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Ocean Science Discussions



# *Interactive comment on* "About the seasonal and fortnightly variabilities of the Mediterranean outflow" *by* C. Millot and J. Garcia-Lafuente

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Dear Vincenzo,

Let me (Claude) first specify that, when submitting, we proposed you as a competent reviewer for our manuscript. Even though we do not (have to) know whether the Editor contacted you in early December when we submitted or urgently in late February after the end of the two-month interactive discussion, we thank you very much for the time you spent to help us improving our work with many suggestions.

Before discussing your suggestions in detail, let me make explicit one basic feature you are aware of since we recently exchanged ideas while writing some kind of a review about the Gibraltar functioning. You know, and we specify this in our Introduction, that

I have recently put forward a series of hypotheses that are not fully accepted by our community, in particular by Jesus. This is why we call your attention about the use of either “I/me/CM” or “we/us” in the following. Whatever the case, we continue collaborating and discussing in order to improve our understanding of the processes. We thus took care not presenting ideas that are rejected by one or the other, or at least we clearly specified adequate references. Because I made all computations and figures, emphasis is put on the presentation of my own ideas (the “CM-hypotheses/concept” hereafter) that are original, so that the reader is expected to be aware of the specified references.

Let us also emphasize that our aim is to deal “only” with three time series that we analyze in a similar way and from which we infer results, whatever the hypotheses/concepts about the actual Gibraltar functioning and outflow composition are. This clearly means that the results we get can be “self-explaining” and that they must be accepted by any “fan/partisan/supporter” of this or that hypothesis/concept. Therefore, and practically, we could easily present the original ideas/hypotheses/concepts previously mentioned either in the Introduction (as actually done) or in an extended Discussion before having a Conclusion focusing more on this time series analysis itself.

Finally, you might have read the review #1 received one month ago so that you are conscious that the two reviews we have in hand are clearly non-redundant. Therefore, we cannot make by ourselves large modifications of our text in order to satisfy one reviewer’s comments, hence taking the risk of markedly disagreeing with the other reviewer’s comments. Whatever the case, we will fully follow the Editor’s requests and advisements, all the more since he already spent some time on our paper to help us improving the English language.

Now, for what concerns your suggestions, thanks for having emphasized the fact that we “tackle an important and hot scientific issue regarding . . . Gibraltar”, for having underlined that it is “relevant for ocean climate variability studies”, and finally for having “recommended this paper to be published”. However, let me comment some of your remarks and suggestions (classified as pages/paragraph numbers).

C783 / #2: "Because the phenomena analysis described in the paper is very complex . . . it is very difficult to read the paper in the present version . . . following consideration". I suppose that your remark concerns more the "CM-hypotheses/concept" than the "phenomena analysis herein". If this is the case, we will follow the Editor's advices as previously said. Whatever the case, let me briefly summarize the CM-concept that only involves the Coriolis effect and the topography. The MWs circulating in the Alboran sub-basin (qualified as Intermediate / IWs) are superimposed along the Spanish slope while the sluggish ones (qualified as Deep / DWs) are pushed off Morocco, hence staying just below AW there. Because the available section is reducing in the strait, the IWs accelerate, their interface with the DWs tilts up southwards allowing the DWs to reach the sill, and both the IWs and DWs outflow while mixing individually with AW. Because the available section is widening after the strait, the IWs decelerate and the IWs-DWs interface tilts down. Then both the IWs and the DWs cascade independently along the Iberian slope (eventually accelerating again, at least temporarily), hence inducing a vertical splitting of the outflow. Secondary interfaces between the IWs and between the DWs just lead to secondary splittings. Such hypotheses are not so complex and should be easily tested with a very simple numerical simulation . . . so that I am expecting more collaboration with you! However, note that Jesus does not share at all this hypothesis about the origin of the different veins in the Atlantic.

