with computers are doing simulations of actual processes and are "simulers" and, as an "experimentalist", I imperatively need to work with them,** just to have my hypotheses checked, and hopefully validated, by numerical computations (simulations) or equations. Would you have had a look at Part1, you would have noticed that I am asking for convenient and dedicated simulations!
Now, with such references, you might be a simuler. But, as a Strait specialist, you might also be aware of dedicated in situ experiments that would have addressed the "homogeneity vs. heterogeneity question". **Please, could you let me know which in situ experiment has already been dedicated to this specific question?**

6) As an overall answer to your comment that I reject, let me specify that neither simulations nor data analyses, as much sophisticated they could be, can be considered as definitive. Yes, I do think that "*the* MO *heterogeneity* indicated by the data sets I am showing *is sufficient to make it form several veins"*, which does not mean other effects such as topographic ones at the Strait exit or alongslope diapycnal mixing do not influence the splitting (downstream from where it initially occurred) and the final characteristics of the veins in the Ocean. **As clearly indicated by my title, it just seems to me that "there is definitive evidence of the MO heterogeneity from the Strait entrance to the Strait itself (there supported by your "*the paper presents* *valuable data and evidence of heterogeneity*") and to the Strait exit"**.

I strongly believe that all polemical aspects of this paper must go before it is published (part of them in the abstract, part of them in the introduction, part of them in the conclusion).
Even though you, as well as Referee #2 of Part1 and the Editor (influenced or not by both of you) see polemics in my writing where I just see my willingness to expose, in a way as clear as possible, the "homogeneous vs. heterogeneous controversy", let me specify that I will markedly modify my writing and, **just because I would first of all like to publish in Ocean Science, I will strictly respect the final Editor's recommendations**.

Even though I obviously have the same "*polemical writing*" all along my papers, I just checked, as examples of discussion between us, what could be the concerned sentences in this Part2 abstract, hence focusing on the splitting as you previously emphasized. I identified four sentences:

1) "We also demonstrate that the density range within the MO in the western side of the Strait (6°05'W) is at least 0.5 kg.m^{-3}, which is the density range, in the vicinity of the Cape St Vincent (8°30'W), of the four veins formed by the MO splitting." Is this an observation worth to be specified? If yes, do you think it is correctly written and, if not, how would you write it? Whatever the case, **is this an observation possibly supporting a "major effect" of the heterogeneity on the splitting**, hence a "minor effect" of both the alongslope diapycnal mixing and the topographic effects?

2) "We show that the lightest component of the MO has started to be split as soon as Camarinal sills and sink all along the Strait."Is this an observation worth to be specified? If yes, do you think it is correctly written and, if not, how would you write it? Whatever the case, **does this observation made markedly upstream from the Strait exit support a "major effect" of the heterogeneity on the final splitting in the Ocean**, hence support a "minor effect" of both the alongslope diapycnal mixing and the topographic effects that occur markedly downstream from the Strait exit?

3) "The splitting of the MO into veins is thus mainly due to its intrinsic heterogeneity, which is a direct consequence of the Sea functioning and of the mixing, within the Strait itself, of the MO with this or that type of Atlantic Waters (AWs)."
First, considering only the second part of the sentence (from "its intrinsic heterogeneity ..."), do you agree that "intrinsic heterogeneity" means (is understood as) "within the Strait itself", hence is clearly the focus of this Part2 paper? If you answer "yes", and even though you did not (at least carefully) read Part1, **do you agree the heterogeneity in the Strait itself that is evident to you (your first overall comment) only results from the Sea functioning and AWs-MWs mixing processes**?
Second, **do you think that the first part of the sentence "The splitting of the MO into veins is (thus) mainly due to its intrinsic heterogeneity" is not justified by the two previous sentences**?
And **don't you think that the "mainly" is sufficient to let some place for the other processes** (the alongslope diapycnal mixing and the topographic effects); **if not, what could be for you an acceptable writing?**