C783/#3: ". . . the strait of Gibraltar is the first obstacle . . . the first site where the MWs undergo a strong mixing with AW. . .". I do not fully agree with the idea that Gibraltar is such a "first obstacle/place" for the MWs-AW mixing and think it is only a "major obstacle/place". Let me specify that, among the four MWs that can be identified all away through the strait (please, have a look at the 1985-1986 GIBEX data as plotted in Millot, 2009), two originate from the eastern basin (LIW and TDW/EMDW) while two (WIW and WMDW) originate from the western one. Even though the channel of Sicily is much wider than the strait of Gibraltar, the Coriolis effect is essential there



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(as well as anywhere in the sea) since LIW is constrained along the Sicilian slope while EMDW spreads along the Tunisian one (before cascading into the Tyrrhenian and forming TDW). These ideas have been published for the first time in Millot (1987) but you can also look at Millot (1999) and Millot and Taupier-Letage (2005). Therefore, each of these two components has been in contact with AW not only in the channel but also in the whole eastern basin. WIW is in direct contact with AW as soon as the Liguro-Provençal sub-basin where it is formed while you will certainly be convinced by the GIBEX data as displayed in Millot (2009) that the upper part of WMDW is strongly mixed with AW much before approaching the strait (see the 4°30'W section). What is emphasized in Millot (2009) in particular, but not in this paper, is that all four MWs encounter relatively intense mixing with AW due to the internal tide in the strait. Because I think that the heterogeneity of the outflow east of the strait is maintained west of it, I do not see the strait as an obstacle that would lead the outflow, as previously thought, to become fully homogenized.

C783/#3: "The main result of the paper . . . the authors investigate in details where the mixing take place and what type of water take part on these events". Sorry but I still do not fully agree. We do not investigate in details the spatial variability of the mixing, which would require data collected all over the strait. We just focus on the two time series we collected at the Camarinal and Espartel sills which are two specific locations that are roughly located along the same streamline. I personally think this is obtained partially by chance since, assuming two sills located at markedly different latitudes, they would obviously not be concerned by the same streamline. The analysis we make about the seasonal variability of the outflow is based on the temporal evolution of the mixing line between two consecutive data on a T-S diagram. This analysis does not focus on which of the four MWs is concerned: we only deal with the slope of such a mixing line, not on the T-S values this line is associated with. The acronyms of the four MWs are indicated for convenience. However, yes we differentiate which type of AW (SAW vs. NACW) is involved in the mixing according to the season. Note that, at least according to me (Millot, 2008), the importance of the AW type dramatically conditions the outflow characteristics.

C783/#3: "... the bifurcation of MWs between Cs and Ms". Assuming that these four MWs are schematically juxtaposed side by side (from WIW in the north to WMDW in the south) and that Cs and Ms are more or less on the same section of the strait, we do not actually consider any "bifurcation". We just consider that, at any given time, the MW outflowing at Cs is not the same that the MW outflowing at Ms. More precisely, and with a relatively large outflow and relatively low tidal mixing between AW and the MWs, the MW outflowing at Ms (80 m) would be denser than the MW outflowing at Cs (270 m) at exactly the same time, as already encountered on some occasions (see fig.22b of Millot, 2009).

C783/#3: "... in situ observations (the validation of the numerical model)". More precisely, I do not think that observations have to validate numerical simulation. For a "sea-going oceanographer not dealing personally with simulations" as me, reliable data are representative of actual phenomena. Up to now in oceanography, such oceanographers can only try putting forward hypotheses able to explain their data sets while obviously agreeing with some basic principles. To be validated, these hypotheses need equations, hence numerical simulations. So that, practically and for me, the correct order in oceanography is: data → hypotheses → simulations. The order is reversed in theoretical physics for instance (data are needed to validate equations), and I just can hope that this will occur one day in oceanography. But for the time being, simulations in oceanography can only validate hypotheses: they have to represent data at best.

C783/#3: "... the mixing in Cs is more pronounced because the hydraulic control ...". You might be right and I have no mean to validate your hypothesis. I just emphasize the fact that Cs is at 270 m while Es is at 360 m and wonder what the importance of such a difference in depth is. Also, we show that AW-MWs mixing at Cs can be insignificant during neap tides while it is significant during the same time at Es. Whatever, I do not want to address processes that we cannot validate with the data we have so that I prefer not to refer to Sannino et al. (2004).

C784/#1: "… when the MW pass over Cs has lost its original characteristic, taking now the properties of the source water of the MW that will be later observed in the North Atlantic". Let me make our results more explicit. The MWs can remain unmixed just after having passed Cs (during neaps). However, they will be mixed when arriving at Es. And I think it is only more to the west (see the GIBEX data in Millot, 2009) that tidal mixing between AW (SAW and/or NACW) and each of the MWs, in particular the one outflowing at Cs/Es, will be markedly reduced. Then, while cascading along the Iberian slope, mixing of each of the four veins of MWs with surrounding waters will continue, even if no more essentially due to the internal tide.