4) "Therefore, the bathymetry in the Strait, and even in the Strait exit surroundings (near 6°20'W), has no major effect on the MO characteristics in the whole Ocean." In case you consider such a writing is polemical, **what could be for you an acceptable writing?**

Secondly, the paper offers little dynamical interpretation of the data.
I consider "*little*" is a bit more than "no". **So, please, could you specify what are the "*little*" dynamical interpretation of the data I provide in this Part2 paper**?
Whatever the case, and even though you never deeply read any of my other papers (be they already published or still submitted as parts of this series), I am sorry to say that I consider myself as an "honest and objective scientist". I never published "bla-bla/interpretation" papers. The "dynamical interpretations" I have offered in the past were i) based on reliable data sets and sound analyses, and ii) offered as schematic diagrams. In the past, I have published, in particular, schematic diagrams for the circulation of the surface, intermediate and deep waters, first in the western basin of the Sea, then in the whole Sea; and I have also published schematic diagrams for the structure of mesoscale eddies in the Algerian sub-basin (references available upon request). And finally, **for what concerns this specific series of papers, I have published schematic diagrams / dynamical interpretation for the whole MO from the Strait entrance downstream to the Iberian continental slope in Millot (2014a), clearly mentioned in Part1 and reproduced as Fig.19-Part3, as well as schematic diagrams / dynamical interpretation of the AWs-MWs mixing as**

Fig.1-Part1 and Fig.1c-Part2. You might be right but, sorry, I am unable to provide more dynamical interpretation than that!!!

Assessing the role of bottom friction on the MW in the strait,
I deal with bottom friction of MW**s**,

of the entrainment of AW at the top of the MW layers,
I do **not** deal with **entrainment** of AWs at the top of the MWs **veins: I deal with AWs-MWs mixing** (as inferred from CTD profiles, i.e. **without any dynamical information**, i.e. without any information about which of the two layers pulls along the other; is it an entrainment of AWs at the top of the MWs or an entrainment of the MWs at the base of the AWs, I do not have adequate data allowing me to specify???), and I do **not** deal with "***MW layers***" since I think/demonstrate/claim/declare (whatever the term you prefer) that the series of **MWs that are superimposed (or layered, i.e. on the vertical) at the Strait entrance (Part1) are juxtaposed side by side (i.e. on the horizontal, from the left-hand side of the MO to its right-hand side, i.e. from south to north) as veins in the Part2 that you reviewed, linked to a not-yet considered Coriolis effect that would be increased in the Camarinal sills surroundings (due to a necessary increase in velocity)**.

calculating orders of magnitudes of the diapycnic mixing rate in the straits, characterizing the mixing due to the internal waves, comparing the time for mixing with the time for advection (in a simple calculation I did, about 20 times longer) would give more support to the author's claim.
Sorry but I am unable:
- to calculate orders of magnitude(s) of the dapycnic (or diapycnal?) mixing rates in the strait(s),
- to characterize the mixing due to internal waves,
- to compare the times for mixing and advection,
- to control such even simple calculations.

I am only able to show data, evidence significant features … and motivate dedicated simulations … or additional data analyses. This is clearly specified at the ends of the Part1 (l. 1076-1095), **Part2 (l. 960-969)** and Part3 (l.1770-1790) papers.
Therefore, why don't you take the opportunity of this series of papers to submit, together with my tetralogy (hopefully near mid April 2018), your simple calculations that would give either more support to what I am "claiming" or make you joining the group of the homogeneity + topographic effect partisans???

This being said, **please could you let me know which kind of process could, in your mind, lead to the kind of profiles shown at 5°50'W and 6°05'W, that are essentially relatively straight mixing lines displaying relatively homogeneous waters in their lowest part?**

Thirdly, the paper contains many words expressing uncertainty "it is clear that, must be assumed, probably, might be, resembles, hypothesized..."
And so what? Would you prefer assertions? I am sorry to say I think that "**definitive peremptory sentences**" can only be used by simulers who fix themselves their own hypotheses and framework. It is then very easy to say that, considering this and this and this, then one can for sure guarantee that and that and that! **As an experimentalist trying to analyze data, I can never be definitive: I can just say that there is "definitive evidence" for this or that feature and express hypotheses for the reasons leading to such a feature!**

or excess "tremendously, tremendous, dramatic consequences..." which are not quantitative and provide little information.

When dealing with Sea in situ data in particular, “quantitative and accurate but unverifiable information can be considered as big, but it can be totally false”. **Such words expressing excess for you, and maybe for simulers in general, just express the exaltation a Sea experimentalist can have after having evidenced features she/he never expected, furthermore when these features support the hypotheses she/he previously made!**

Please, be sure that I do not want to compare myself with so “tremendously big scientists”, but don’t you think that Archimedes, when he discovered buoyancy, or Newton when he discovered gravity or Galileo when he discovered the Earth’s rotation had reasons for being “excited” … hence for probably using what was considered as excessive words by the “reluctant” persons? Sorry but this is a writing you have to use to fight against skepticism. Let me confess that, when I was fighting wit colleagues to convince them that the IWs from the eastern basin were flowing along the European continental slope in the western basin and did not cross the Algerian sub-basin directly towards Gibraltar as generally believed at these times, I was thinking “And yet they turn!”...

They also must go.
I understand you ask me to remove such qualifiers. **Please, am I allowed to differentiate a major consequence from a minor one, or should I only deal with consequences without any qualifier?**

typos and corrections abstract : and sinkS along the strait
You can be right but I want to be sure: I want to say that “this component **has started to be split and has started to sink**”**. What would be a correct (and “elegant”) writing?**

introduction : is a DYNAMICAL AND THERMODYNAMICAL machine which...
I am not sure I clearly see the necessity for having used capital letters for what is, I think, only a suggestion from you. **In any case, I disagree**:

1) Any “machine”, since you agree with me that the Sea is a “machine”, needs “energy” to function and produce what it “has been built for”. Energy is electricity for a coffee machine, coal for a steam locomotive or gasoline for a car. Energy for the Sea is, **ONLY** (at least from my point of view), the water balance between Evaporation (of the Sea) for the output and both Precipitation (over the Sea) and River runoff (from the land) for the input (leading to the famous **E-P-R budget**). **Therefore, and at least for me, the Sea is “just” an hydric machine!** Note that, following the Cambridge Dictionary, I avoid using “hydraulic” which means “operated by or involving pressure”. **And, at least for me, the major consequence of the water balance is just the difference between the Sea-level and the Ocean-level, leading the Ocean to cascade (sic) into the Sea.**

2) More specifically: temperature in the Sea does not have any major direct effect, even though a climate much warmer over the Sea would lead, via thermal expansion, to reduce the level difference, hence the intensity of the cascading; the same could be said for atmospheric pressure since, for instance, a mean pressure much larger over the Sea would increase the level difference; dynamical constrains, such as for instance wind stress, can obviously modify the inflow from the Atlantic but only at relatively small time scales.

3) In addition to the water balance, and **to understand most of what is occurring at Gibraltar** … as well as most of the circulation within the Sea (with some knowledge about the thermal+hydric meteorological forcing), it is then **ONLY** necessary to consider the **Coriolis effect**. Indeed, it is only this effect that makes the IWs (intermediate MWs) outflowing (sic) on the right-hand side of the MO (on the European side of the Strait) and the DWs (the deep MWs) overflowing (sic) on the left-hand side of the MO (on the African side of the Strait).