C784/#1: "The authors conclude that the outflow within the strait display a spatial heterogeneity …". This is an obvious conclusion from the analysis in Millot (2009) of a data set such as the GIBEX one, NOT a conclusion of this paper. In case the MWs at Cs and Es are the same (as during the examples shown for winter, summer and typical spring in fig. 4b-d), one can hypothesize either that the outflow is spatially homogeneous (Cs and Es being or not on the same streamline) or that the outflow is spatially heterogeneous with both points on the same streamline. The only evidence provided in the paper about the spatial heterogeneity comes from the "atypical neap period" (Fig. 4f) since the MW found at Cs is not the one found at Es. The spatial heterogeneity will be addressed in a paper to come.

C784/#1: "… it's well known … the main core of MW divides into two major cores …". Sorry but I (not Jesus) disagree with you, as briefly indicated at the beginning of this answer and in the Introduction of our paper. I think that each of the four MWs will lead, after cascading, to a specific vein at depths ranging from 300 to 1400 m (to deal with your values).

C784/#1: "Is there a relationship between spatial heterogeneity and bifurcation?" When dealing with "bifurcation", I mainly think about horizontal splitting so that I am not sure I fully understand your question. I hypothesize that each of the MWs which outflows in a noticeable amount, thus leading to the spatial heterogeneity of the out-



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flow, then cascades individually, thus leading to a vertical splitting of the outflow in the form of a series of corresponding veins.

C784/#1 "... Siedler (1968) put forward the hypothesis ... Zenk (1975) shows that ...". We agree with these authors on the fact that tidal mixing within the strait is of major importance. However, none of them realized that the outflow within the strait maintains, and eventually increases, the heterogeneity it had before crossing the strait, which is clearly the CM-hypothesis only. Therefore, all these hypotheses have to be given at least the same confidence and they all must be validated by numerical simulations. Because this is not the topic of our paper, we propose not to mention these references.

C784/#1: "... I'm very curious ... interannual variability ... lenses of MW ... number of meddies ... Fusco et al. (2008) ... salt anomaly ...". We are sorry but these are questions we cannot address with our data set.

C784/#2: "line... introduction, again not clear what are the major/new results of the paper". A major/new result of the paper is that, whatever the composition and heterogeneity of the outflow in terms of MWs is, a seasonal variability of its characteristics when in the ocean is induced by the seasonal variability of its mixing with AW (SAW vs. NACW) within the strait. Another important result of the paper is that the outflow is generally unmixed with AW at Cs during neaps while it is always mixed at Es. Furthermore the outflow entering the strait displays marked spatial heterogeneity and long-term temporal variabilities (Millot, 2009) while the inflow displays marked spatio-temporal (Millot, 2008) and both long-term and seasonal (Millot, 2007) variabilities, I do think that the overall results for climate variability studies is that predicting the outflow characteristics when in the ocean appears almost impossible. I must say that this last assertion is only partially accepted by Jesus.

C784/#2: "... the word "concept"...vs. "result" ...". For us, the word "result" must apply to something shown/demonstrated and accepted by the whole community. Otherwise, what you would like to call "result" must be called "hypothesis/concept". I do not believe



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in the previous description of the processes since such a description is based on a series of hypotheses clearly not supported by the available data sets (i.e. the GIBEX one). We thus prefer comparing “one concept” (that has prevailed up to recently) with “another concept” (the CM one).

C784/#2: “line 44-45 the sentence is not clear”. We are sorry but we are not sure which sentence you are referring to. We suppose it concerns comments about what we just called “another concept”. We are ready to improve the writing of any sentence, so that please do not hesitate to contact me directly.

C784/#2: “line 53-54 appropriate reference about the two branches ...”. We are sorry but we do not address the issue of the number of branches the outflow splits in. My “concept” is that, as previously said, if the outflow entering the strait is composed of four MWs of significant importance, then its splitting when cascading will result in four veins.

C785/#1: “... the concept atypical is important...” We do not think this can be qualified as a “concept”. What we qualified as “atypical” is something that has to deal only with the specific location of our Cs and Es time series. We assume (and this will be demonstrated in a paper to come) that the outflow is heterogeneous and that both Cs and Es are in general (i.e. “typically”) along the same streamline. However, we have found a period during which this was not the case (outflow heterogeneous and Cs and Es not along the same streamline) and we have qualified this period as “atypical”. Maybe it is a matter of English language and “usual/unusual” should be more appropriate?

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Interactive comment on Ocean Sci. Discuss., 7, 2043, 2010.