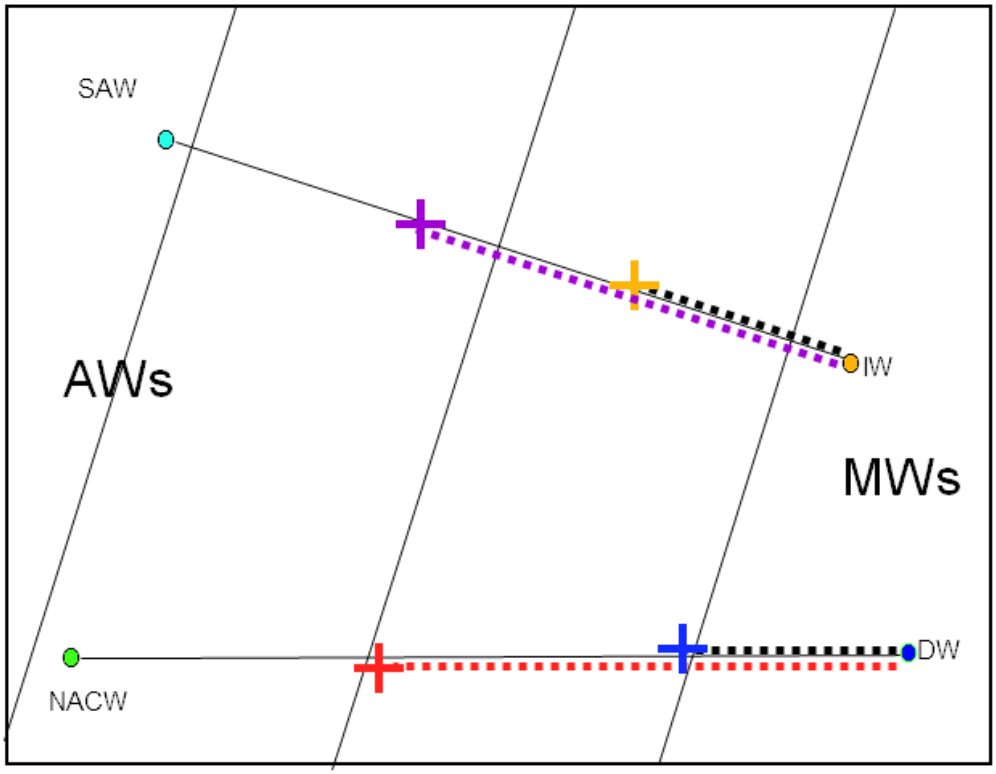
To conclude about the machine, **I can just accept adding "hydric"**, which will somehow synthesize all what is said in the remainder of this first sentence of my Introduction.

figures 1a and 1b I cannot see the correspondence in geographical locations for the same colors
I am sorry but **there is no correspondence** in geographical locations for the same colors. As clearly indicated in both the captions and the figures:
- Fig.1b **displays** CTD profiles at i) 5°43'W (light blue), ii) 5°50'W (dark blue) and 6°05'W (green)
- Fig.1a **locates** CTD profiles and time series at i) 5°43'W and 6°05'W (yellow, as yo-yo time series), ii) 5°50'W and 6°05'W (dark blue, as transects) and iii) 5°45'W and 6°05'W (green and light blue, as yo-yo time series)

Note that:
- Fig.1a aims at showing **strategies** (CTD profiles distributed along transects in dark blue, CTD profiles yo-yoed at specific locations in yellow, and CTD time series near the bottom at specific locations in green/Camarinal longitude and light blue/Espartel longitude)
- Fig.1b aims at evidencing the CTD profiles **variability** over both time (light blue vs. dark blue) and space (blue vs. green).

lines 270-275 : the discrepancies seem dismissed here
If I understand well your succinct comment, **I am not dismissing anything** and just say that "one general feature is not retrieved on a single transect": for instance, and on the basis of any sports records, men are stronger and/or more rapid than women; but during a single competition, in some sports at least, a female champion can beat a male champion!

Let me know explain **my point about the AWs-MWs mixing in general**, and more especially when differentiating the lightest and densest components of these two types of waters and, please consider the drawing I made **especially for you** (I hope you will understand I did not polish my writing as I use to do for my papers):



- for the MWs, the lightest (the IWs, one in orange) are always (sic) in the north, so that the densest (the DWs, one in blue) are always in the south; this is a direct consequence of the Coriolis effect that has a “tremendous” (sorry for this “excess”!) importance for the circulation in both the Sea and the Strait.

- for the AWs, the lightest (SAW in cyan) is generally (sic) found over the MWs in the north because the densest (NACW in green) is generally (sic) found over the MWs in the south; this is a direct consequence of the fact that the SAW is a surface water that is found everywhere in the west of the Strait and, probably (sorry for this “uncertainty”), circulates sluggishly before entering in the Strait area, while NACW circulates markedly (it is always found alongslope in the west of the Strait, hence necessarily -this is not an “excess”- constrained by the Coriolis effect, which allows evidencing its circulation).

- Therefore, in general (sic), SAW mixes more with the IWs (in the north) and NACW mixes more with the DWs (in the south).

Now, let us consider the mixing rates (the dashed lines in the figure) and make some hypotheses:

1) In the most simple case, which appears not to be the most general one, let us hypothesize roughly similar mixing rates (dashed black lines). In such a simple case, the maximum density (the σ_{max} in our text) of the MO will thus be a function of latitude with maximum values (i.e. the blue cross) in the south and minimum values (i.e. the orange cross) in the north. Note that, in such a case, the density difference between the two MWs is not markedly modified by the mixing, or the density range for the MWs is not modified along the Strait … which is not what is generally observed (the density range increases downstream)

.

2) In the most general case, the mixing between the lightest of the MWs with SAW is more intense (the violet cross) than the mixing of the densest of the MWs with NACW (the blue cross); this is the most general case because the densest of the MWs always tends to sink more than the others, hence becoming deeper and deeper and leading to a reduced mixing with NACW while the lightest of the MWs remains the closest to the AWs, SAW in particular. As in case 1), the maximum density of the MO will still be a function of latitude with maximum values (i.e. the blue cross) in the south and minimum values (i.e. the violet cross) in the north, but the density differences between the two MWs will be increased by the mixing, which is what is generally observed.

3) In the most abnormal case, the mixing rate in the south is much more important (leading to the red dashed line and cross) than the mixing in the north (the black dashed line and the orange cross), thus leading the MO densities in the south (the red cross) to be lower than in the north (the orange cross). This is the most abnormal case just because, as exposed in 2), there are no normal reasons for that; but, as demonstrated by the example shown in our text, this can "probably" occur under specific circumstances!

Do you understand, and hopefully accept, my point of view?

all acronyms and variable names : q(E), S(E), MLS(E,C), S(C), sigma_q, S_q... must be defined.
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.

Now, **I hope you have understood my computations** and have eventually had a look at the results presented in Millot (2014a).

In view of these remarks, this paper is inappropriate for publication in its present state and must be sufficiently revised to suppress polemics and to provide more scientific arguments connecting the various observations and quantifying the physics involved in this process.
When just considering your comments above, and obviously keeping in mind your first overall comment *"the paper presents valuable data and evidence of heterogeneity of the MW current in the Strait of Gibraltar"*, **I think it should be normal that the Editor ask for "major revisions", what I have already planned to do** … at least with the aim to "*suppress polemics"*. However, as I have tried to explain above, and apart from improvements in the writing, **I think I will be unable (or not willing)** *to provide more scientific arguments connecting the various observations and quantifying the physics involved in this process.*

To conclude, some points recapped **for both you and the Editor**:
- I have not been lucky in having three different Referees #2: none of them has had an overview of my whole work.
- I have not been lucky for this Part 2 because you are probably more a theoretician than an experimentalist. Whatever the case, I am sorry to say that you did not comment very much on the data themselves and on the specific analyses I make.
- And you did not answer a major question that I would like to reformulate on the basis of the three sentences published in the 2015 and 2017 papers as i), ii) and iii) of 2) in page 2:

* the sentence i) just indicates that, roughly, everybody agrees on the heterogeneity at the Strait entrance I address in my Part1.
* sentences ii) and iii) essentially claim for homogeneity within the Strait itself (typically near 6°05’W) that I address in this Part2.

Therefore, my question: **When considering the data sets and analyses presented in this Part2, would you characterize the MO at 6°05’W as homogeneous or heterogeneous and, in case of disagreement with these 2015 and 2017 papers, how would you formulate it?**

I hope you will accept answering my questions, discussing my own comments and helping me in providing me with the information you would consider as worth to be specified, and I thank you in advance for your help.